United States
Department of
Agriculture

Economic
Research
Service

Food Assistance
and Nutrition
Research Report
Number 19-3

USDA

Effects of Food Assistance and Nutrition Programs on Nutrition and Health

Volume 3, Literature Review

Edited by
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Food Assistance & Nutrition
Research Program

Effects of Food Assistance and Nutrition Programs on Nutrition and Health: Volume 3, Literature Review. Edited by Mary Kay Fox and William Hamilton, Abt Associates Inc., and Biing-Hwan Lin, Food and Rural Economics Division, Economic Research Service, U.S. Department of Agriculture. Food Assistance and Nutrition Research Report No. 19-3.

Abstract

This report provides a comprehensive review and synthesis of published research on the impact of USDA's domestic food and nutrition assistance programs on participants' nutrition and health outcomes. The outcome measures reviewed include food expenditures, household nutrient availability, dietary intake, other measures of nutrition status, food security, birth outcomes, breastfeeding behaviors, immunization rates, use and cost of health care services, and selected nonhealth outcomes, such as academic achievement and school performance (children) and social isolation (elderly). The report is one of four volumes produced by a larger study that includes Volume 1, Research Design; Volume 2, Data Sources; Volume 3, Literature Review; and Volume 4, Executive Summary of the Literature Review. The review examines the research on 15 USDA food assistance programs but tends to focus on the largest ones for which more research is available: food stamps, school feeding programs, and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). Over half of USDA's budget—$41.6 billion in fiscal year 2003—was devoted to food assistance and nutrition programs that provide low-income families and children with access to a healthy diet.

Keywords: Dietary intake, food expenditures, nutrient availability, nutrient intake, nutritional status, nutrition and health outcomes, USDA's food assistance and nutrition programs

Washington, DC 20036 October 2004

Acknowledgments

Many individuals deserve recognition for their roles in making this report a reality. First and foremost are the chapter authors. Without their tireless efforts, this report would not exist. Authors include current and former Abt Associates staff—Joy Behrens, Nancy Burstein, David Connell, Mary Kay Crepinsek, Mary Kay Fox, Frederic Glantz, Cristofer Price, and William Hamilton—as well as consultants—Virginia Casey, John Cook, Peter H. Rossi, and Joanne Tighe.

We also owe a debt of gratitude to a cadre of reviewers who offered thoughtful review and comment on one or more chapters of the report in one or more drafts. Their contributions improved the report. Reviewers included a team of senior advisors (Johanna Dwyer, Peter H. Rossi, and Rick Trowbridge), a group of external technical experts (Janet Currie, Barbara Deveney, Shiriki Kumanyika, Suzanne Murphy, Barry Popkin, and Mike Puma), other Abt staff members (Nancy Cole, Joan McLaughlin), and staff at USDA's Economic Research Service (Jane Reed, Betsy Frazao, Linda Ghelfi, Craig Gundersen, Joanne Guthrie, Bill Levedahl, Vic Oliveira, Mark Prell, David Smallwood, Laura Tiehen, Jay Variyam, and Parke Wilde), Food and Nutrition Service (Lisa Ramirez-Branum, Steven Carlson, Edward Herzog, Jay Hirshman, Patricia McKinney, Anita Singh, and Tracy von Ins), and Center for Nutrition Policy and Promotion (Peter Basiotis and Andrea Carlson). We offer special thanks to Johanna Dwyer and Betsy Frazao who took on reviewing tasks that were larger than most and who offered extensive and useful feedback.

Sharon Christenson and Daniel Singer deserve special recognition for coordinating the literature search as well as the document retrieval process. And, finally, several people at Abt Associates and the Economic Research Service (ERS) deserve our gratitude for managing production and editing of the report. Eileen MacEnaney and Eileen Fahey coordinated production of the report at Abt Associates. Linda Hatcher, Courtney Knauth, Sharon Lee, Tom McDonald, Victor Phillips, Mary Reardon, Dale Simms, Priscilla Smith, and John Weber completed the cover design and final editing and production at ERS.

We sincerely appreciate the efforts of all these colleagues.

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Chapter 1

Introduction

Since the mid-1940s, when concerns about the nutritional status of young men drafted for service in World War II led to establishment of the National School Lunch Program (NSLP), the U.S. Government has committed to ensuring that its citizens neither go hungry nor suffer the consequences of inadequate dietary intake.[1,2] Over the years, many Federal programs have been deployed to meet this commitment. Today, the Federal nutrition safety net includes 16 distinct food assistance and nutrition programs (FANPs) (table 1). Administered by the Food and Nutrition Service (FNS), U.S. Department of Agriculture (USDA), the 16 programs together were funded at approximately $38 billion in fiscal year (FY) 2002.[3] An estimated one in five Americans participated in one or more FANPs at some point during FY 2002 (Oliveira, 2003).

Although FANPs vary greatly in size, target population, and benefit-delivery strategy, all provide children or low-income households with food, the means to purchase food, and/or nutrition education. Several programs also provide avenues for disbursement of surplus agricultural commodities. All FANPs share the main goal of ensuring the health of vulnerable Americans by providing access to a nutritionally adequate diet.

In recent years, the efficacy of the web of programs that make up the nutrition safety net has been questioned. In 1996, during the throes of welfare reform, Congress seriously considered abolishing key components of the current Federal system in favor of block grants to States. While this initiative was ultimately defeated, welfare reform—specifically the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA)—resulted in significant changes to several FANPs. Most of these changes tightened eligibility standards and/or reduced benefit levels.

The continued pressures of welfare reform, and the increased accountability encompassed in the Government Performance and Results Act (GPRA), are certain to lead to heightened scrutiny of all Federal assistance programs. In the past, much of the assessment of FANPs centered on issues related to program operations, such as whether only eligible participants received benefits. Future program reviews are likely to be more broadly based, to focus on program effectiveness, and to ask if the program is achieving its objectives.

Recent program policies have emphasized the nutrition focus of the FANPs, which separates them from other federally sponsored income support programs. Indeed, in FY 1998, FNS made a "renewed commitment to nutrition education in all FNS programs" and established a special staff within the agency to "refocus efforts toward nutrition and nutrition education" (USDA/FNS, 2003). The growing emphasis on nutrition education in the Food Stamp Program (FSP) is one example of this renewed commitment. In FY 1992, only five States had approved State plans for FSP nutrition education, and the Federal share of expenditures for FSP nutrition education was $661,000. In FY 2002, 48 State agencies had approved FSP nutrition education plans and Federal expenditures for FSP nutrition education exceeded $174 million (USDA/FNS, 2003). Most of this increase occurred after 1998 (Speshock, 1999).

A further example of the renewed focus on nutrition in the FANPs is the set of goals and core objectives defined in the FNS strategic plan for 2000-05 (USDA/FNS, 2000). One of two key goals is "improved nutrition for children and low-income people." Core objectives under this goal include improving food security, promoting healthy food choices among FANP participants, and improving the quality of meals, food packages, commodities, and other program benefits.

In recognition of both the renewed emphasis on nutrition and nutrition education in the FANPs and the increasing Federal focus on program accountability,

[1]Many World War II draftees who were rejected had nutrition-related problems, including stunted growth, missing or rotted teeth, and physical deformities associated with rickets or other severe nutritional deficiencies during infancy and childhood.

[2]The earliest version of a federally operated food assistance and nutrition programs was actually the New Deal food stamp program (operated in the 1930s). This program allowed poor households to purchase stamps that were redeemable for most foods. Households also received a supply of free bonus stamps that were redeemable for selected surplus commodities. The New Deal food stamp program was discontinued during World War II.

[3]The list of FANPs used in this report differs slightly from the list used by FNS. FNS considers the Nutrition Education and Training Program and Team Nutrition to be part of the National School Lunch and School Breakfast Programs. FNS also operates the Disaster Relief Program, a program that is not considered in this review because its role in the nutrition safety net is substantively different from that of the other FANPs.

Table 1—Federal food assistance and nutrition programs

Program	Year begun[1]	FY 2002 costs[2]	FY 2002 participation[2]
		$ millions	
National School Lunch Program (NSLP)	1946[3]	6,857[4]	28,006,873 lunches per day
Special Milk Program (SMP)	1955	16	112,781,614 total half-pints
Commodity Supplemental Food Program (CSFP)	1968	110	427,444 participants per month
Summer Food Service Program (SFSP)	1968	263	121,865,417 total meals and snacks
Food Stamp Program (FSP)	1974	20,677	19,099,524 participants per month[5]
Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)	1975	4,319[6]	7,490,841 participants per month
School Breakfast Program (SBP)	1975	1,566[4]	8,144,384 breakfasts per day
Nutrition Services Incentive Program (NSIP)[7]	1975	152	252,748,643 total meals[8]
Nutrition Education and Training Program (NET)	1977	0	0
Food Distribution Program on Indian Reservations (FDPIR)	1977	69	110,122 participants per month
Child and Adult Care Food Program (CACFP)	1978[9]	1,852[4]	1,691,448,979 total child meals and snacks; 44,570,764 total adult meals and snacks
Nutrition Assistance Program for Puerto Rico, American Samoa, and the Northern Marianas (NAP)	1981	1,362[10]	Not available
The Emergency Food Assistance Program (TEFAP)	1981[11]	435[12]	611 million total pounds of food distributed
WIC Farmers' Market Nutrition Program (FMNP)	1992	25[13]	2+ million total participants[13]
Team Nutrition Initiative (TN)	1995	10[14]	Not available
Senior Farmers' Market Nutrition Program (SFMNP)	2002	13[15]	Not available

[1]Year of permanent authorization. Several food assistance and nutrition programs started as pilot projects before being established as permanent programs.

[2]Unless otherwise noted, data on costs and participation were obtained from USDA/FNS administrative data for FY 2002 (http://www.fns.usda.gov/pd, accessed April 2003). Reported costs include all cash benefits/reimbursements, food/commodity costs (as applicable), and administrative costs.

[3]In 1998, the program began covering snacks served in after-school programs. In FY 2002, a total of 122,914,873 snacks were served.

[4]In FY 2002, an additional $124 million was spent on State administrative expenses for the NSLP, the SBP, and the CACFP.

[5]Individuals in participating households.

[6]Excludes estimated cost of WIC Farmers' Market Nutrition Program (FMNP), based on FY 2002 appropriation for FMNP.

[7]Formerly known as the Nutrition Program for the Elderly (NPE). In FY 2003, administration for the program was transferred to the U.S. Department of Health and Human Services. FNS continues to supply commodities and financial support to the program.

[8]Total meals for FY 2001, the latest year for which FNS collected data.

[9]The adult day care component was added in 1989. In 1999, the program expanded to serve children living in homeless shelters.

[10]The FY 2002 grant for Puerto Rico was $1,351 million, the grant for American Samoa was $5.3 million, and the grant for the Northern Marianas was $6.1 million.

[11]Until 1996, FNS operated a separate Commodity Distribution Program for Charitable Institutions, Soup Kitchens, and Food Banks. Under the Personal Responsibilities and Work Opportunities Reconciliation Act (PRWORA), this program was merged into TEFAP.

[12]In FY 2002, FNS donated an additional $16 million in commodities to disaster relief and charitable institutions.

[13]Cost reflects FY 2003 appropriation. Source: http://www.fns.usda.gov/wic/FMNP/FMNPfags.htm, accessed April 2003.

[14]FY 2002 appropriation. Source: L. French (2002). Personal communication.

[15]Based on FY 2002 appropriation ($15 million) and residual carried over into FY 2003 ($1.7 million). Source: http://www.fns.usda.gov/wic/ Senior FMNP/SFMNPFY02.htm and SFMNPFY03.htm, accessed April 2003.

USDA's Economic Research Service (ERS) contracted with Abt Associates Inc. to conduct the Nutrition and Health Outcomes Study. A major focus of the study was a comprehensive review and synthesis of existing research on the impact of FANPs on nutrition- and health-related outcomes. This report presents results of that effort.[4]

Identifying Relevant Research for Review

The objective of the literature review was to summarize current knowledge about the effects on FANP participation on nutrition- and health-related outcomes. The first step was a comprehensive literature search. The approach to identifying empirical studies to be included in the research summary followed principles in *The Handbook of Research Synthesis* (Cooper and Hedges, 1994). This text is generally accepted as a definitive reference on research synthesis. The cornerstone of the process is a comprehensive computerized search of bibliographic databases. The following sections describe the methods used to conduct the computerized search and the steps taken to cross-check and expand the resulting list of citations.

Computerized Literature Search

In defining parameters for a literature search, two key concerns are *recall* and *precision* (White, 1992). Recall refers to the hypothetical percentage of all relevant citations that are actually identified through the search. Precision refers to the percentage of identified citations that are ultimately judged relevant to the research synthesis. Precision and recall tend to vary inversely. A search designed to yield a high recall will invariably have less precision—that is, it will yield numerous irrelevant references. On the other hand, a search designed to be highly precise will yield fewer, more focused references but will run a greater risk of missing relevant research.

[4]A separate summary report (Fox and Hamilton, 2004) presents major findings from each of the detailed chapters included in this report. In addition, the Nutrition and Health Outcomes Study produced six other reports. One report reviews the research designs available to researchers interested in studying the effects of FANPs (Hamilton and Rossi, 2002) and another describes existing data sources that might be useful in these endeavors (Logan et al., 2002). The four other reports summarize the nutrition and health characteristics of low-income populations, using data from the third National Health and Nutrition Examination Survey (NHANES-III). The reports cover FSP participants and nonparticipants (Fox and Cole, 2004a), participants and nonparticipants in the Special Supplemental Nutrition Program for Women, Infants, and Children (Cole and Fox, 2004a), school-age children (Fox and Cole, 2004b), and older adults (Cole and Fox, 2004b).

The search completed for this summary emphasized recall over precision. In essence, it was accepted that staff would need to weed through numerous irrelevant citations to identify literature that was truly representative of the existing research. The search was highly inclusive and used overlapping search methods. The selection of searchable databases and search terms (keywords) were both carefully considered, as described below. The actual search was carried out by a research librarian with extensive experience in supporting social science research.

Selecting Searchable Databases

The first step in selecting databases was to define relevant disciplines (or fields of study) and research subject areas. After a careful review of available databases and their topical coverage, the following list of disciplines/subject areas was defined:

- Medicine and health
- Nutrition
- Nursing and allied health
- Health economics
- Health education
- Social science research
- Agricultural research, economics, and policy
- Education research
- Social services and public welfare
- Public health

These subject areas were used to select a group of searchable databases. The initial subject-specific list was expanded to include a number of more general databases targeted toward "gray" or unpublished research, including those that cover dissertations, conferences, foundation grants, ongoing research projects, and government documents. A total of 26 databases was included in the online search (table 2).

The *Dialog Information Retrieval Service* (Dialog) was selected as the main vehicle for the search. Among information retrieval services, Dialog provides access to the largest number of social science research databases via a single, integrated user interface. Indeed, as noted in table 2, Dialog provided direct access to all but three of the selected databases. It also provides such special features as the capability to search multiple databases simultaneously and to remove duplicates as they occur across databases.

Defining Search Parameters

Because the search was so large and complex, it was completed in two waves. The 26 databases were divided

Table 2—Searchable databases used in computerized literature search

Database name[1]	Database producer	Subject category
Ageline	American Association of Retired Persons	Social services and public welfare
Agricultural Online Access (AGRICOLA)	U.S. National Agricultural Research Library	Agricultural research; economics; policy
Biological and Agricultural Index (BAI)	H.W. Wilson Company	Agricultural research
Combined Health Information Database (CHID)[1]	U.S. National Institutes of Health	Health education; public health
Computer Retrieval of Information on Scientific Projects (CRISP)[2]	U.S. National Institutes of Health	Public health; medicine and health
Conferences Papers Index	Cambridge Scientific Abstracts	General
Current Research Information System (CRIS)	U.S. Department of Agriculture	Nutrition
Dissertation Abstracts Online	University Microfilms, Inc.	General
Economic Literature Index (EconLit)	American Economic Association	Health economics
Education Research Information Center (ERIC)	U.S. Department of Education	Education research
Excerpta Medica (EMBASE)	Elsevier Science; Netherlands	Medicine and health; health economics; public health
Federal Research in Progress (FEDRIP)	U.S. National Technical Information Service	General
Foundation Grants Index	The Foundation Center	General
GPO Monthly Catalogue	U.S. Government Printing Office	General
Health and Wellness Database (HPD)	Information Access Company	Medicine and health; nutrition
HealthStar	U.S. National Library of Medicine	Health economics
Inside Conferences	British Library	General
MEDLINE	U.S. National Library of Medicine	Medicine and health; nutrition
National Technical Information Service Bibliographic Database	U.S. National Technical Information Service	General
Nursing and Allied Health Database[3]	Cinahl Information Systems	Nursing and allied health; medicine and health; nutrition
Nutrition Abstracts and Reviews, Series A: Human and Experimental	CAB International; England	Nutrition
PAIS International	Public Affairs Information Service	Social science research
Social Sciences Index	H.W. Wilson Company	Social science research
Social Sciences Abstracts	H.W. Wilson Company	Social science research
Social SciSearch	Institute for Scientific Information	Social science research
Sociological Abstracts	Sociological Abstracts, Inc.	Social services and public welfare

[1] Searched via Dialog, except as noted.
[2] Searched via the Worldwide Web.
[3] Searched via Data Star.

into two groups and each group was searched independently. Databases were grouped to minimize overlap; that is, those likely to yield duplicate records were grouped together to permit removal of duplicates before citations were downloaded.

For each set of databases, 16 separate searches were conducted—one for each program listed in table 3, as well as one using the generic terms "nutrition assistance," "food assistance," "nutrition supplementation," and "nutrition education." Each search included all of the search terms identified in table 4.

Searches were limited to English language documents and to records from 1973 to 2002.[5] Program-specific sets of citations were created by merging results of the two search waves and removing duplicate records.

[5]The initial search was conducted in 1999. The bibliography was updated in 2002, before preparation of the final version of the report. The 2002 update included only published research. Additional published research was incorporated before final publication in 2004.

Identifying Relevant References

All of the citations generated by the search were initially captured in a "browsing format" that provided title and indexing information (keywords used in indexing the citation in the database) without the cost of retrieving a full citation. These abbreviated citations were manually reviewed by chapter authors to identify sources that were potentially relevant for the research review. Because the focus of the literature review was the impact/effect of FANPs on nutrition and health outcomes, citations deemed potentially relevant were those that appeared to summarize research comparing program participants with nonparticipants. All citations selected for further review were downloaded in full format, consisting of a complete citation and, where available, an abstract.

Table 3—Program names, acronyms, and variants used in computerized literature search

Child and Adult Care Food Program (CACFP) Child Care Feeding/Food Program (CCFP) Adult Care Feeding/Food Program Homeless Children Nutrition Program[1] Child Nutrition Homeless Demonstration Project[1]	Nutrition Program for the Elderly (NPE)[3] Elderly Feeding Program Elderly Nutrition Program
Commodity Distribution to Charitable Institutions, Soup Kitchens, and Food Banks[2] Commodity Distribution Program[2] Commodity Donation Program[2]	School Breakfast Program (SBP) Breakfast Program
Commodity Supplemental Food Program (CSFP)	Special Milk Program (SMP) Supplemental Milk Program
Food Distribution Program on Indian Reservations (FDPIR)	Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) Special Supplemental Food Program for Women, Infants, and Children WIC program
Food Stamp Program (FSP) Food Stamps	Summer Food Service Program (SFSP) Summer Feeding Program
National School Lunch Program (NSLP) School Lunch Program	Team Nutrition (TN) Team Nutrition Initiative (TNI)
Nutrition Assistance Program for Puerto Rico and the Northern Marianas (NAP) Puerto Rico/Puerto Rican Nutrition Assistance Program	Temporary Emergency Food Assistance Program (TEFAP) Emergency Feeding Program Emergency Food Program
Nutrition Education and Training (NET) Nutrition Education and Training Program (NETP)	WIC Farmers' Market Nutrition Program Farmers' Market Nutrition Program[4]

[1]In July 1999, the Homeless Children Nutrition Program was discontinued as a separate program and formally incorporated into the CACFP.
[2]Under PRWORA, the previously separate Commodity Distribution to Charitable Institutions, Soup Kitchens, and Food Banks Program was combined with the Temporary Emergency Food Assistance Program to form The Emergency Food Assistance Program (TEFAP).
[3]In 2001, the Nutrition Program for the Elderly (NPE) was renamed the Nutrition Services Incentive Program (NSIP).
[4]The Senior Farmers' Market Nutrition Program was not included in the search because the program was not established until 2002.

Table 4—Keywords used in querying searchable databases

General terms	Specific terms		
Food/nutrient availability Food/nutrient intake Food/nutrient consumption	Breakfast consumption Diet Dietary adequacy Dietary effects Dietary impacts Dietary intake Dietary outcomes Dietary quality Dietary patterns Dietary practices	Dietary trends Dietary variety Eating behaviors Eating practices Folic acid Food choices Food consumption Food costs Food expenditures Food intake	Food purchases Food selections Food use Healthy Eating Index (HEI) Nutrient availability Nutrient content Nutrient intake Nutritional adequacy Nutritional intake
Health-related behaviors Health-related practices	Alcohol use Breastfeeding Breast feeding Cigarette (tobacco) use	Cow's milk (use of) Drug abuse Drug use Immunizations	Infant feeding practices Perinatal care Prenatal care Smoking
Pregnancy and birth outcomes	Birthweight Birth weight Fetal growth Fetal outcomes Gestational age Head circumference Infant morbidity Infant mortality Intrauterine growth retardation	Length of gestation Light-for-date infants Low birthweight Low birth weight Low birth-weight Maternal morbidity Maternal mortality Maternal weight gain Neonatal morbidity Neonatal mortality	Neural tube defects Perinatal morbidity Pregnancy Pregnancy outcome(s) Prematurity Preterm delivery Preterm infants Very low birthweight Very low birth weight Very low-birthweight
Nutrition/health status Nutrition outcomes Health outcomes	Allergies Anemia Body Mass Index (BMI) Body measurements Body weight Bone density Fertility Folacin status Food intolerances Growth Growth rate Growth velocity Health	Health outcome(s) Health status Height Hematocrit Hemoglobin Iron deficiency Iron-deficiency Iron deficient Iron-deficient Iron status Length Malnutrition Morbidity	Mortality Nutrition Nutritional status Obesity Overnutrition Overweight Postnatal growth Skinfold(s) Stature Undernutrition Underweight Weight Weight gain
Other relevant outcomes	Behavioral development Cognitive development Cognitive performance Food insecurity	Food security Functional status Hunger School attendance	School performance Social isolation Quality of life
Health economics	Healthcare (access, utilization, needs, costs) Medical (care, costs, needs) Medicaid Medicare Medicaid costs Medicare costs		

Citations flagged as irrelevant for the research review included:

- General program descriptions.
- Program manuals and guidance materials.
- Descriptive research on program participation and/or costs.
- Descriptive research on participant characteristics.
- Research on issues related to program operations, such as use of electronic benefits transfer (EBT) in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC).
- Research related to program accountability, fraud, or abuse.
- Research related to determinants of outcomes of interest with no mention of impact or effect of program participation (for example, research on factors that influence decisions about breastfeeding).

In addition, research that involved FANP participants but did not explicitly compare participants and nonparticipants was excluded. For example, studies that examined the effectiveness of a specific smoking cessation or breastfeeding promotion program among WIC participants were excluded, as were studies that examined specific interventions designed to decrease the fat content of school lunches. Although useful for other purposes, this type of research sheds no light on the impact of FANP participation on nutrition- and health-related outcomes.[6]

Not surprisingly, numerous relevant citations were located for the flagship FANPs (FSP, WIC, and NSLP). Many fewer citations were located for the smaller programs. Exclusion criteria were relaxed somewhat for programs that generated few relevant citations. Although the citations considered under these relaxed standards were not expected to include information on program effects or to lead to other relevant research, they were retained in the bibliography to ensure that the final report would provide general information about the type of research that has been done on the FANP in question.

Though the computer searches were comprehensive, as tables 2-4 demonstrate, any such search is imperfect. To guard against important omissions, initial lists of program-specific citations from the computer searches (minus the exclusions noted above) were cross-checked against several existing research reviews (Nelson et al., 1981; Rush et al., 1988; Fraker, 1990; Rossi, 1998; Besharov and Germanis, 2001), as well as against a listing of recent FNS research publications. A summary of preliminary citations was submitted to ERS and was reviewed by staff at ERS, FNS, and members of the project's expert panel. Additional citations provided by these reviewers were incorporated before documents were retrieved and reviewed.

Documents were obtained from Abt's in-house library, local university libraries, interlibrary loan, relevant Federal agencies, and, when necessary, from primary authors. All retrieved citations were reviewed by chapter authors. Using the exclusion criteria described previously, as well as a review of research design and methodology, authors identified research that provided empirical information on the effect of FANP participation on nutrition- and/or health-related outcomes. These documents formed the foundation of the research review. Other relevant references were identified by authors as they reviewed papers and reports and cross-checked bibliographies.

Organization of This Report

The next chapter provides an overview of the research designs and outcome measures used in the literature reviewed.[7] **All readers are encouraged to read chapter 2 before reading any of the program-specific chapters that follow it.**

The remainder of the report consists of 14 chapters that summarize available research for all of the FANPs identified in table 1, with the exception of the Senior Farmers' Market Nutrition Program, which was not established until 2002. The Team Nutrition Initiative (TN) and the Nutrition Education and Training Program (NET) are covered in a single chapter.

[6]Much of this research on FANP participants (without nonparticipant controls) involved nutrition education interventions. Readers interested in general information on the effectiveness of such interventions are referred to a comprehensive series of literature reviews prepared by FNS. These reviews summarize research on the effectiveness of nutrition education for six population groups: pregnant women and caretakers of infants, preschool-age children, school-age children, adults, older adults, and intermediaries, paraprofessionals, and professionals. Complete citations for these reports are provided in the reference list at the end of this chapter (highlighted with asterisks).

[7]A more comprehensive discussion of the strengths and weaknesses of the various designs, as well as descriptions of other possible designs, can be found in a separate report (Hamilton and Rossi, 2002).

Each program-specific chapter includes the following:

Program Overview**—**A summary of the program's legislative history and its benefits and eligibility requirements, with current information on program costs and participation, and, as appropriate, on current policy issues.

Research Review**—**A description and synthesis of research on the impact of the relevant FANP on nutrition- and health-related outcomes. Where no such research was identified, there is a description of the type of research that has been done and important or interesting findings from the most recent or most relevant research.

Summary**—**A review of what is and is not known about the nutrition- and health-related impacts of the FANP, with areas for future research identified.

For FANPs that have been widely studied, two types of tabular presentations are used to provide an overview of the breadth of existing studies and the relative consistency of their results:

(1) Tables that summarize the important characteristics of each study, including the year published (or written, for nonpublished reports), data sources, population studied, sample size, research design, measure of program participation, and analysis method(s). Table 5 is an example.

(2) Tables that summarize research results for a specific outcome or set of outcomes. These tables provide a visual overview of the patterns of research findings, using a format similar to that in table 6.

As with any distillation of complex data, these tabular summaries involved compromise. It is important that readers understand four aspects of this compromise before reading the program-specific chapters.

First, summaries do not provide information on the size of any effects detected or on the level of statistical significance reported. This information would greatly increase the size and complexity of the summary table, making it harder for the reader to see the general pattern of statistically significant effects. Interested readers should refer to original papers and reports for more detailed information. Summary tables include all differences reported to be significant at the 5 percent level or better.

Second, nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. A consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted that way.

Third, to give a complete picture, summary tables present findings for all studies reviewed, including older studies and those with comparatively weak designs. However, when discussing conclusions that can be drawn from the available research, the authors intentionally avoid the simplistic and flawed approach of "vote counting" (adding up the number of studies that report differences favorable to participants). Rather, the authors give greater weight to findings from studies that have the strongest research designs and are most recent.

Finally, as in table 6, summaries of findings related to impacts on dietary intake show whether participants consumed more or less food energy or nutrients than nonparticipants, which is consistent with the general approach in the reviewed literature. Comparisons of participants and nonparticipants were most often based on mean intakes as a percentage of age- and gender-appropriate Recommended Dietary Allowances (RDAs), and study authors generally interpreted greater mean intakes among participants as evidence of a positive program impact.

This approach to assessing dietary intakes of groups was common practice at the time most of the studies reviewed in this report were completed. Readers are cautioned to avoid this "more is better" interpretation, however. The reality is that a significant difference in the mean intakes of two groups does not necessarily mean that the two groups differ in the proportion of individuals with inadequate diets. In recent years, methods to assess dietary intakes have improved substantially. For many nutrients, researchers can now reliably estimate the prevalence of inadequate intakes in specific population subgroups, which is discussed in more detail in chapter 2.

Table 5—Studies that examined the impact of the Food Stamp Program on dietary intakes of individuals

SAMPLE TABLE—INCLUDED FOR ILLUSTRATIVE PURPOSES ONLY

Study	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group IA: Participant vs. nonparticipant comparisons—Secondary analysis of national surveys						
Dixon (2002)	1988-94 NHANES-III	24-hour recall	Adults ages 20 and older	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Bhattacharya and Currie (2000)	1988-94 NHANES-III	24-hour recall and nonquantified food frequency	Youth ages 12-16 (n=1,358)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Group IB: Participant vs. nonparticipant comparisons—State and local studies						
Fey-Yensan et al. (2003)	Low-income areas in Connecticut (1996-97)	Food frequency questionnaire	Low-income elderly living in subsidized housing (82% female) (n=200)	Participant vs. nonparticipant	Participation dummy	Chi-square tests and analysis of variance
Group IIA: Dose-response estimates—Secondary analysis of national surveys						
Gleason et al. (2000)	1994-96 CSFII/DHKS	2 nonconsecutive 24-hour recalls	Low-income individuals (n=3,935)	Dose-response	Benefit amount	Comparison of regression-adjusted means
Group IIB: Dose-response estimates—State and local studies						
Butler and Raymond (1996)	1980-81 FNS SSI/ECD and 1969-73 RIME	24-hour recall via telephone and in-person	Low-income elderly individuals (n=1,542) Low-income individuals in rural areas (n=1,093)	Dose-response	Participation dummy; bonus value	Multivariate endogenous switching model with selection bias adjustment

[1]Data sources:

CSFII = Continuing Survey of Food Intakes by Individuals.
DHKS = Diet and Health Knowledge Survey.
FNS SSI/ECD = Food and Nutrition Service Supplementary Security Income/Elderly Cashout Demonstration.
NHANES = National Health and Nutrition Examination Survey.
RIME = Rural Income Maintenance Experiment.

Note: this is a partial version of the actual table, included for illustrative purposes only.

Table 6—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals

SAMPLE TABLE—INCLUDED FOR ILLUSTRATIVE PURPOSES ONLY

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Food energy and macronutrients				
Food energy	***Children*** Fraker (1990) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {preschool} Perez-Escamilla (2000) [2 sites; P-N] Rose (1998a) [national; D-R] Cook (1995) [national; P-N] Gregorio (1984) [national; P-N] ***Elderly*** Fey-Yensan (2003) [1 State; P-N] Weimer (1998) [national; P-N] Posner (1987) [6 sites; P-N] Lopez (1987a) [national; P-N] Butler (1985) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Whitfield (1982) [1 city; D-R] Bishop (1992) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {school-age} West (1978) [1 State; D-R] ***Elderly*** Lopez (1987a) [national; P-N] ***Women*** Fraker (1990) [national; P-N]	***Elderly*** Butler (1996) [6 sites; D-R]

See notes at end of table.

Continued—

Table 6—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals

SAMPLE TABLE—INCLUDED FOR ILLUSTRATIVE PURPOSES ONLY

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Protein	***Children*** Fraker (1990) [national; P-N] ***All households*** Bishop (1992) [national; P-N]	***Children*** Rose (1998a) [national; D-R] Cook (1995 [national; P-N] Gregorio (1984) [national; P-N] ***Elderly*** Lopez (1987a) [national; P-N] Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Fraker (1990) [national; P-N] ***Rural*** Butler (1996) [2 sites; D-R] ***All households*** Whitfield (1982) [1 city; D-R]	***Children*** Gleason (2000) [national; D-R] Perez-Escamilla (2000) [2 sites; P-N] West (1978) [1 State; D-R] ***Elderly*** Fey-Yensan (2003) [1 State; P-N] Weimer (1998) [national; P-N] Lopez (1987a) [national; P-N] ***Adults*** Dixon (2002) [national; P-N]	***Elderly*** Butler (1996) [6 sites; D-R]

Notes: Cell entries show the senior author's name, the publication date, the scope of the study (for example, national vs. 1 city or 1 State), and the research approach (P-N = participant vs. nonparticipant study, D-R = dose response study).

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

This is a partial version of the actual table, included for illustrative purposes only.

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*Asterisked citations are literature reviews prepared by FNS (see footnote 6 in the text). These reports summarize information on the effectiveness of nutrition education interventions for specific population groups.

Chapter 2

Research Methods

This chapter provides an overview of the research methods used in the studies summarized in this report. There are two main sections:

(1) Evaluation design—Much of this discussion is adapted from Volume 1 of this series, *Research Design* (Hamilton and Rossi, 2002).

(2) Outcome measures.

Readers with limited knowledge of research design or measurement issues in nutrition and health-related research are encouraged to read this chapter before any of the program-specific chapters that follow and to use it as a technical resource, as needed.

Evaluation Design

The studies reviewed in this report attempted to measure the impact of specific food and nutrition assistance programs (FANPs) on nutrition- and health-related outcomes. The impact of a program or other intervention is defined as the difference between what happens in the presence of the intervention and *what would have happened* in its absence, generally called the "counterfactual."

Establishing the counterfactual—that is, estimating what would have happened without a given program—is usually accomplished by examining a population that has not been subjected to the program. What makes the task difficult is the fact that people who become participants in a social program are often quite different from those who do not because they either have been selected for participation or have selected themselves (Campbell and Stanley, 1963).[8] These selective processes may make participants different in important ways from those who do not participate. These differences include not only people's permanent characteristics, such as their gender or race, but also transitory ones like their current income or employment, the opportunities they face, and the experiences they have had. Many of the transitory characteristics result from the time and place in which people live, which means that similar people in a different time or place may not appropriately represent the counterfactual. All of these influences may contribute to selection bias, which distorts the evaluation of a program's impact.

The sections that follow describe key research designs encountered in FANP research and their various strengths and limitations. In the program-specific chapters that constitute the remainder of this report, the research design used in each study is clearly identified. The text generally includes some discussion of design limitations; however, the present chapter serves as the primary source of information on research methodology.

The Randomized Experiment

There is a strong consensus in the scientific community that only randomized experiments are fully capable of providing reliable estimates of a program's impacts. The randomized experiment is the "gold standard" of program evaluation.

In the simplest randomized design, potential participants are randomly assigned to either an experimental (or treatment) group, which will be subject to the program being assessed, or to a control group, from which the program will be withheld. The program's impact is then estimated by comparing the average outcomes in the experimental group, after sufficient exposure to the program, with control group outcomes measured at the same time.

Because the experimental and control groups differ at the outset only by chance, they are considered to be fully comparable at that point. In other words, the two groups are considered to be equivalent, in the statistical aggregate, on all permanent and transitory characteristics. Subsequently, the only systematic difference between the groups is exposure to the program. Accordingly, it is credible to infer that any post-program differences between the two groups are caused by the program, provided that the differences are greater than what might occur by chance.

The fundamental requirement of randomized experimentation is that program services be deliberately withheld from some people who are otherwise like the

[8]Evaluation designs often focus on units other than people, either aggregations of people (households, students in a school, the population of a county) or operating entities (program offices, schools, businesses). For simplicity of presentation, the present discussion generally refers to individuals rather than aggregations or other entities.

people receiving the service. Such a practice is generally prohibited in entitlement programs because law and regulation require that program benefits be provided to everyone who meets eligibility requirements and takes the necessary steps to qualify. Many FANPs are entitlement programs.

Saturation programs—those with sufficient funding and infrastructure to serve essentially all eligible people—pose similar problems. Whether a potentially eligible person can receive benefits from a nonentitlement program depends on the local availability of program funding and infrastructure. For many nonentitlement programs that approach full saturation, like the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), it can be virtually impossible to find a reasonably representative set of potential participants to whom the program could be considered unavailable. If program services would normally be provided to everyone who applies and is eligible, it may be considered unethical to withhold services for research purposes from people who might apply.

Given these challenges, it is not surprising that the literature reviewed for this report included only one study that used a randomized experiment to evaluate the impacts of a specific FANP. This study was completed by Metcoff and his colleagues (1985) during the early years of the WIC program. Random assignment was feasible because, at the time, the demand for WIC participation at the site in which the study was conducted exceeded the available funding.

A few studies have used randomized experiments to estimate the impact of demonstrations or pilot programs, rather than of the FANPs per se. These demonstrations typically represented policy initiatives that were tested on a limited scale before full-scale implementation. The most prominent examples are demonstrations of cashing out food stamps (the so-called “cash-out” studies (Fraker et al., 1992; Ohls et al., 1992) and studies of pilot projects in which school breakfasts were offered free to all school children (universal free breakfast projects) (for example, Peterson et al., 2003; McLaughlin et al., 2002; Murphy and Pagans, 2001).

Keep in mind when interpreting results of evaluations of demonstration projects that, in these evaluations, the counterfactual is not the absence of the program. Rather, it is the *status quo*, or the program as it exists without the innovation or modification introduced by the demonstration. Control subjects experience usual program services but are not offered the new services specified in the intervention. In the case of the food stamp cash-out demonstrations, for example, the evaluations estimated the effects of receiving benefits in the form of checks rather than as food stamps, but not the overall impact of the Food Stamp Program (FSP) itself.

Quasi-Experiments

Virtually all the research that has examined the impact of FANPs on nutrition and health outcomes has identified counterfactual conditions without random selection into treatment and control groups. Such impact evaluation designs are known as quasi-experiments. That is, they resemble experiments in providing a specific representation of the counterfactual, but the counterfactual is identified through some means other than random selection. Most of the FANP research reviewed in this report used one of four quasi-experimental designs.

Quasi-Experiment 1: Comparing Participants With Nonparticipants

This design, referred to as “participant vs. nonparticipant” in the program-specific chapters, is the one most commonly used in the research summarized in this report. It calls for identifying comparable groups of participants and nonparticipants and interpreting the average difference in outcomes between the groups as the effect of the program. Nonparticipants must be potentially eligible—that is, people who apparently could have applied and qualified for the program, but did not—to be a credible representation of the counterfactual. In most, but not all, FANP studies, researchers apply an approximation of the means test to identify nonparticipants with incomes below the eligibility cutoff for the program in question.

Selection Bias in Participant/ Nonparticipant Comparisons

The major problem with this quasi-experimental design is that identified nonparticipants may not be sufficiently comparable to participants. This problem, known as selection bias, is a difficult issue in all quasi-experimental designs and is especially troublesome when people who have taken the actions necessary to participate in a program are compared with people who have not.

Selection bias often occurs because participants are more highly motivated to achieve the program-relevant outcomes than nonparticipants. Suppose, for example, that the women who seek WIC benefits for themselves or their children tend to be very concerned about the effect of diet on their children's health. Such women may well take other actions with the same objective, such as following nutrition advice included in brochures they pick up in the doctor's office—or getting to a doctor's office at all. If this supposition were true, one would expect the children of mothers who seek WIC benefits to have better nutrition and health outcomes—even in the absence of the program—than children of mothers who are less motivated and do not seek WIC benefits. A simple comparison of WIC and non-WIC children would therefore reveal that the WIC children had more positive outcomes even if the program had no effect at all.

Sometimes selection bias operates in the opposite direction. Mothers of children with nutrition-related problems might be especially motivated to seek WIC benefits, for example, whereas mothers of healthy children might be less inclined to participate. WIC might improve the participating children's condition, but the children might not catch up to their nonparticipating, healthier counterparts. In this example, the simple comparison would find WIC children to have less positive outcomes even though the program had a positive effect.

Motivation of participants toward the program outcome is one of the most common sources of potential bias, and one of the most difficult to counteract. Other common sources of self-selection bias include need (often proxied by income), potential for gain (often proxied by the dollar value of the benefit), and the individual's desire not to depend on public assistance.

Selection bias may also result from program rules or procedures. In nonentitlement programs, local staff often decide which applicants will be approved for participation based on a combination of program policies and individual judgment. In all programs, outreach practices, referral networks, office locations and hours, and community customs may make some people more likely to participate than others.

Finally, some selection bias occurs when program participation is based on transitory characteristics. For example, some people who qualify for means-tested programs are permanently poor, or nearly so, and would be income-eligible for program participation for many years. Other people who qualify for the same programs are not permanently poor, but are at a temporary low point in a fluctuating income pattern. In an earlier period, their income was high enough that they did not qualify for the program, and at some point, they will regain that level. These two types of people might have similar incomes at the time they enter the program, but their subsequent outcomes, in the absence of the program, might not be at all similar.

Approaches To Dealing With Selection Bias

Researchers have used a variety of approaches to attempt to counteract selection bias, the most common of which are described below.[9] All have the basic objective of making the participant and nonparticipant groups "alike" on certain specified dimensions. However, all leave open the possibility that bias remains.

Regression Adjustment. A prime example of this approach is the WIC-Medicaid study conducted by Devaney et al. (1990 and 1991) to assess the impact of prenatal WIC participation on birth outcomes. Taking advantage of the fact that all Medicaid recipients were automatically eligible for WIC benefits, Devaney and her colleagues contrasted birth outcomes of Medicaid recipients who had participated in WIC during pregnancy with those who had not. The relevant dataset was assembled by linking Medicaid records to WIC participation records and birth registration records. Birth registration records provided information on the critical outcome of birthweight, WIC records identified WIC participants, and Medicaid records identified those who gave birth during the period of study. The resulting linked WIC-Medicaid database included approximately 112,000 births to Medicaid mothers in five States over a 2-year period.

To minimize selection bias, Devaney and her associates used regression adjustments. The equations included variables that were likely to capture ways in which participants and nonparticipants might differ, including educational attainment, prenatal medical care, gestational age, race, mother's age, and birth parity. As typically happens, the researchers were limited to the variables available in existing datasets, which seldom measure all of the factors that might create different outcomes for participants and nonparticipants. Alternative attempts to counter selection biases led to quite drastic changes in estimates of the effects, without any clear indications of which attempt was more sensible.

[9]Another technique for dealing with selection bias is the use of propensity scores. Propensity scoring allows a more comprehensive and complex treatment of covariates than is possible with regression adjustment (Hamilton and Rossi, 2002). However, though propensity score methods have been used extensively in public health research, they were not used in the literature reviewed for this report.

Matched Pairs. Sometimes researchers construct a comparison group by matching participants and nonparticipants on characteristics thought to be related to selection tendencies. For each participant in the research sample, the researcher identifies a nonparticipant with identical or closely similar characteristics on key variables. Because the matching procedure can normally consider only a few variables, regression adjustment is still needed to estimate impacts.

The matched-pair approach is advantageous mainly when there is a substantial marginal cost for including subjects in the evaluation, typically when significant new data collection is to be carried out. If the analysis is based on existing administrative or survey datasets, the matched-pairs approach excludes otherwise usable observations and thus reduces the sample size available for analysis.

More general matching procedures may identify more than one nonparticipant (perhaps even many) similar enough to each participant. When combined with regression adjustment, matched sampling is one of the most effective methods for reducing bias from imbalances in observed covariates (Rubin, 1979).

Dose-Response. If program rules prescribe different amounts of the program benefit or service for different participants, a dose-response analytic model may be applicable. The underlying hypothesis is that greater benefits will lead to greater effects on outcomes. The dose-response relationship may be estimated with a sample that consists only of participants, which eliminates the issue of whether participants differ from nonparticipants in unmeasurable ways. If this relationship can be estimated, then the program's impact may be described as the difference between the effect at any given level of benefits (typically the average benefit) and the projected effect at the zero-benefit level (what participants would receive if they did not participate).

The FSP, with benefits measured in dollars and a large number of actual benefit amounts, is the main candidate for dose-response analysis among the FANPs. A number of researchers have used this approach, although with considerable variation in the way it was applied. Some researchers have estimated models that exclude nonparticipants (for example, Neenan and Davis, 1978; Levedahl, 1991; Kramer-LeBlanc et al., 1997), while others include nonparticipants and specify the model to include both a variable representing the benefit amount and a variable representing participation per se (for example, Fraker, 1990; Devaney and Fraker, 1989).

The dose-response model requires that benefits vary across households that are similar in terms of the factors expected to affect their health and nutrition outcomes. The food stamp situation appears to meet that condition. Households of a given size with a given amount of cash income receive differing benefit amounts depending on, for example, how much of the income is earned and their allowable deductions. However, because the underlying logic driving benefit rules is that the benefit amount should be responsive to need, it would be desirable to see more extensive analysis of the extent to which food stamp benefit variation actually meets the requirements of dose-response analysis.

Two-Stage Models. Some researchers use a two-stage approach in which they first model the likelihood that an individual will be a participant in the program. The model yields a predicted probability of participation for each participant and nonparticipant. The second stage of analysis models the outcome as a function of some measure of participation.

One class of solutions simply uses the predicted probability of participation in place of actual observed participation as an explanatory variable in the second-stage model. Another includes observed participation along with an inverse Mills ratio, which is a function of the predicted probability of participation (Heckman, 1979).

In order for two-stage approaches to offer a material gain over simple regression adjustment, the participation model must include one or more "instruments"—variables that predict participation but are not correlated with the outcomes of interest. Finding an appropriate instrument is often impossible, however, especially when the researcher is working with existing datasets. Participation is typically related to demographic characteristics, need or potential benefit, motivation, and pre-program measures of relevant outcomes, such as nutrition or health status. These same factors usually influence post-program outcomes. And many factors that initially seem like good instruments turn out, on closer examination, to be related to outcomes. For example, living close to a program office might be expected to make an individual more likely to participate and initially seems unrelated to health and nutrition outcomes, but the program's location may have been selected to give easy access to a high-risk community.

In addition to the instrumental variable, some two-stage approaches use functional form to achieve identification in the models. In a procedure known as the two-step Heckman method, the participation model uses a nonlinear functional form (Heckman, 1979; Heckman and Hotz, 1989). Alternatively, the participation and outcome equations can be estimated simultaneously using a maximum likelihood approach. In both cases, the effectiveness of the method depends on the validity of assumptions made about the error terms in the model, assumptions that cannot be verified empirically.

All of these two-stage approaches have been used in evaluating FANPs, but with no clear consensus that any of them can be considered generally reliable. For example, Gordon and Nelson (1995) used three approaches (instrumental variables, Heckman two-step, and simultaneous equations) and a rich dataset to estimate WIC effects on birthweight. They found that the approaches to selection bias correction yielded "unstable and implausible results, [possibly] because the factors affecting WIC participation and birthweight are very nearly identical, since WIC targets low-income women at risk for poor pregnancy outcomes." Ponza et al. (1996) similarly used multiple approaches to selection-bias adjustment in evaluating the Elderly Nutrition Program (ENP). The authors rejected all of the two-stage approaches and based their conclusions on the results of the simple, one-stage regression adjustment.

Caveats to Selection-Bias Adjustment

The most troubling aspect of statistical approaches to adjusting for selection bias is that one cannot be certain whether the procedure has, in fact, eliminated selection bias. Well-conceived applications of selection-bias adjustment models have yielded some plausible and some implausible results in evaluating FANPs. The situations that produce implausible results cannot be identified a priori, and none of the approaches has consistently yielded plausible results. Moreover, a plausible selection-bias adjustment has not necessarily accomplished its purpose just because it is plausible.

When researchers have compared the effects estimated in randomized experimental evaluations with those derived from comparing participants with nonparticipants, the two sets of findings have often been divergent. For example, when La Londe and Maynard (1987) compared the findings from a randomized experiment with those obtained by using comparable nonparticipants as the counterfactual, they found that none of several methods for identifying comparable nonparticipants produced results consistent with the findings from the randomized experiment. However, subsequent work argued that specification tests could have led to a result approaching the estimate from the randomized experiment (Heckman and Hotz, 1989). Nonetheless, after decades of research and debate, the statistical community has not yet reached a consensus that any particular approach will consistently remove selection bias.

In addition, data limitations hamper nearly all attempts to counter selection bias. Careful theorizing about the determinants of participation usually suggests many factors that are not measured in existing datasets. Even with special data collection, many of the factors pertain to the time period before the individual began participating (or not participating) and cannot be measured reliably on a retrospective basis.

Although the extent of remaining bias cannot be known for sure, testing the robustness of the results is usually informative. A program impact estimate that remains stable under various alternative specifications is somewhat more credible than one that varies dramatically. Of course, if several specifications fail equally to remove the bias, their results will be consistent with one another but inaccurate.

Quasi-Experiment 2: Comparing Participants Before and After Program Participation

This simple design (referred to as "participants, before vs. after" in the program-specific chapters) eliminates some dimensions of selection bias but has other major vulnerabilities. Subjects are selected into the study before they have been meaningfully exposed to the program—for example, when they apply for program services. They are clearly aware of the program at this point and have already taken some action to respond to its requirements, but they have not normally been "exposed" to any of the program's benefits in ways that would affect their status on the outcome dimensions of interest.[10] The subjects' status on the outcome dimensions is measured upon their selection into the study and again after program exposure (long enough after exposure that effects are expected to be visible).

The subjects' preparticipation status serves as the counterfactual. The design assumes that, without the program, the individual's preprogram status would not change. If this assumption is valid, the before vs. after difference represents the effect of the program.

A prime example of the "participants before vs. after" design in FANP research is the work done by Yip et al.

[10]This may not be true if the program requires some action before enrollment that may itself affect the person's status on outcome variables of interest. Examples would be preenrollment requirements, such as looking for a job or visiting a doctor.

(1987) on anemia among preschool children. Yip and his colleagues studied infants and preschool children participating in WIC and contrasted hematocrit levels at the time of admission into the program with levels at a followup visit a few months later. The data showed a marked decrease in anemia over the few intervening months. Because the time frame was so short, it is unlikely that the effects were attributable to natural developmental processes or to long-term secular declines in anemia among American children.

When program effects are not expected to occur quickly, the assumptions of the before vs. after design become more tenuous because forces other than program participation might cause changes in participants' status. For example, normal patterns of child development involve substantial changes in many variables over relatively short periods. A related issue is that some conditions improve naturally over time without intervention, a phenomenon known in medical treatment as spontaneous remission and in some statistical circumstances as regression toward the mean.[11] Many people become eligible for means-tested programs because they have experienced a temporary drop in income. Over time, many such people have an improved income, even if they do not enroll in a program. Accordingly, it would be a mistake to assume that the program causes such post-participation gains in income—or in any conditions affected by income, such as many dimensions of nutrition and health status.

General societal trends may also improve conditions of a target population. These include not only long-term trends, like the general reduction in nutrient deficiencies in the United States, but such short-term phenomena as swings in the unemployment rate or changes in Medicaid coverage. Any before vs. after period that lasts more than a few months is potentially vulnerable to such temporal effects, and seasonal effects can sometimes occur within a few months.

Given this vulnerability, the participants before vs. after design is useful mainly for evaluating impacts that are expected to be fully visible within a brief period. If temporal effects might also occur, the design can neither refute the possibility nor control for it statistically.

[11]A related issue is measurement error. If a measure is not fully reliable, that is, is not capable of producing the same result in repeated application, a before vs. after design may indicate negative results for an individual simply because of measurement error. Special measurement efforts may therefore have to be made with this design. For example, infant development studies often require two independent measures of infant length at each time point because infant length is difficult to measure accurately.

Although this design is usually applied prospectively, it can be applied retrospectively if panel datasets provide appropriate information. The researcher must be able to identify people who participated in the program, determine when they began participating, and have comparable measures of the key outcome dimensions for both the pre- and post-program periods.

Quasi-Experiment 3: Comparing Participants to Nonparticipants Before and After Program Participation

This design ("participant vs. nonparticipant, before and after") combines the strengths of the two previous quasi-experiments. It has less vulnerability to selection bias than a simple comparison of participants to nonparticipants and less vulnerability to bias from temporal effects than a before vs. after comparison.

In this design, outcomes for participants and nonparticipants are measured once before participation begins and again after the effects of participation are expected to be visible. Conceptually, the program's impact is estimated as the post-program difference in outcomes, subtracting out the difference that already existed before participation. This design is therefore commonly called a difference in differences or double difference design.

In practice, this design is usually applied with multivariate modeling. The dependent variable in the model is often the post-program outcome, with the pre-program outcome measure as a predictor variable, along with participation status. As in the regression adjustment model discussed previously, the model adjusts for the differing composition of the participant and nonparticipant populations by incorporating covariates that are expected to be related to the outcome measure or to the likelihood of participation.

A noteworthy example of this design is a study conducted by Kennedy and Gershoff (1982). The authors compared changes in hemoglobin and hematocrit levels of pregnant WIC participants and nonparticipants between the first and final prenatal visits.

Although this variation is the strongest of the quasi-experimental designs, it is rarely used to evaluate ongoing entitlement or saturation programs. Because the design calls for pre- and post-participation measures on both participants and nonparticipants, data collection can be complicated and very costly. Moreover, existing national surveys or administrative datasets that collect substantial amounts of nutrition and health outcome data are cross-sectional rather than longitudinal in design.

Quasi-Experiment 4: Time Series Analysis

Time series analyses are an important extension of before-and-after studies that can be employed when many observations of outcomes exist for periods before and after program implementation. Unlike simple before-and-after designs, time series analyses take trends into account. Observations that occur before the program is in place are used to model outcome trends in the absence of the program. The predicted trend represents the counterfactual, and is contrasted with the trend actually observed after the program is in place. The difference between the two trends is attributed to the program.

The version of time series analysis that has been used in FANP research is the cross-sectional time series. This approach uses time series on multiple units, such as series for individual States or counties. A good example is the study undertaken by Rush and colleagues (1988) to assess effects of the WIC program. Taking advantage of the rapid growth of the WIC program in the 1970s, Rush and his colleagues conducted a time series analysis of the effect of the program's growth on birth outcomes. They related the growth of the WIC program between 1972 and 1980 in a large number of counties to county-aggregate data on birth outcomes. The research strategy was based on the expectation that if WIC is effective in improving birth outcomes, improvements ought to be proportional over time to its expansion. Using birth registration records and State WIC records, Rush found that the growth of WIC over this period led to increased average birthweight, longer average duration of gestation, and decreased fetal mortality. These effects were over and above the secular trends for this time period and were especially pronounced for births to less-well-educated and minority women. The analysis covered 19 States and almost 1,400 counties.

Unlike all of the preceding research designs, time series analyses do not focus on outcomes for individual program participants. Rather, they focus on a more broadly defined population that can be examined both before and after the program is introduced. Because the unit of aggregation in most data series is some geographic unit, the analysis estimates the program's impact on the overall population of that area. Where a data series is available for a relevant subpopulation, such as low-income households or pregnant women, the analysis can speak to the impact on that more specific target population.

Estimating impacts for the target population has both advantages and disadvantages. An impact estimate for the target population combines the program's effectiveness in reaching people (its penetration or participation rate) with its effectiveness in helping those it does reach (the impact on participants). Because FANPs are designed to ameliorate problems in specified target populations, this kind of analysis addresses the question of how well the program is achieving its ultimate objective. However, it risks the possibility that a positive impact on program participants may be so diluted by nonparticipants that it is invisible in the analysis. If the data represent the entire population of an area, including those outside the program's target population, the dilution problem is exacerbated.

Outcome Measures

Existing research has examined the impact of FANP participation on a number of different outcomes. The outcomes are logically sequential, as summarized below, using the FSP as an example.

- *Household food expenditures* is the first outcome in the sequence. The FSP, which provides earmarked economic benefits, can be expected to have a direct impact on the amount of money a household spends on food.

- *Household nutrient availability* is the second outcome. If a household increases the amount of money it spends for food, it is expected to increase the availability to household members of food energy and at least some nutrients.

- *Individual dietary intake* is the next outcome in the sequence. For the FSP, the hypothesis is that increased availability of nutrients in the household leads to increased nutrient intake by individual household members. Programs like WIC and the school nutrition programs, which provide specific foods or meals to participants, are hypothesized to have a direct impact on individual dietary intake.

- *Measures of nutrition and health status other than dietary intake, which FANP participation may influence through the above pathways*. Such measures include, for example, birth outcomes, nutritional biochemistries, linear growth in children, and body weight. Relatively recent research on the School Breakfast Program has expanded this set of outcomes to include measures of school and academic performance.

With the exception of the WIC program and the ENP, relatively few FANP studies have examined the last group of outcomes. Moreover, conclusions from studies that have examined these outcomes must be interpreted with caution. Establishing causality between FANP participation and long-term nutrition and health outcomes requires that data support a logical time sequence. For long-term outcomes (measures that develop over time, such as linear growth and body weight), FANP participation must precede the outcome for a reasonable period and be of sufficient intensity to provide a plausible basis for a hypothesized impact. In addition, reliable assessment of impacts on measures such as linear growth and nutritional biochemistries requires at least two measurements, one before participation and one after. Finally, nutrition and health status are influenced by a complex interplay of diet, heredity, and environment, making the task of determining the specific impacts of FANPs on these long-term outcomes a challenge.

A few studies have examined the impact of FANP participation on health-related behaviors, including, specifically, the impact of the WIC program on breastfeeding and child immunizations and the impact of the ENP on socialization among the elderly.

A potential limitation for all outcome measures used in FANP research is the problem of measurement error. Estimation of key outcomes—including household food expenditures, household nutrient availability, and individual dietary intake—involves collecting detailed data over a day, multiple days, a week, or a month. The data are subject to errors associated with respondents' abilities, cooperation, and recall. These errors are assumed to affect participants and nonparticipants in FANP studies equally; however, the overall effect is a reduction in measurement reliability. In turn, reduced reliability increases the likelihood that differences between participants and nonparticipants will be obscured (Rossi, 1998).

The next sections of this chapter describe key outcome measures used in existing FANP research. Later program-specific chapters also include some discussion of the strengths and limitations of various outcome measures; however, the present chapter serves as the primary source of such information.

Household Food Expenditures

Most of the studies that have examined the impact of FANP participation on household food expenditures have focused on the FSP. However, a handful of studies have assessed impacts on food expenditures relative to participation in the WIC program, the National School Lunch Program (NSLP), and the Nutrition Assistance Program (NAP) in Puerto Rico.

Although studies of the impact of FANP participation on food expenditures are conceptually similar, they vary substantially in how food expenditures were measured. Some studies were based on money spent on food for at-home use over the course of a week (or weekly food purchases), while others used the monetary value of food eaten out of household supplies over a week or a month. The former measure includes expenditures for foods not necessarily eaten during the week of purchase and excludes the value of foods used from household inventories during the recall period.

Another important difference relates to whether the measure considered expenditures only for food eaten at home or total food expenditures, including meals and snacks eaten away from home. Finally, some measures included the value of purchased food only, while others also included nonpurchased food (for example, home-grown foods and food received as gifts).

Some researchers analyzed expenditures for the household as a whole, while others normalized expenditures to account for the household's size, its age/sex composition, meals eaten away from home, meals served to guests, and/or economies of scale. Commonly used approaches standardize food expenditures based on "equivalent adults" (EAs), counting additional family members less heavily because of economies of scale, "adult male equivalents" (AMEs), counting family members according to caloric requirements, and "equivalent nutrition units" (ENUs), counting family members according to caloric requirements and percentage of meals eaten at home. In general, the more factors considered in normalizing expenditure data, the better. That is, ENUs provide a more precise assessment of expenditures per household member than the more basic EA measure.

In examining the impact of FANP participation on food expenditures, researchers have used both primary data collection and secondary analysis of data collected in national surveys, such as the Consumer Expenditure Survey (CES), the Panel Study of Income Dynamics (PSID), the Nationwide Food Consumption Survey (NFCS), and the Continuing Survey of Food Intakes by Individuals (CSFII). The latter two surveys are no longer conducted.

Household Nutrient Availability

Assessment of household nutrient availability is based on detailed records of household food use for an extended period, usually 1 week. Information on quantities of food withdrawn from the household food supply is translated into nutrient equivalents to represent the food energy and nutrients available to household members. Although household nutrient availability excludes the nutrient content of food eaten away from home, it is still an important measure because the FSP is specifically intended to improve in-home food consumption.

Nonetheless, nutrient availability at the household level is not equivalent to nutrient intake at the individual level. The relationship between the two measures is weakened by several considerations.

- Some household members will get nutrients from foods eaten away from home.
- Some of the food used from household supplies is wasted.
- Household members may unequally consume nutrients from household food supplies, relative to their needs, depending on their tastes and appetites.

Moreover, increased availability of food energy and nutrients at the household level does not necessarily translate into better diets—for example, lower intakes of nutrients and food components that tend to be overconsumed by many Americans (fat, saturated fat, cholesterol, and sodium) or greater adherence to recommended patterns of food intake (for example, eating fruits and vegetables or whole grains). For these reasons, one must examine the dietary intakes of individual household members to adequately assess nutrition-related impacts of the FSP.

In assessing household nutrient availability, the amount of energy and nutrients available in the foods withdrawn from the household food supply is evaluated relative to the Recommended Dietary Allowances (RDAs) and the household's size and composition. Household nutrient requirements are generally defined based on adult male equivalents (AMEs), which take into consideration the number of individuals in the household and their differing nutrient requirements based on age, gender, and pregnancy/lactation status, or equivalent nutritional units (ENUs), which further adjust for the number of meals each family member eats at home and the number of meals served to guests.

All studies of impacts on household nutrient availability have focused on the FSP. Research has included both primary data collection and secondary analysis of national survey data. Most of the secondary analyses used data from the 1977-78 NFCS (low-income supplemental sample) or data from a followup NFCS low-income sample that was collected in 1979-80.

Individual Dietary Intake

A number of techniques can be used to assess individual dietary intake (Thompson and Byers, 1994). In research on FANP impacts, the technique used most often is a single 24-hour recall or a single-day food record. Some studies collected multiple days of data, ranging from 2 to 7 days, using recalls, records, or a combination approach. Respondents usually reported on their own intakes, but parents or other caregivers served as proxy respondents for infants and young children.

Although all dietary data collection techniques have limitations, it is generally accepted that the more days of data available, the better the measure.[12] In addition, food records are generally believed to be more accurate than recall-only methods because respondents, at least in theory, record food intake on a prospective basis rather than recalling it retrospectively and have the opportunity to measure or carefully observe portions. Food records impose a significant response burden, however, and are particularly problematic for respondents with limited literacy. Moreover, the need to record food intake may alter respondents' eating behavior. For these reasons, recall-based data collection is preferred for assessment of low-income populations.

The 24-hour recall has three key disadvantages. First and most obvious, the method relies on memory, which tends to be imperfect. Second, 24-hour recalls have been shown to be subject to systematic underreporting by some subgroups, including individuals who are overweight (Briefel et al., 1997) and the elderly (Madden et al., 1976). Third, because intakes vary so much from day to day in highly industrialized countries, such as the United States, a single day's intake is unlikely to be representative of the respondent's usual diet (Beaton, 1983).

The accuracy of 24-hour recall data can be improved by careful, standardized interviewing techniques.

[12] There are limitations, however. Experience has shown that quality and completeness of data decrease as the number of days increases. Respondents tend to fill out records less carefully as time goes on, after approximately 4 or 5 days (Gersovitz et al., 1978).

Computer-assisted interviewing is one way to achieve a high level of standardization. One of the first applications of computer-assisted 24-hour recalls was developed for the third National Health and Nutrition Examination Survey (NHANES-III) (McDowell et al., 1989). The approach was refined and improved, based on methodological research, to better engage respondents in the interview process and to provide memory cues for accurate recall of food and beverage consumption (Moshfegh et al., 2001). A version of the improved system was used to collect data for the 1994-98 CSFII, and the final version is being used to collect data in NHANES-IV. A comparable system is included in the Nutrition Data System (NDS), managed by the Nutrition Coordinating Center (NCC) at the University of Minnesota (NCC, 2001).

Recent guidelines for dietary assessment issued by the Institute of Medicine (IOM, 2001) recommend that studies examining dietary intakes of groups collect a minimum of 2 nonconsecutive days or 3 consecutive days of data for a subgroup of the population(s) being studied. The additional data for the subgroup(s) can be used to adjust intake distributions for day-to-day, within-person variation (IOM, 2001).[13] The adjustments provide reliable estimates of usual energy and nutrient intakes. These improved dietary assessment methods are just beginning to appear in FANP research (McLaughlin et al., 2002).

Nutrient estimates generated from dietary intake data generally include only the nutrients provided by the foods and beverages consumed. While studies may collect information on use of vitamin and mineral supplements, the contributions of supplements are seldom included in the estimates.[14] None of the studies reviewed for this report included contributions from supplements.

Comparison to Reference Standards

Most studies that have examined the impact of FANPs on nutrient intakes assessed intakes in reference to established intake standards rather than just comparing raw intakes in kilocalories, milligrams (mg) or grams (gm). At the time most of these studies were conducted, the standards used were the Recommended Dietary Allowances (RDAs) (National Research Council (NRC), 1989a). More recent studies have also used the *Dietary Guidelines for Americans* (U.S. Departments of Agriculture (USDA) and Health and Human Services (HHS), 2000). A few studies used the Healthy Eating Index (HEI) as a summary measure of dietary quality (Kennedy et al., 1995). Each of these reference standards is discussed in turn below.

Recommended Dietary Allowances. Most FANP researchers compared mean intakes of participants and nonparticipants, expressed as a percentage of age- and gender-appropriate RDAs. Some researchers compared the proportion of individuals in each group with intakes below a defined cutoff, generally between 70 and 100 percent of the RDA. The latter approach is less common, perhaps because an expert panel convened in the early 1980s by USDA specifically recommended against the use of fixed cutoffs relative to the RDAs as a means of assessing the prevalence of inadequate intakes (NRC, 1986).

In assessing program impacts, researchers generally deemed a significantly greater mean intake among participants or a significantly greater percentage of participants with intakes above a specified cutoff as evidence of a positive program effect. Effects were characterized as program participation leading to "increased intake(s)."

Although these interpretations are common in the available literature, differences in the mean percentage of the RDA consumed, or in the proportion of individuals consuming some percentage of the RDA, do not provide information on the underlying question: Is the percentage of FANP participants with adequate diets different than the percentage of nonparticipants with adequate diets? Even when mean nutrient intake of a group approximates or exceeds the RDA, significant proportions of the population may have inadequate intakes. On the other hand, use of RDA-based cutoffs seriously overestimates the proportion of a group at risk of inadequate intake because, by definition, the RDA exceeds the needs of nearly all (97-98 percent) healthy individuals in the group (IOM, 2001).

Thus, the available research provides an imperfect picture of both the prevalence of inadequate intakes and the substantive significance of differences in intakes of

[13] Adjustment of intake distributions is necessary to develop accurate estimates of the proportion of the population with inadequate intakes. If research goals are limited to estimates of mean intake for each group, additional days of data are not necessary as long as sample sizes are sufficient (IOM, 2001).

[14] This trend may be changing. There is increasing interest in basing assessment of nutrient intake on complete intake data, including vitamins and minerals provided by supplements. However, because supplement use can be intermittent and because most extant data have inconsistent reference time periods for dietary intake data and supplement data (previous 24-hours vs. use during preceding month or week), combining the two sources of data is not a straightforward task.

FANP participants and nonparticipants. That is, the available data provide information on whether FANP participants have "increased intakes" of food energy or key nutrients relative to nonparticipants but do not provide any information on whether these differences affect the likelihood that FANP participants consume adequate amounts of food energy or nutrients.

This imperfect picture of the risk of inadequacy reflects a limitation in the reference standards and dietary assessment methods available when most of the existing FANP research was conducted, rather than shortcomings in the research per se. This limitation has been addressed in the Dietary Reference Intakes (DRIs), a revised set of nutrient intake standards that has replaced the RDAs (IOM, 1999, 2000a, 2000b, 2002a, 2002b).

The development of the DRIs has led to statistically based guidance on estimating the prevalence of inadequate intakes of population groups (IOM, 2001). The recommended approach, referred to as the "EAR cut-point method," differs in two important ways from the approach used in previous research. First, assessment of adequacy is based on the Estimated Average Requirement (EAR) rather than the RDA. The EAR is the level of intake estimated to meet the requirements of half of the healthy individuals in a given gender and life-stage group.[15] It was developed specifically to provide a better standard for assessing the adequacy of nutrient intakes than is possible with the RDA.

Second, assessment is based on estimates of *usual* rather than observed intakes. As discussed above, estimation of usual intakes requires collecting 2 nonconsecutive or 3 consecutive days of intake data for a subgroup of the population(s) under study. These data are then used to adjust the distribution of intakes to remove within-person variation and better represent usual intake patterns.

Compared with estimates from previous research, the recommended approach to estimating the prevalence of inadequate intakes is likely to yield lower estimates of the prevalence of inadequacy because, as noted, using the RDA as a reference point for assessing adequacy always leads to an overestimation of the problem (IOM, 2001).[16] Similarly, using observed intakes rather than usual intakes tends to overestimate the percentage of individuals falling below a given cutoff because the distribution of observed intakes is usually wider than the distribution of usual intakes.

At the time this report was finalized, only one FANP study had used the EAR cut-point method to estimate the effect of FANP participation on the prevalence of inadequate intakes (McLaughlin et al., 2002). Applying the EAR, in combination with data on usual intakes, is not as straightforward as one might expect because (1) the procedures used to estimate usual intakes adjust *distributions* rather than individual estimates and (2) the IOM specifically cautions against using a binary variable to represent inadequacy in a standard regression model (IOM, 2001). The DRI applications report outlines an analysis strategy for assessing the impact of FANP participation on the prevalence of inadequate intakes (IOM, 2001).

Dietary Guidelines for Americans. The *Dietary Guidelines for Americans* (DGAs) were developed specifically to provide consumers with recommendations that could be used to plan healthful diets (USDA/HHS, 2000). The DGAs have been revised over the years but have always stipulated moderate intake of fat, saturated fat, cholesterol, and sodium.

Relatively few FANP studies have used the DGAs to assess dietary intakes of program participants vs. nonparticipants. Most research that has used the DGAs compared intakes of total fat and/or saturated fat, as a percentage of total energy intake, to DGA recommendations. Because early versions of the DGAs did not include quantitative recommendations for cholesterol and sodium intake, most studies used recommendations from the NRC, which include a maximum of 300 mg per day for cholesterol and a maximum of 2,400 mg per day for sodium (NRC, 1989b). The NRC recommendations for these two nutrients, which were incorporated into guidelines for nutrition labeling, are the ones now included in the DGAs.

The DRIs have defined a new reference standard for intake of total fat, referred to as an Acceptable

[15]For some nutrients, most notably calcium, available data were insufficient to establish an EAR. In these instances, a different DRI—an Adequate Intake or AI—was established. The AI is a level of intake that is assumed to be adequate, based on observed or experimentally determined estimates of intake. The DRIs also define ULs (Tolerable Upper Intake Levels) for selected nutrients. The UL is the highest intake likely to pose no risk of adverse health effects. The DRI applications report provides guidance on appropriate uses of AIs and ULs in assessing nutrient intakes of groups (IOM, 2001).

[16]For some nutrients, the estimated prevalence of inadequate intakes would be lower even if the old approach was replicated using the latest RDAs because the new RDAs for some nutrients differ substantially from previous RDAs. For example, for children ages 1-3, the 1989 RDAs for zinc and vitamin C were, respectively, 10 mg and 40 mg. The new RDAs for these nutrients are substantially lower, at 3 mg (zinc) and 15 mg (vitamin C).

Macronutrient Distribution Range (AMDR) (IOM, 2002b). AMDRs have also been defined for carbohydrates, protein, and specific types of polyunsaturated fatty acids. AMDRs have not been defined for saturated fat or cholesterol because these dietary components have no known beneficial effect in preventing chronic disease and are not required at any level in the diet (IOM, 2002b). DRIs for electrolytes, including sodium, are currently in development (IOM, 2003).

The Healthy Eating Index. Very few FANP studies have examined impacts of FANP participation on the HEI. Developed by USDA's Center for Nutrition Policy and Promotion (CNPP), the HEI is a summary measure of overall diet quality (Kennedy et al., 1995). It is based on 10 component scores, all of which are weighted equally in the total score. The component scores measure different aspects of a healthy diet, based on current public health recommendations. Five of the component scores are food-based and evaluate food consumption compared with the recommendations of the Food Guide Pyramid for grains, vegetables, fruits, dairy, and meat (USDA/CNPP, 1996). Four component scores are nutrient-based and assess compliance with the DGA recommendations for intake of fat and saturated fat (USDA/HHS, 2000), as well as with the NRC recommendations for intake of cholesterol and sodium (NRC, 1989b). The 10th component score is food-based and assesses the level of variety in the diet. Dietary variety is stressed in the Food Guide Pyramid, the Dietary Guidelines, and the NRC's diet and health recommendations (Basiotis et al., 2002).

Health-Related Behaviors

Breastfeeding

A handful of studies have examined breastfeeding initiation and duration among WIC participants and nonparticipants. Initiation is generally defined as ever having breastfed, regardless of frequency or duration. Duration is measured as total length of time and/or as the percentage of mothers who breastfed for 6 months or more.

Socialization Among the Elderly

The ENP was designed to address the psychological and sociological needs of the elderly as well as their nutritional needs. Studies that have compared socialization among ENP participants and nonparticipants have used two different approaches. Two studies classified respondents based on a five-point isolation index: (1) living alone, (2) reporting having too few friends, (3) having no one to confide in, (4) having children that do not visit them, and (5) feeling lonely more often. A third study defined socialization based on number of social contacts (with relatives and/or friends) per month.

Other Measures of Nutrition and Health Status

While the majority of studies of the impact of FANPs on nutrition- and health-related outcomes have focused on food expenditures, household nutrient availability, and/or individual dietary intake, some studies have examined impacts on longer term measures of nutrition and health status. The most studied outcome in this group is birthweight (and related measures). Others include measures of food sufficiency/security/insecurity, nutritional biochemistries, linear growth in children, body weight, and school/academic performance.

Birthweight and Related Measures

Impacts of FANP participation on birthweight—perhaps the most fundamental measure of nutrition and health status in infants—has focused almost exclusively on the WIC program. This is an obvious and appropriate focus, given that one of the issues WIC was specifically designed to address is birth outcomes among low-income pregnant women. Note, however, that birthweight reflects multiple influences exerted both before and during a pregnancy. These include, but are not limited to, maternal health and nutrition, intrauterine exposures (tobacco, drugs, alcohol), and genetic factors.

Compared with the measures discussed in the previous sections, reliable and complete data on birthweight, which is routinely measured at birth and recorded on the birth certificate, is easy to collect. However, proper interpretation of data on birthweight depends on relating birthweight to the expected weight for the infant's gestational age (duration of pregnancy). Infants who are below the expected weight are classified as having intrauterine growth retardation (IUGR). IUGR infants are at increased risk for adverse birth outcomes, compared with those of low birthweight whose weight is appropriate for their gestational age. Infants born at full term (39+ weeks) with a birth weight of less than 2,500 gm (5.5 pounds) are classified as IUGR.

Another issue that affects interpretation of data on birthweight is the simultaneity of WIC participation and gestational age. Women who deliver early have less chance of enrolling in WIC than women who go to term. Consequently, both the decision to participate in WIC and the length of participation are inexorably linked with gestational age, an important predictor of most birth outcomes. Moreover, women who enroll late in pregnancy will automatically have better outcomes than other women by virtue of their increased gestation. This simultaneity means that assessments of the impact of WIC on birthweight that rely on a binary indicator of participation are likely to overstate the impact of the program. Moreover, because the duration of WIC participation is also simultaneous with gestational age, a traditional dose-response approach employed by several studies—estimating WIC impacts based on number of months of WIC participation—is not a viable solution to the problem.

Gordon and Nelson (1995) studied several approaches to addressing the relationship between the timing of WIC enrollment and gestational age. These approaches included omitting very late WIC enrollees (enrolled after the eighth month), including gestational age as an independent variable in the birthweight regression, and defining several cohorts of WIC participants by gestational age (pregnancy duration) at the time of WIC enrollment. The authors found, however, that these approaches systematically underestimated the impact of WIC and suggested that results from analyses using a binary indicator (participant vs. nonparticipant) and results of analyses that compare various cohorts of WIC participants (e.g., early vs. late enrollees) bound the likely magnitude of the effect.

Food Security

In 1997, USDA released the 18-item Federal food security module, the currently accepted standard for measuring household and individual food security (Price et al., 1997; Bickel et al., 2000). Studies completed before 1997 used one or more of the questions included in the early food security assessment work done by Wehler et al. (1991) and by Radimer and her colleagues at Cornell University (1992). Studies completed after 1997 used either the early questions or the 18-item module.

Nutritional Biochemistries

Several studies have examined the impact of FANP participation on blood levels of key nutrients. The nutrient studied most often is iron. Iron deficiency is the most common known form of nutritional deficiency, affecting the entire age span from infancy to old age. In infancy and early childhood, iron deficiency is an especially important problem that may be associated with anemia as well as with delayed psychomotor development (de Andraca et al., 1997).

Impacts on nutritional biochemistries are best assessed using a design that compares participants (and potentially nonparticipants) before and after FANP participation. As described earlier, Yip and his colleagues (1987) conducted a widely recognized study of the impact of WIC on the prevalence of anemia among young children, using a classic “participants, before vs. after” design.

Studies that rely on single measures of iron status (or other nutritional biochemistries) are subject to significant selection bias, particularly WIC studies because low blood levels of iron and other nutrients are used to define eligibility for WIC participation.

Linear Growth in Children

One of the most fundamental measures of health status in preschool children is the attainment of normal growth. Failure to attain normal linear growth (stunting) is a highly sensitive indicator of underlying nutritional deficits or other health problems. Height-for-age is used to assess the adequacy of linear growth, relative to growth curves established by the Center for Disease Control and Prevention (CDC) (Kuczmarski et al., 2002). Height-for-age below the fifth percentile is indicative of growth retardation (HHS, 2000).

Similar to nutritional biochemistries, proper assessment of the impact of FANP participation on measures of linear growth in children requires at least two measurements, ideally collected for both treatment and control groups (World Health Organization, 1995). For example, children of Asian descent, many of whom came into the United States as refugees in the late 1970s and early 1980s, had an increased prevalence of growth stunting relative to other children in the WIC program. Over time, coincident with participation in WIC, the prevalence of stunting decreased significantly to levels approaching those of other low-income children served by WIC (Yip et al., 1993).

Body Weight

The substantial increase in the prevalence of overweight and obesity in the United States over the past several decades has heightened interest in this aspect of nutritional status among low-income Americans. Few studies have attempted to estimate the impact of FANP participation on this indicator, and none has studied the issue adequately. Development of overweight and obesity is a complex process that takes place over a long period and is influenced by a number of factors other than dietary intake, including levels of physical activity/inactivity and genetics. Moreover, low-income and food-insecure individuals are more likely to be overweight or obese than higher income and food-secure individuals. This confounding makes it difficult to assess relationships between these characteristics using cross-sectional data.

For adults, overweight and obesity are defined based on body mass index (BMI), a measure of the relationship between height and weight that is commonly accepted for classifying adiposity (or fatness) in adults (CDC, 2003).[17] For adults, a healthy weight is defined as a BMI of at least 18.5 but less than 25. Overweight is defined as a BMI between 25 and 30, and obesity as a BMI of 30 or more. A BMI of less than 18.5 indicates extreme thinness or underweight.

Classifying children as overweight is fundamentally different from classifying adults (Cole, 2001). Adults have traditionally been classified as overweight based on life insurance mortality data and data relating weight status to morbidity and mortality (Troiano and Flegal, 1998). These criteria cannot be used to define overweight in childhood, however, because childhood mortality is not associated with weight, and weight-related morbidity in childhood is too infrequent to define meaningful cutoffs (Cole, 2001). Therefore, children are classified as overweight by comparing their weights and heights with appropriate reference populations. For children, overweight is defined as a BMI at or above the 95th percentile on CDC growth charts, which define BMI percentile distributions by age and gender (CDC, 2003). Children with BMIs between the 85th and 95th percentiles are considered to be at risk of overweight.

[17]BMI is equal to [weight in kilograms] / [height in meters] ^{2}.

School Performance

A relatively recent body of research has examined impacts of breakfast consumption on school performance. Virtually all of these studies have evaluated the issue within the context of demonstration projects of “universal free” school breakfast programs—that made breakfast available to all students free of charge, regardless of household income. Measures examined include attendance and tardiness, academic achievement—generally measured with standardized test scores—cognitive functioning, student behavior, and referrals to school nurses.

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Chapter 3

Food Stamp Program

The Food Stamp Program (FSP) stands at the intersection of two sets of Federal programs: those for whom the primary goal is improving access to adequate nutrition and those for whom it is income maintenance. The FSP is particularly important because of its universality; it is an entitlement program with eligibility requirements based almost solely on financial need, while the other major food and nutrition assistance programs (FANPs) are targeted toward certain types of individuals or households. Food stamp benefits are distributed as electronic transfers with an explicit cash value, which can be used only to purchase food for home consumption.

The FSP is the cornerstone of the Nation's nutrition safety net. In FY 2002, the total Federal expenditure for the FSP was $20.7 billion, or about 54 percent of the $38 billion Federal expenditure for FANPs. The program served more than 19 million participants per month (U.S. Department of Agriculture (USDA), Food and Nutrition Service (FNS), 2003a).

Program Overview

The goal of the FSP is to "safeguard the health and well-being of the Nation's population by raising the level of nutrition among low-income individuals." To achieve this objective, the FSP provides electronic benefits that can be used at most retail grocery stores.[18]

The FSP began as a small pilot program in 1961.[19] The program expanded during the 1960s and early 1970s, finally reaching nationwide coverage in 1975. The FSP specifies the household rather than any individual living in the household as the program participant. A household includes all people living together in a dwelling who normally purchase food and prepare meals as a unit. Eligibility is based on the pooled income, resources, and expenditures of all members of the household. Elderly and disabled individuals who cannot prepare and purchase food because of a substantial disability may apply as a separate household, as long as the pooled income of the remainder of the household is less than 165 percent of poverty. Monthly benefit levels increase with the number of people in the household but not at a flat rate per person.

Program Eligibility

To be eligible for the FSP, a household must meet certain financial, work-related, and categorical requirements. Financial requirements include a *gross income limit* of 130 percent of poverty, a *net income limit* (gross income less allowable deductions) of 100 percent of poverty, and a *countable assets limit* of $2,000. Households with elderly or disabled members are not subject to the gross income limit, are eligible for deductions for medical expenses and increased deductions for shelter costs, and have a countable assets limit of $3,000. Households in which all members receive Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), or general assistance are exempt from both income and asset tests.

Work-related eligibility conditions require certain household members to register for work, accept suitable job offers, and comply with State welfare agency work or training programs. Finally, a few groups are categorically ineligible for the FSP, including strikers, most people who are not citizens or permanent residents, postsecondary students, and people living in institutional settings.

Program Participation

Because the FSP is available to most people who meet income and resource standards, the households that participate in the program are quite diverse and represent a broad spectrum of the needy population (Rosso, 2003). In FY 2001, almost all FSP participants lived in poverty. The gross monthly income of 89 percent of FSP households was less than or equal to 100 percent of the poverty guideline. More than half of all FSP households had incomes that were less than or equal to 75 percent of the poverty guideline, and one-third had incomes that were less than or equal to 50 percent of the poverty guideline (Rosso, 2003).

Administrative data for FY 2001 (Rosso, 2003; Tuttle, 2002) indicated that the vast majority (88 percent) of FSP households included either a child, an elderly person (60 or older), or a disabled person. More than half

[18]FSP benefits can be used only to purchase food or seeds and plants used to produce food.

[19]An earlier version of the FSP, which distributed surplus commodities to needy families, came to an end in 1943. For a detailed description of the program and its history, see, for example, Ohls and Beebout (1993).

(54 percent) of all FSP households had children. Of these, more than two-thirds (67 percent) were single-parent households. Twenty percent of FSP households included one or more elderly individuals. The majority (80 percent) of these households were elderly individuals living alone. More than a quarter (28 percent) of all FSP households included a disabled individual, and 58 percent of these households were disabled people living alone. Overall, 51 percent of all FSP participants in FY 2001 were children, 10 percent were elderly, and 13 percent were disabled.

Participation in the FSP has changed dramatically in recent years. The number of participants increased by about 47 percent between 1989 and 1994 (from 18.9 million in 1989 to a record high of 28.0 million in March 1994) (Tuttle, 2002). After that, participation declined steadily through 2000. Between 1994 and 2000, the number of individuals participating in the FSP decreased from 28.0 million to 16.9 million, or by 40 percent (Tuttle, 2002). Between 2000 and 2001, participation increased for the first time in 6 years, by approximately 1 million people, or 6 percent.

A number of investigators have studied the shifts in FSP participation, particularly the unprecedented decline in the mid- to late 1990s. (See, for example, USDA/FNS, 2001; Jacobsen et al., 2001; Figlio et al., 2000; Wilde et al., 2000a, 2000b; Wallace and Blank, 1999.) There is strong evidence that economic conditions played a role in the shifts seen in FSP participation levels over the past 10 to 15 years. The dramatic increase in participation in the early 1990s went hand-in-hand with a declining economy (Tuttle, 2002). Similarly, the drop in participation between 1994 and 2000 was consistent with an improving economy. The recent upswing in participation may be associated with the latest economic downturn.

The relationship between FSP participation and economic indicators does not tell the whole story, however. FSP participation and unemployment rates diverge at some points in time, indicating that factors other than the economy have been in play (Wilde, 2001). Key changes in program policies and regulations may also have contributed to fluctuating FSP rolls, although it is generally believed that the impact of program policies is substantially less than that of economic conditions. The most notable policy changes in recent years include reforms enacted in 1996 as part of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). These changes restricted program participation for resident aliens and other subgroups and placed strict limits on participation for "able-bodied adults without dependents" (ABAWDs). (Eligibility restrictions for some resident aliens and several other groups were rescinded in 1998.) Since the PRWORA reforms, participation in the Aid to Families With Dependent Children (AFDC)/TANF programs has decreased dramatically, and such families are accounting for a decreasing share of all FSP households.[20] Between 1995 and 2001, TANF-recipient households fell from 38 percent to 26 percent of all FSP households (Rosso, 2003).

While economic factors and program policies explain a substantial portion of the decline in FSP participation, other factors clearly were also involved. From the mid- to late 1990s, FSP participation declined not only because fewer individuals were eligible, but also because of a noteworthy drop in the percentage of eligible individuals who actually elected to participate. Indeed, the rate of FSP participation among income-eligible people declined from 75 percent in 1994 to 58 percent in 1999 (Cunnyngham, 2002). Factors that may have contributed to this decline include confusion about eligibility, erroneous termination of FSP benefits when TANF cases terminated, effects of TANF diversion programs on the FSP application process, and shortening of FSP certification periods (Kornfeld, 2002). In 2000, FSP participation rates increased slightly for the first time in 5 years, from 58 to 59 percent (Cunnyngham, 2002).

Program Benefits

Food stamp benefits per household are determined by a schedule of maximum benefits per household size. Individual households receive the maximum benefit less 30 percent of the household's net income (households are expected to set aside 30 percent of their non-food stamp disposable income for food). Benefit levels are based on the Thrifty Food Plan, an estimate of what it costs for a household of a given size to purchase the foods required for a nutritious diet. USDA annually determines the cost of the Thrifty Food Plan. Maximum monthly food stamp allotments for FY 2003, before deductions, are shown in table 7.

A key feature of the program before 1979 was the *purchase requirement*. The benefit allotment for households of a given size had a fixed value. Participating households paid cash for their allotment, with the payment amount depending on household income. The

[20]Under PRWORA, the AFDC program was replaced by TANF.

difference between the amount paid and the value of food stamps received was termed the "bonus." The purchase requirement was eliminated in 1979. Subsequently, eligible households simply received what had previously been the bonus amount of coupons.

The FSP originally issued benefits in the form of paper coupons of various denominations. Recipients redeemed these coupons for food at authorized stores. After a series of demonstration projects, FNS authorized States to use electronic benefits transfer (EBT) systems in place of paper coupons. In an EBT system, the recipient receives a credit on a computerized account for the amount of the monthly benefit. To make a purchase, the recipient presents an EBT card and enters a personal identification number (PIN) on a point-of-sale (POS) terminal. The terminal verifies the amount of benefits available, debits the amount of the purchase from the recipient's balance, and records a credit for the retailer. The retailer receives daily an electronic bank deposit for the net amount of FSP redemptions.

Nearly all States use online EBT systems, in which the POS terminal communicates with a central computer to obtain authorization for each transaction. These online EBT systems use the same technology, and often the same POS equipment, as commercial debit and credit payment systems. Ohio and Wyoming use offline EBT systems, in which a computer chip on the card maintains the recipient's balance and authorizes the transaction.

PRWORA mandated that all FSP benefits be distributed via electronic transfers. The nationwide changeover from coupons to EBT was completed in June 2004 (USDA, 2004).

Table 7—Maximum monthly food stamp benefits before deductions, FY 2003

Number in household	Maximum monthly benefit
	Dollars
1	141
2	259
3	371
4	471
5	560
6	672
7	743
8	849
Each additional person	+106

Nutrition Education

Nutrition education is a relatively recent, though increasing, emphasis in the FSP. In FY 1998, FNS made a "renewed commitment to nutrition education" in the FSP (and all FANPs) and established a special staff within the agency to "refocus efforts toward nutrition and nutrition education" (USDA/FNS, 2003b). The focus on nutrition education as an adjunct to the economic benefits provided by the FSP reflects an important shift in the overarching mission and objectives of the program. As stated in FNS's strategic plan for 2000-05, there is a "growing awareness that making sure people have enough food is not enough; people must have the knowledge and motivation to make food choices that promote health and prevent disease" (USDA/FNS, 2000).

This growing awareness is based on accumulated scientific evidence that dietary patterns are associated with 4 of the 10 leading causes of death—coronary heart disease, certain types of cancer, stroke, and diabetes—and with the development of obesity and hypertension (both of which contribute to these and other chronic diseases) (Frazao, 1999). In addition, diet plays an important role in several other health conditions, including osteoporosis, iron-deficiency anemia, and neural-tube birth defects. Most important, low-income individuals, the target population for the FANPs, are at increased risk of developing many of these health problems (U.S. Department of Health and Human Services (HHS), 2000).

The goal of food stamp nutrition education is to promote healthy food choices and active lifestyles among FSP participants. Four core elements have been defined for nutrition education efforts: dietary quality, food security, food safety, and shopping behavior/food resource management. Although nutrition education is still a very small part of the overall program (less than 1 percent of total program expenditures in FY 2002), efforts in this area have increased substantially in the past decade. In FY 1992, only five States applied for and received optional funding for nutrition education activities in the FSP, and the Federal share of the expenditure for these activities was $661,000. In FY 2002, 48 States had approved nutrition education plans, and Federal expenditures for FSP nutrition education exceeded $174 million (USDA/FNS, 2003b). Most of this increase occurred after FY 1998, when FNS made a renewed commitment to nutrition education in the FSP. Virtually all of the research discussed in this chapter was conducted before the increased, and still growing, focus on nutrition education in the FSP.

Recent Legislative Changes

The FSP has been legislatively revised several times since its inception, but the basic nature of the benefit and the eligible population have remained relatively stable. As mentioned, the PRWORA legislation of 1996 placed a time limit on benefits for ABAWDs. ABAWDS can receive benefits for only 3 months in a 36-month period unless they are working or are participating in certain types of qualified work experience or workforce programs. States can get approval to exempt ABAWDs from work requirements in designated geographic areas, however, and the legislation provides for other types of exemptions. In addition, PRWORA made most legal immigrants ineligible for the FSP, but such households accounted for only a small percentage of all recipients, and later legislation in 1998 restored benefits to many of them. Other changes include the introduction and expansion of employment-related requirements for various types of households and the replacement of food stamp coupons with electronic benefit transfers.

More recently, the Food Stamp Reauthorization Act of 2002 included several provisions to improve access to the FSP and simplify program administration. The 2002 Act removed the prohibition on benefits for several categories of legally resident aliens, including children, elderly or disabled people, and others legally residing for 5 years. To make benefits more responsive to household circumstances, the 2002 Act modified the standard deduction applied to income when determining benefits, so that the deduction is scaled to family size and indexed to inflation. The 2002 Act also authorized a transitional benefit alternative (TBA) for households leaving TANF and wider use of semiannual income reporting. Several provisions of the act give States more flexibility and encourage efforts to promote FSP access. Most notably, the act lowered the standards for benefit accuracy, replacing the system of enhanced matching tied to payment accuracy with bonuses for a broader range of performance objectives. Finally, the 2002 Act repealed the requirement of PRWORA that EBT systems be cost-neutral (that is, no more expensive than the inflation-adjusted cost of paper coupon issuance).

Assessing Impacts of the Food Stamp Program

FSP benefits are expected to directly affect household *food expenditures*. By increasing food expenditures, the FSP is expected to increase the *nutrients available to participating households*, and therefore the *nutrient intake of individuals in those households*. Through this path, the FSP may improve other nutrition and health outcomes, such as *food security*, *birthweight*, and *iron status*.

This chapter summarizes existing research on the impact of the FSP in each of these areas. Three basic approaches have been used to assess FSP impacts on nutrition- and health-related outcomes:

- Participant vs. nonparticipant designs that compare mean outcomes.
- Dose-response analysis of the effect of the FSP per dollar of benefits.
- Cashout demonstrations that estimate the impact of a single component of the FSP (the use of coupons) to obtain lower-bound estimates of impacts.

As described in chapter 2, dose-response analysis is a variant of the “classic” participant vs. nonparticipant design. Each of these research approaches, and their relative strengths and weaknesses, is now discussed.

Participant vs. Nonparticipant Comparisons

Several studies have estimated impacts of the FSP by comparing outcomes for FSP participants and nonparticipants. These studies generally (but not always) compared FSP participants and FSP-eligible nonparticipants, so that the comparison was limited to people with similar incomes. The comparison is done with multivariate analysis to control for the characteristics of FSP participants and nonparticipants. An indicator of FSP participation captures the direct impact of the FSP—that is, the difference in outcomes between FSP participants and nonparticipants that is unexplained by other characteristics.

Comparisons between FSP participants and income-eligible nonparticipants yield direct estimates of the impacts of the FSP. As discussed in chapter 1, however, such estimates are subject to selection-bias problems because unmeasured characteristics of FSP participants may be correlated with both FSP participation and the outcomes of interest. For example, households choosing to participate in the FSP may give food expenditures higher priority (compared with households choosing not to participate) even in the absence of the program. In this case, participant vs. nonparticipant comparisons would overstate the impact of the FSP, attributing higher food expenditures to FSP participation when, in fact, households participating in FSP have higher food expenditures even in the absence of the program. Conversely, participant vs. nonparticipant comparisons could understate the impact of the FSP if FSP households are especially needy in unmeasured ways that are unrelated to food (for example, high medical expenses). Such households, in the absence of the FSP, would spend less on food than otherwise-similar nonparticipant households.

Several studies, including most of the more recent ones, have used econometric techniques to attempt to control for selection bias in estimating program impacts. The standard approach is to identify and control for variables (instruments) that affect FSP participation but do not affect the outcomes of interest. However, most FSP studies rely on national survey data that have a limited number of potentially useful variables. Moreover, these methods provide no guarantee that bias has actually been eliminated, and few valid instruments have been identified in the literature.

Dose-Response Analysis

A key feature of the FSP is that the benefit varies across participating households according to estimated need (based on the cost of the Thrifty Food Plan for a given household size and income, minus various exclusions and deductions). The benefit received by a household can be as little as $10 or, in FY 2002 for an eight-person household, as much as $838. Benefits can vary among households of the same size because of differences in total income, in whether income is earned or unearned, and in deductions for housing, child care, and medical expenses.

Several researchers have taken advantage of the variation in FSP benefit amounts and used dose-response analysis to identify the marginal impact of FSP benefits. Dose-response studies generally estimate the impact of the FSP based on variations in benefits and impacts among participants only, ignoring nonparticipants entirely. The overall impact of the FSP is estimated as the impact *per dollar* of FSP benefits multiplied by the average FSP benefit. This approach arguably removes a major source of selection bias because the implicit comparison group is households that have chosen to participate in the FSP but are receiving zero benefits, rather than nonparticipants. Alternatively, nonparticipants may be included in the analysis (with zero benefits). In this case, the coefficient on the FSP participation indicator, if included in the model, indicates the presence of selection bias.[21]

Dose-response analysis is not, however, a panacea. First, functional form is crucial. Because no FSP participants actually receive zero benefits, this approach relies on the researcher's ability to extrapolate the relationship from very low observed benefit levels down to zero. As will be seen later in this chapter, alternative functional form assumptions can lead to different estimates of FSP impacts.

Second, some selection bias may remain because those households that choose to participate when the "dose" is low—that is, households that receive only a small FSP benefit—may be unlike households that participate in order to receive a large benefit. This difference seems a less serious matter, however, than the potential differences between participants and nonparticipants.

Similarly, unmeasured household characteristics likely affect both the FSP benefit and food expenditures (as well as other outcomes). When households that have the same measured characteristics but different FSP benefits are compared, one is tempted to think of the comparison as an experiment in which Household A, which is essentially similar to Household B, receives more food stamps and spends some amount more on food as a consequence. However, if the reason Household A is getting more food stamps than Household B is that Household A is receiving an excess shelter cost deduction while Household B is living in a rent-free situation, one cannot expect outcomes absent the FSP to be the same for both households.

Despite these caveats, dose-response analysis holds promise for assessing the impact of the FSP. While this approach is not as strong as randomly assigning FSP benefits to households, dose-response analysis is stronger than participant vs. nonparticipant comparisons because it is less subject to (although not free from) selection-bias problems.

[21]Selection bias may be said to occur if the expected value of the outcome absent the FSP, conditional on the other variables in the model, is different for FSP participants than for nonparticipants. Omitting an indicator of FSP participation from the specification when it should be present (i.e., when outcomes would be different even in the absence of the program) subjects the coefficient on the FSP benefit amount to an omitted-variables bias that is proportional to the true coefficient on FSP participation.

Cashout Demonstrations

The FSP provides to eligible households monthly cash value benefits, which can be spent only for food. In the cashout demonstrations, participating households were given checks rather than food stamp coupons, eliminating the restriction that benefits can be spent only for food. Impacts of cashout can be interpreted as lower-bound estimates of the FSP impact, corresponding to the effects of just one program component—namely, the earmarking of benefits.[22]

Lower-bound estimates would not be particularly useful, given the many available estimates of the impacts of the FSP as a whole, except that two of the cashout demonstrations were randomized experiments. If these studies find that coupon recipients spend significantly more on food than cash-benefit recipients, the conclusion (without fear of selection bias) is that the FSP does affect food expenditures. Moreover, if the measured difference is, say, $0.20 per dollar of benefits, the conclusion is that the effect of food stamp coupons on household food expenditures is *at least* $0.20 on the dollar—and, in fact, that it is at least $0.20 more on the dollar than the presumably positive effect on food expenditures of ordinary income. Similarly, the effect of cashout on household nutrient availability, as measured in the two randomized experiments, may represent the effect of the FSP in general.

Food Expenditures

The FSP is virtually certain to result in increased food purchases, if for no other reason than that the program increases participating households' incomes and the income elasticity for food is positive. That is, increasing a household's income by $1,000 per year would always be expected to increase its food expenditures by some fraction of that amount.

Economists have debated whether giving households coupons that must be spent on food consumed at home is more effective at increasing food expenditures than simply giving them a non-earmarked income supplement. (See, for example, Southworth, 1945; Senauer and Young, 1986; Moffitt, 1989.) A simple theory of rational behavior implies that coupons should have the same effect as cash because households can use the coupons to free up the money they would otherwise have spent on groceries. Nonetheless, a substantial body of evidence shows that coupons are more effective than cash in increasing food expenditures. This idea is often expressed in terms of the *marginal propensity to spend on food*, or MPS_F.[23] This quantity represents the increase in food expenditures per dollar increase in income. The MPS_F has been found to vary between different types of income, being higher for food stamps than for other sources. Explanations for this difference are as follows:

- For some households, the amount of the benefit is greater than desired food expenditures. These households are "constrained" because they are unable to spend food stamp benefits on nonfood items, MPS_F=1.
- In multiple-adult households, food stamps are under the control of the "food manager" in the household, while a cash benefit can be co-opted by other adults to purchase other items.[24]
- When food stamp benefits are received as a lump sum at the beginning of the month, the household has many urgent and competing needs. The food stamps can be used only for food, and so are promptly spent for food.[25] An equivalent cash benefit received at the beginning of the month, in contrast, might be spent in part on other things, such as health insurance or rent. As the month proceeds, the household cannot go without food altogether, so more non-food-stamp income is allocated for this purpose, even though the household spent heavily on food at the beginning of the month.
- Because food stamps are a steady and reliable income source for low-income households, they are treated as "permanent income."[26] Hence, they have more power

[22]The households still may have treated these benefits as lightly earmarked because they were formally identified as a food assistance benefit. If so, the cashout impacts are an even lower underestimate of the total impact of the FSP.

[23]Some authors use the notation MPC_F (marginal propensity to consume food). This refers not to the consuming (or eating) of food, but to households allocating their income to *consumption goods* of various kinds instead of to savings. To avoid any confusion, the MPS_F notation is used here.

[24]This explanation was tested using data from the San Diego cashout experiment by comparing impacts between one- and multiple-adult households (Breunig et al., 2001). The "food manager" hypothesis would suggest that cashout would reduce food expenditures by a greater amount in multiple-adult households, which was indeed found to be the case. The authors remark that although the household as a whole is unconstrained in its food expenditures, one of the adults may be constrained if he or she does not spend anything on food. Giving the household cash instead of food stamps leads to the constrained adult's controlling a greater fraction of the household's resources.

[25]A study in Reading, PA, found that food stamp recipients using electronic benefits transfers spent 19 percent of their monthly benefits on the day of issuance and 70 percent within the first week (Bartlett and Hart, 1987). Quite similarly, a more recent study in Maryland found that recipients spent 23 percent of their benefits on the day of disbursement and 71 percent within the first week (Cole, 1997).

[26]Permanent income refers to normal or expected income over a long period of time. Current income is the sum of permanent income and (positive or negative) transitory income (see Friedman, 1957).

to affect routine and nonpostponable expenditures like food than do income sources that fluctuate greatly.

- Finally, the psychological effect of earmarked benefits cannot be ignored. It seems to be human nature not to treat food stamps in the same way as cash. When constrained to spend a certain minimum amount on food, even if the constraint is not binding, households evidently end up allocating more of their budget to food.[27]

Research Overview

Since the mid-1970s, dozens of researchers have investigated the impact of the FSP on household food expenditures. The literature search identified 32 such studies completed since 1973. Key characteristics of these studies are summarized in table 8. Studies have been classified by the three alternative research approaches discussed above: participant vs. nonparticipant comparisons (Group I), dose-response estimates of the MPS_F (Group II), and cashout demonstrations (Group III). Participant vs. nonparticipant and dose-response studies are further subdivided by data source (national survey data or State and local studies). Cashout studies are separated on the basis of design (randomized experiment ("pure" cashout) or quasi-experiment).

Of the 32 studies, 7 used participant vs. nonparticipant comparisons to estimate the impact of the FSP on household food expenditures, 20 used dose-response analyses to estimate the marginal impact of FSP benefits, and 5 estimated impacts of food stamp cashout.[28] In addition to varying in the basic research approach, these studies varied with respect to data source, definition and measurement of food expenditures, and model specification. With just a few exceptions (Kisker and Devaney, 1988; Lane, 1978), researchers used some form of multivariate modeling in their analysis.

Five of the seven participant vs. nonparticipant studies are based on secondary analyses of data collected in national surveys, including the Nationwide Food Consumption Survey (NFCS) and the Bureau of Labor Statistics' Consumer Expenditure Survey (BLS-CES). The other two studies that used participant vs. nonparticipant comparisons are based on State and local data. Fifteen of the 20 dose-response studies used national survey data and 5 used State and local data. Finally, two of the five studies of food stamp cashout are based on cashout demonstrations that used experimental designs (in Alabama and San Diego) and three are based on demonstrations that used quasi-experimental designs (in Washington State, Alabama, and Puerto Rico).

Most studies of the impact of the FSP on food expenditures measured household food expenditures as expenditures for foods used at home, although some studies also examined impacts on total food expenditures (food used at home and away from home). Food stamp benefits can be applied only toward food used at home, but several authors who examined both measures concluded that FSP participation induces households to substitute food at home for food away from home.

Some studies defined food expenditures as food purchases during a specified period, while others also included the value of nonpurchased food. A small number of studies measured food expenditures as food actually used during a particular period.

The bulk of the impact estimates are derived from models of the form:

$$FOOD\_EXP = b_0 + b_1\ FSP + b_2\ BENEFIT + b_3\ OTHER\ INC + b_4\ X + u$$

Where:

FOOD_EXP is household expenditure on food;

FSP is an indicator of participation in the Food Stamp Program;

BENEFIT is the size of the food stamp benefit (zero for nonparticipants);

OTHER INC is the amount of other income available to the household; and

X is a vector of household characteristics.

Three main variations on this model have been used: Models may include FSP, BENEFIT, or both. Four of the seven participant vs. nonparticipant studies estimated models with FSP but not BENEFIT, and three of these studies included both FSP and BENEFIT. Half

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[27] A classic example of the effect of earmarking is the difference in behavior between a person who loses a $40 concert ticket and a person who loses $40 en route to buying a concert ticket. The loss of the ticket (earmarked) is much more likely to result in the person's forgoing the concert than is the loss of the money. (This example is taken from Amos Tversky, a cognitive psychologist who studied human-choice behavior and the limits of the rational choice model.) Similarly, a recipient whose food stamp benefit is cut by $40 is likely to curtail food expenditures more than one whose cash assistance is curtailed by $40.

[28] Three of the studies in Group I also include dose-response estimates. These studies have not been double-counted as part of the 20.

Table 8—Studies that examined the impact of the Food Stamp Program on household food expenditures

Study	Data source[1]	Measure of expenditures[2]	Population (sample size)	Design	Measure of participation	Analysis method
Group IA: Participant vs. nonparticipant comparisons—Secondary analysis of national surveys						
Hama and Chern (1988)	1977-78 NFCS elderly supplement	At-home Nonpurchased food included Per person per week	FSP-eligible households with elderly members (n=1,454)	Participant vs. nonparticipant	Participation dummy	Simultaneous food expenditure/nutrient availability equation[3]
Kisker and Devaney (1988)	1979-80 NFCS-LI	At-home Nonpurchased food included Per ENU per week	FSP-eligible households (n~2,900)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Basiotis et al. (1983)	1977-78 NFCS-LI	At-home Nonpurchased food included Per household per week	FSP-eligible households (n=3,562)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Price (1983)	1973-74 BLS-CES	At-home Purchased food only Per equivalent adult per week	All households (n=10,359)	Participant vs. nonparticipant; also dose-response	Participation dummy; benefit amount	Multivariate regression
Salathe (1980)	1973-74 BLS-CES	At-home, away, total Purchased food only Per person per week	FSP-eligible households (n=2,254)	Participant vs. nonparticipant; also dose-response	Participation dummy; benefit amount	Multivariate regression
Group IB: Participant vs. nonparticipant comparisons—State and local studies						
Lane (1978)	Kern County, CA (1972-73)	At-home Nonpurchased food included Per person per month	FSP-eligible households (n=329)	Participant vs. nonparticipant	Participation dummy	Bivariate comparisons based on proportion of income spent on food
West et al. (1978)	Washington State (1972-73)	At-home Nonpurchased food included Per equivalent adult per month	FSP-eligible households with child age 8-12 (n=332)	Participant vs. nonparticipant; also dose-response[4]	Participation dummy; bonus amount	Weighted multivariate regression

See notes at end of table.

Continued—

Table 8—Studies that examined the impact of the Food Stamp Program on household food expenditures—Continued

Study	Data source[1]	Measure of expenditures[2]	Population (sample size)	Design	Measure of participation	Analysis method
Group II A: Dose-response estimates—Secondary analysis of national surveys						
Kramer-LeBlanc et al. (1997)	1989-91 CSFII	At-home, total Purchased food only Per household per week	FSP participant households (n=790)	Dose-response	Benefit amount	Multivariate regression
Levedahl (1991)	1979-80 NFCS-LI	At-home, total Purchased food only	FSP participants who used all their food stamps (n=1,210)	Dose-response	Bonus value	Multivariate regression
Fraker et al. (1990)	1985 CSFII	Expenditures on food during previous 2 months	FSP- and WIC-eligible households (n=515)	Dose-response	Participation dummy; benefit amount	Multivariate regression
Devaney and Fraker (1989)	1977-78 NFCS-LI	Aided recall of food used in last 7 days	FSP-eligible households (n=4,473)	Dose-response	Participation dummy; bonus value	Multivariate regression
Basiotis et al. (1987)	1977-78 NFCS-LI	At-home Nonpurchased food included Per household per week	FSP-eligible households (n~3,000)	Dose-response	Participation dummy; bonus value	Simultaneous equations for food cost/nutrient availability/nutrient intake relationship
Senauer and Young (1986)	1978 PSID	At-home Purchased food only Per household per month	FSP participant households (n=573)	Dose-response	Bonus value	Multivariate regression
Smallwood and Blaylock (1985)	1977-78 NFSC-LI	At-home Purchased food only Per person per week	FSP-eligible households (n=3,582)	Dose-response	Participation dummy; expected weekly bonus value	2-equation selection-bias model
West (1984)	1973-74 BLS-CES	At-home, away, total Purchased food only Per equivalent adult per week	FSP-eligible households (n=2,407)	Dose-response	Participation dummy; bonus value	Multivariate regression
Allen and Gadson (1983)	1977-78 NFCS-LI	At home, away, total Purchased food only Per household per week	FSP-eligible households (n=3,850)	Dose-response	Bonus value	Multivariate regression
Chen (1983)	1977-78 NFCS-LI	Aided recall of food used in last 7 days	FSP participant households (n=1,809)	Dose-response	Participation dummy; bonus value	Multivariate regression

See notes at end of table.

Continued—

Table 8—Studies that examined the impact of the Food Stamp Program on household food expenditures—Continued

Study	Data source[1]	Measure of expenditures[2]	Population (sample size)	Design	Measure of participation	Analysis method
Brown et al. (1982)	1977-78 NFCS-LI	Aided recall of food used in last 7 days	FSP participant households (n=911)	Dose-response	Bonus value	Multivariate regression
Chavas and Yeung (1982)	1972-73 BLS-CES	At-home Purchased food only Per household per week	FSP-eligible households, southern region (n=659)	Dose-response	Bonus value	Seemingly unrelated regression model, interactions between bonus value and demographic variables[5]
Johnson et al. (1981)	1977-78 NFCS-LI	At-home Nonpurchased food included Per household per week	Low-income households (n=4,535)	Dose-response	Participation dummy; bonus value	Multivariate regression
Benus et al. (1976)	1968-72 PSID	Annual expenditures for food used at home	All households (n~3,300)	Dose-response	Participation dummy; bonus value	Dynamic adjustment model
Hymans and Shapiro (1976)	1968-72 PSID	Annual expenditures for food used at home	All households (n~3,300)	Dose-response	Participation dummy; bonus value	Multivariate regression
Group IIB: Dose-response estimates—State and local studies						
Breunig et al. (2001)	San Diego cashout demonstration (1990)	At-home Purchased food only Per person per month	FSP participant households receiving coupons (n=487)	Dose-response	Benefit amount	Multivariate regression
Levedahl (1995)	San Diego cashout demonstration (1990)	At-home Purchased food only Per person per month	FSP participant households receiving coupons (n=494)	Dose-response	Benefit amount	Multivariate regression
Ranney and Kushman (1987)	Counties and county groups in California, Indiana, Ohio, Virginia (1979-89)	At-home Nonpurchased food included	FSP-eligible households (n=896)	Dose-response	Participation dummy; bonus value	Multivariate regression
Neenan and Davis (1977)	Polk County, FL (1976)	At-home Purchased food only Per household per month	FSP participant households (n=123)	Dose-response	Participation dummy	Multivariate regression

See notes at end of table.

Continued—

Table 8—Studies that examined the impact of the Food Stamp Program on household food expenditures—Continued

Study	Data source[1]	Measure of expenditures[2]	Population (sample size)	Design	Measure of participation	Analysis method
West and Price (1976)	Washington State (1972-73)	At-home Nonpurchased food included Per equivalent adult per month	Households with children ages 8-12[6] (n=995)	Dose-response	Bonus value	Multivariate regression
Group IIIA: Cashout demonstrations—Experimental design						
Fraker et al. (1992)	Alabama cashout demonstration (1990)	At-home, away, total Purchased food only and nonpurchased food included Per household, ENU, and AME per month	FSP participants (n=2,386)	Random assignment of participants to check or coupon	Group membership dummy; benefit amount	Multivariate regression
Ohls et al. (1992)	San Diego cashout demonstration (1990)	At-home, away, total Purchased food only and nonpurchased food included Per household, ENU, and AME per month	FSP participants (n=1,143)	Random assignment of participants to check or coupon	Group membership dummy; benefit amount	Multivariate regression
Group IIIB: Cashout demonstrations—Nonexperimental design						
Cohen and Young (1993)	Washington State cashout demonstration (1990)	At-home, away, total Purchased food only and nonpurchased food included Per household, ENU, and AME per month	Households participating in AFDC and who applied after FIP[7] implementation (n=780)	Comparison of treatment and matched comparison counties	Group membership dummy; benefit amount	Multivariate regression
Davis and Werner (1993)	Alabama ASSETS demonstration (1990)	At-home, away, total Purchased food only Per household and AME per month	ASSETS and FSP participants (n=1,371)	Comparison of treatment and matched comparison counties	Group membership dummy; benefit amount	Multivariate regression

See notes at end of table.

Continued—

Table 8—Studies that examined the impact of the Food Stamp Program on household food expenditures—Continued

Study	Data source[1]	Measure of expenditures[2]	Population (sample size)	Design	Measure of participation	Analysis method
Beebout et al. (1985)	1977 Puerto Rico supplement to the NFCS and 1984 Puerto Rico HFCS	At-home, total Nonpurchased food included Per household and AME per week	Participant and FSP-eligible nonparticipant households using 1977 eligibility criteria (n= 3,995)	Pre-cashout compared with cashout (1977 vs. 1984)	Group membership dummy; participation dummy; benefit amount	2-equation selection-bias models

[1] Data sources:

ASSETS = Avenues to Self-Sufficiency through Employment and Training Services.
BLS-CES = Bureau of Labor Statistics' Consumer Expenditure Survey.
CSFII = Continuing Survey of Food Intakes by Individuals.
HFCS = Household Food Consumption Survey.
NFCS = Nationwide Food Consumption Survey.
NFCS-LI = Nationwide Food Consumption Survey - Low Income Supplement.
PSID = Panel Study of Income Dynamics.

[2] Includes indications of whether the dependent variable corresponds to food consumed at home, food consumed away from home, or all food; whether measure(s) represent only food purchased with cash, credit, or food stamp coupons or include the estimated dollar value of home-grown food, gifts, etc.; whether expenditures are measured per person, per household, per adult male equivalent (AME), per equivalent adult, or per equivalent nutrition unit (ENU); and the time unit for expenditures.

[3] Does not treat FSP as endogenous.

[4] Eligible participants were isolated in the nonparticipant group.

[5] Main effects were not reported.

[6] Eligible participants not isolated in the nonparticipant group.

[7] FIP = Family Independence Program.

of the dose-response models included BENEFIT only, and half included both FSP and BENEFIT.

When only FSP is included in the model, a direct estimate of the impact of the program is obtained from the value of b_1, the coefficient on the participation dummy. When BENEFIT is included in the model, b_2 is the MPS_F out of food stamps while b_3 is the MPS_F out of nonfood stamp income. In models with both FSP and BENEFIT, b_1 represents the impact of the FSP on food expenditures that is independent of the benefit level—for example, FSP nutrition education may have a fixed effect on food expenditures regardless of the FSP benefit amount. Alternatively, b_1 may be interpreted in these models as the *selection effect*, or the expected difference in expenditures *absent the FSP* (or if FSP benefit levels were zero) between individuals with similar characteristics who do and do not choose to participate in the FSP. Some researchers excluded this term when including nonparticipants in their samples, risking a bias in the estimated MPS_F if there is indeed a selection effect (Kramer-LeBlanc et al., 1997; Chavas and Yeung, 1982). Other researchers excluded nonparticipants altogether, analyzing only variations in benefit levels and dropping the FSP term (Levedahl, 1995, 1991; Senauer and Young, 1986; Neenan and Davis, 1977).

Numerous variations on these model specifications are found in the literature. For example:

- Household expenditures on food may be dollars spent over a particular period or the monetary value of food consumed from household supplies during the period.

- Household food expenditures may be normalized to account for the household's size, age/sex composition, meals eaten away from home, and/or economies of scale; or alternatively, household food expenditures that have not been normalized may be analyzed with household size and composition included as covariates.

- Other income may be subdivided to estimate the separate effects of different income sources on food expenditures.

- The food stamp benefit and income may enter the equation nonlinearly, for example, in quadratic or logarithmic form.

The measure of food expenditures is often determined by the data. For example, researchers using national survey data often do not have a choice because available measures are limited. As shown in table 8, researchers using the 1968-72 PSID were limited to annual expenditures for food used at home, which is not likely to be a very precise measure.

Normalization of household food expenditures to account for household size and composition is usually done by standardizing food expenditure on a per capita basis, or by one of several alternatives that reflect relative nutritional needs of household members, including "equivalent adults" (EAs), counting additional family members less heavily because of economies of scale; "adult male equivalents" (AMEs), counting family members according to caloric requirements; or "equivalent nutrition units" (ENUs), counting family members according to caloric requirements and percentage of meals eaten at home.

Research Results

The following sections summarize findings from research that examined the impact of the FSP on food expenditures. The discussion addresses results, in turn, for each of the three design/analysis approaches.

Participant vs. Nonparticipant Comparisons

Seven studies used participant vs. nonparticipant comparisons to directly estimate the impact of the FSP on food expenditures. As expected, all of these studies found that FSP participants spent more on food than did nonparticipants (table 9). Although the studies were conceptually similar, they varied substantially in how they measured food expenditures. Some used money spent on food for at-home use over the course of a week, while others used the monetary value of food consumed out of household supplies over a week or a month.[29] Furthermore, some studies analyzed total household food expenditures, while others normalized household food expenditures to account for household composition.

The numerical estimates shown in table 9 are taken directly from the cited studies and hence vary in their units. Some pertain to food expenditures per week, others per month, and so on. To achieve some roughly comparable measure across studies, the last column in table 9 shows the estimated impacts as a percentage of food expenditures. Depending on how the authors reported sample characteristics, these values were calculated either as a percentage of sample mean food expenditure or as a percentage of the "counterfactual"—the amount participants would have spent on food absent the FSP.

[29]Authors analyzing national survey data did not have a choice in this regard. The studies conducted by Lane (1978) and West et al. (1978), however, were based on data collected specifically for this purpose.

Hama and Chern (1988) estimated a simultaneous model of food expenditure, but treated FSP participation as exogenous. Price (1983) estimated a model based on nonparticipants and then compared predicted values (evaluated at the mean values of participants' characteristics) with participants' actual expenditures. Basiotis et al. (1983), Salathe (1980), and West et al. (1978) simply used FSP participation dummies.

Four of the available studies cannot be generalized to the FSP population as a whole. Studies by West et al. (1978) and Hama and Chern (1988) used samples that made up only part of the eligible population—households with children ages 8-12 and households with one or more elderly members, respectively. In addition, West et al. (1978) and Lane (1978) used samples that were geographically restricted—to the State of Washington and to a single county in California, respectively. Findings from the studies completed by Kisker and Devaney (1988) and Lane (1978) are limited because the authors did not estimate multivariate models.

Although the potential for selection bias remains, the strongest evidence in this group of studies comes from the work done by Basiotis et al. (1983), Price (1983), and Salathe (1980). Putting aside differences in methodology and measurement and assuming that an FSP household contains, on average, two people, estimates from these three studies suggest that FSP participation increases household food expenditures by $2-$4 per week. The absolute effect corresponds to 18-20 percent of at-home food expenditures.

Dose-Response Studies

Of the 23 of the 32 identified studies, 23 used dose-response models to study the impact of FSP participation on household food expenditures, including the 20 studies in Group II (table 8), as well as 3 studies from Group I (Price, 1983; Salathe, 1980; West et al., 1978) that used both direct and dose-response estimates. The dose-response studies related food expenditures to the FSP benefit amount, calculating the MPS_F out of food stamps. Table 10 shows the MPS_F from food stamps, as estimated

Table 9—Findings from studies that examined the impact of the Food Stamp Program on household food expenditures using participant vs. nonparticipant comparisons

Study	Population	Measure	Estimated impact: Absolute	Estimated impact: As a share of food expenditures[1]
			Dollars	*Percent*
Hama and Chern (1988)	Households with 1 or more people 65+	Per capita at-home food expenditures per week	0.64	3.7
Kisker and Devaney (1988)	FSP-eligible households	Money value of food used at home per "equivalent nutrition unit" per week	2.49	10.8
Basiotis et al. (1983)	FSP-eligible households	At home food cost per household per week	3.70	20.4
Price (1983)	All households	Expenditures for at-home food per week per adult equivalent	2.01	18.2
Salathe (1980)	FSP-eligible households	Per capita at home food purchases per week	At home: 1.45 Total: .88	18.8 9.4
Lane (1978)	FSP-eligible households	At home food expenditures + value of food in-kind, per person per month	3.26	10.9
West et al. (1978)	FSP-eligible households with child ages 8-12	Value of food consumed at home per month per "equivalent adult"	5.14	13.0

[1]These percentages were calculated relative to either the *sample mean* as reported by the author (Basiotis et al., $18.11; Hama and Chern, $17.48; Kisker and Devaney, $23.14), or the author's estimated counterfactual value—that is, what participants would have spent on food if they did not receive food stamps or what nonparticipants actually did spend on food (Lane, $30.00; West et al., $39.63; Salathe, $7.71 and $9.28; Price, $11.03).

in these studies. This table relies heavily on table IV.1 in Fraker (1990), which summarized 17 studies.

Fraker completed a careful analysis of the bulk of this research. He remarked that the estimates of the MPS_F varied greatly in size, ranging from 0.17 at the low end to 0.64 and 0.86 at the high end.[30] The two highest estimates are clearly outliers, since the third-highest estimate is 0.47 and four other estimates are in the range of 0.42-0.45.

Fraker goes on to explain why the two highest estimates are so different from the others. One of the estimates, obtained from a dynamic-adjustment model, represents “the full long-run or steady-state responses of households to changes in food stamp (and other food subsidy) benefits.” The other estimate is based on an unstable model that yields vastly different estimates for two half-samples of the data. Both estimates rely on a measure of non-food stamp income that excludes welfare and nonwelfare transfer payments but includes some imputed income elements, and both estimates mingle other FANP benefits (such as school lunches) with the FSP benefit. Consequently, these two estimates can be discounted, leaving a set of estimates “roughly evenly distributed over the range of 0.17 to 0.47, indicating that a $1.00 increase in the value of the food stamp benefit of a typical recipient household would lead to additional food expenditures of between $0.17 and $0.47.”

The studies listed in table 10 span the period before and after the elimination of the purchase requirement (EPR) in the FSP. Before the EPR, participants were required to use the food stamps they paid for, as well as the bonus stamps, to purchase food. After the EPR, only the bonus amount was given in stamps. Fraker stated that estimates based on data collected before the EPR are likely to be biased upward, relative to the current MPS_F, because the EPR should have led to many more participants being unconstrained in their food purchases—that is, treating their food stamp allotment as cash. Their MPS_F should therefore be much lower, close to that of non-food stamp income.[31] Yet, Fraker notes that “the three estimates that are based on post-EPR data range from 0.23 and 0.29 and are only slightly toward the low end of the distribution of all estimates.”[32]

Four of the more recent post-EPR estimates that were not available to Fraker (Breunig et al., 2001; Kramer-LeBlanc et al., 1997; Levedahl, 1995, 1991) do not support the notion that the MPS_F has declined since 1979. Their values are 0.40, 0.35, 0.26, and 0.69, respectively. A possible explanation for this apparent paradox is that the EPR substantially increased participation, drawing households into the program that were

[30]The estimate of 0.64, which is from Hymans and Shapiro (1976), is not included in table 10. Where Fraker’s table IV.1 gave multiple estimates from the same study, table 10 includes only the most general estimate—in this case, the estimate from the full sample and not those from two half-samples. The estimate of 0.69 shown in table 10 (Levedahl, 1991) was not included in the research reviewed by Fraker.

[31]Fraker also presents estimates of the MPS_F out of non-food stamp income, which are not discussed here. They range from 0.05 to 0.24 and are invariably lower than the corresponding MPS_F out of food stamps from the same study.

[32]These estimates come from Chen (1983), Senauer and Young (1986), and Fraker et al. (1990).

Table 10—Findings from studies that examined the impact of the Food Stamp Program on household food expenditures using dose-response analyses[1]

Study		Estimated MPS_F from food stamps
Breunig et al. (2001)[2]		0.40
Kramer-LeBlanc et al. (1997)[2]		.35
Levedahl (1995)[2]		.26
Levedahl (1991)[2]		.69
Fraker et al. (1990)		.29
Devaney and Fraker (1989)	Weighted:[3]	.42
	Unweighted:	.21
Basiotis et al. (1987)		.17
Ranney and Kushman (1987)[2]		.40
Senauer and Young (1986)	Pre-EPR:[4]	.30
	Post-EPR:[4]	.26
Smallwood and Blaylock (1985)		.23
West (1984)	Participants:	.17
	Eligibles:	.47
Allen and Gadson (1983)		.30
Chen (1983)	Pre-EPR:[4]	.20
	Post-EPR:[4]	.23
Price (1983)[2]		.42
Brown et al. (1982)		.45
Chavas and Yeung (1982)		.37
Johnson et al. (1981)		.17
Salathe (1980)		.36
West et al. (1978)		.31
Neenan and Davis (1977)		.45
Benus et al. (1976)		.86
Hymans and Shapiro (1976)		.29
West and Price (1976)		.30[5]

[1]Adapted and expanded from Fraker (1990), table IV.1. The MPS_F is the fraction of each additional dollar of income that is spent on food.

[2]These studies were not included in Fraker (1990).

[3]Using sample weights from the NFCS.

[4]EPR = Elimination of the purchase requirement.

[5]Fraker reports this value as 0.37, citing p. 729 of West and Price. This appears to be an error on Fraker's part. The text there reads: “The marginal propensity to obtain food out of bonus stamp income (0.30) is still below the average propensity of food stamp recipients to consume out of all income (0.37).” The latter value is apparently the ratio of food expenditures to total income for food stamp recipients. Data reported in the article are not sufficient, however, to make this calculation directly.

not willing to spend as much on food as the purchase requirement necessitated. These new participants might indeed be constrained in their food purchases, even if the constraint was removed for those who would have participated under the old system.

All of the estimates reported in table 10 are subject to caveats. Most studies have criticized their predecessors and further criticism has been applied in review articles. Among the issues affecting some or all of the estimates are the following:

- Early studies used data collected before 1975, when uniform national standards for food stamp eligibility and benefits were implemented.

- Many studies used data that are not nationally representative samples of FSP eligibles—that is, that were restricted to a particular geographic area or demographic subgroup.

- The functional form of the relationship between food stamps and food expenditures may be misspecified. (Levedahl (1991) reestimates the expenditures equation with three common functional forms plus the one he believes is correct and gets alternative values of the MPS_F, ranging from 0.29 to 0.69.)

- Many researchers identify constrained households as those in which monthly food expenditures exceed their allotment by no more than a small margin and exclude these households from the analysis. No further mention is then made of the constrained households for which, indeed, the FSP increases food expenditures markedly.

- If, as seems plausible, FSP households have a higher MPS_F out of non-food-stamp income than nonparticipant households, a model that includes both participants and nonparticipants and does not fully account for selection bias will overestimate the MPS_F from food stamps.

- Sample weights may have been used improperly (or not at all). Devaney and Fraker (1989) found that using weights in the NFCS nearly doubled the estimated MPS_F.[33]

- Faulty accounting for the effects of household size and composition on food expenditures may lead to a biased estimate. Blaylock (1991) estimated food expenditure elasticities of 0.778 when both food expenditures and income were measured on a per household basis, 0.687 when both were measured on a per capita basis, and 0.521 when food expenditures were measured on a basis that accounted for economies of scale and income was measured on a per capita basis. Assuming that the last of the cited estimates applies, the household-based estimates are too large by nearly 50 percent.[34]

The Levedahl (1991) estimate of 0.69 is so distant from the others that it requires further comment. In a later article (1995), Levedahl stated:

> The theoretical and empirical results presented in this paper demonstrate that, except for the specification used by Senauer and Young, approximations used to estimate the food expenditure equation of food stamp recipients are misspecified. ...Given the availability of this specification, it would be difficult to justify using a functional form that was not flexible when estimating the food expenditure equation of food stamp recipients.

The Senauer and Young specification that Levedahl was recommending is the double-log form, which gave Levedahl an MPS_F out of food stamps of 0.29 in his 1991 paper and 0.26 in the 1995 paper (using San Diego cashout demonstration data). One, therefore, can reasonably conclude that the 0.69 estimate, based on translog specification, is an outlier.

The Cashout Demonstrations

Finally, findings from the five cashout studies (table 11) provide lower-bound estimates of the impact of the FSP. Included in this group are the following:

- Two studies of "pure" cashout demonstrations in Alabama and San Diego, in which the only difference between groups was the form of the food stamp benefit (cash vs. check).

- Two studies of other cashout demonstrations—Alabama Avenues to Self-Sufficiency Through Employment and Training Services (ASSETS) and Washington Family Independence Program

[33] In a comment on the Devaney and Fraker (1989) paper, Kott (1990) suggested that the effect of weights could be due to differences in the MPS_F between low-income households that lived in high-poverty vs. low-poverty areas, which was a sample stratifier. The latter group was undersampled, and if its MPS_F is substantially higher than that of the former group, then a weighted estimate of the overall MPS_F would be higher than the unweighted version.

[34] The elasticity of food expenditures with respect to income, η_F, is the percentage increase in food expenditures associated with a 1-percent increase in income. If a household spends one third of its income on food, then its MPS_F is equal to η_F X 1/3. Blaylock's analysis, based on the 1982 CES, used total expenditures as the measure of income and did not break out the effects of food stamps per se.

(FIP)—in which other programmatic changes were made simultaneously.

- One study of the conversion in Puerto Rico from food stamps to the cashed-out Nutrition Assistance Program (NAP).

The impact of cashout may be interpreted as the effect of one of the two components of food stamp benefits, namely the coupon format. The cashout effect is a lower bound of the total impact of the FSP because it excludes the effect on food expenditures of giving households more money. Note that the cashout effects are expected to be *negative*: They represent the effect of not providing benefits in coupon form.

The direct estimates of differences in food expenditures provide comparisons that are free of a major potential source of selection bias: Both check and coupon recipients are FSP participants. Other biases are possible, however, as the Puerto Rico study used a pre-/post-design, with a 7-year interval, and both the Alabama ASSETS and Washington State demonstrations were based on matched treatment and comparison counties. The pure cashout demonstrations in Alabama and San Diego were, however, true experiments. An additional limitation of the cashout studies is their limited generalizability. While many of the studies discussed were based on national surveys, each cashout evaluation reports results from a single State.

The estimated impacts on expenditures per AME or ENU per month for *food used at home* range from -$0.34 (Alabama “pure” cashout) to -$25 (Alabama ASSETS).[35] In percentage terms, the range is from -0.3 to -21.9 percent. It is generally acknowledged that

[35]Estimate based on 4.3 weeks per month. Results are discussed on an AME or ENU basis, so the Puerto Rico study can be included in the comparison.

Table 11—Findings from studies that examined the impact of food stamp cashout on household food expenditures

	Study/demonstration				
Estimated impact	Cohen and Young (1993)/ Washington (FIP)	Davis and Werner (1993)/ Alabama (ASSETS)	Fraker et al. (1992)/ Alabama (pure cashout)	Ohls et al. (1992)/ San Diego (pure cashout)	Beebout et al. (1985)/ Puerto Rico (FSP conversion)
On purchased food used at home per household per month:					
Absolute (dollars)	-28.08	-56.44	2.66	-22.25	
Percent	-12.1	-26.8	1.1	-7.5	
On purchased food used at home per AME/ENU per month:					
Absolute (dollars)	-22.12	-25.43	-.34	-9.39	-2.95
Percent	-17.2	-21.9	-.3	-6.9	-2.4
On total food expenditures per household per month:					
Absolute (dollars)	-25.60	-54.47	2.16	-23.85	
Percent	-7.3	-23.6	.9	-7.3	
On total food per AME/ENU per month:					
Absolute (dollars)	-26.62	-23.62	-.99	-10.98	-1.00
Percent	-13.4	-18.5	-.7	-7.3	-.5
On MPS_F out of food stamp benefits			.01	-.17	-.06

Notes:
AME = Adult Male Equivalent.
ASSETS = Avenue to Self-Sufficiency through Employment and Training Services.
ENU = Equivalent Nutrition Unit.

the Puerto Rico conversion and the Alabama "pure" cashout demonstration were not realistic tests of the differences between checks and coupons. In Puerto Rico, food stamps were used as a second currency even before the changeover, so they were, in a sense, already cashed out. In Alabama, the issues were that cashout was introduced with little publicity as a short-term demonstration, and food assistance was issued as a separate check that was not combined with AFDC. Hence, check recipients were still likely to treat their food stamp benefits as earmarked for food. The San Diego result, an impact of -$9.39 (-6.9 percent), seems the strongest, being unconfounded with other changes and based on an experimental design.

Four of the five studies reviewed also estimated impacts on *total food expenditures*. The estimated impacts were quite similar to those for food at home, indicating that offering food stamps as coupons rather than cash reduces expenditures on food away from home only slightly, if at all.

The authors of three of the cashout studies also estimated the MPS_F for food stamp checks vs. coupons. The difference between the two estimates is again a lower-bound estimate of the impact of the FSP. These differences were quite small in Puerto Rico and the "pure" cashout demonstration in Alabama, but an impact of 0.17 was found in San Diego. Because of its strong design, the San Diego study settles, in the affirmative, the question of whether the FSP increases food expenditures more than would a cash grant. As an aside, the MPS_F for food stamp coupons, per se, was estimated as 0.28 in this study, typical of other estimates.

Household Nutrient Availability

Most studies that examined nutrition-related impacts of the FSP, especially the more recent ones, focused on impacts on the dietary intake of individuals residing in FSP households. A smaller number of studies examined nutrient availability at the household level. These two outcomes are logically sequential. The hypothesis is that the FSP benefit leads to increased food spending, which leads to increased household nutrient availability, which, in turn, leads to increased intake by individual household members. This section focuses on the middle, or household, link in this chain.

As discussed in the preceding section, FSP participation definitely leads to an increase in food expenditures. One would suppose that, by spending more on food, households would increase the availability of food energy and at least some nutrients. This seemingly obvious effect may not occur for several reasons, however, particularly for nutrients that are in short supply. Participating households may increase expenditures on food in ways that actually reduce the availability of some nutrients, for example, by choosing foods that are convenient or especially palatable, but lower in nutrients.[36] They may also purchase more expensive forms of the same food, resulting in no net gain in nutrients. In addition, nonparticipants may obtain more of their food from nonpaid sources, such as friends, relatives, soup kitchens, and food pantries (Gleason et al., 2000).

Moreover, even if increased food expenditures lead to increased nutrient availability, there is no guarantee that this effect will be consistently positive. For example, increased expenditures may lead to greater availability of nutrients and food components that Americans consume to excess, including fats, cholesterol, sodium, and added sugars.

Assessment of household nutrient availability is based on detailed records of household food use for an extended period, usually 1 week. Information on quantities of food withdrawn from the household food supply is translated into nutrient equivalents to represent the amount of food energy and nutrients available to household members. Although household nutrient availability thus excludes the nutrient content of food consumed away from home, it is still an important measure because the FSP is intended to have its beneficial effects specifically through improving in-home food consumption.

The amount of energy and nutrients available is evaluated relative to the Recommended Dietary Allowances (RDAs) and the household's size and composition. Household nutrient requirements are generally defined based on AMEs, which take into consideration the number of individuals in the household and their differing nutrient requirements based on age, gender, and pregnancy/lactation status, or ENUs, which further adjust for the number of meals each family member eats at home and the number of meals served to guests.

Research Overview

The literature search identified 14 studies that examined the impact of the FSP on household nutrient availability (table 12). All but three of these studies (Bishop et al., 2000; Devaney and Moffitt, 1991;

[36]See, for example, Prato and Bagali (1976).

Table 12—Studies that examined the impact of the Food Stamp Program on household availability of food energy and nutrients

Study	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group IA: Participant vs. nonparticipant comparisons—Secondary analysis of national surveys						
Hama and Chern (1988)	1977-78 NFCS elderly supplement	Aided recall for food use from household supply (7 days)	FSP-eligible households with elderly members (n=1,454)	Participant vs. nonparticipant	Participation dummy	Simultaneous food expenditure/nutrient availability equation[2]
Kisker and Devaney (1988)	1979-80 NFCS-LI	Record of household food use (7 days)	FSP-eligible households (n~2,900)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Allen and Gadson (1983)	1977-78 NFCS-LI	Aided recall for food use from household supply (7 days)	FSP-eligible households (n=3,850)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Basiotis et al. (1983)	1977-78 NFCS-LI	Aided recall for food use from household supply (7 days)	FSP-eligible households (n=3,562)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Scearce and Jensen (1979)	1972-73 BLS-CES	Food category amount and expenditure diary	FSP-eligible, southern region (n=1,360)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Group IB: Participant vs. nonparticipant comparisons—Local studies						
Lane (1978)	Kern County, CA (1972-73)	24-hour recall of food consumed at home	FSP-eligible households (n=329)	Participant vs. nonparticipant	Participation dummy	Bivariate comparisons
Group II: Dose-response estimates—Secondary analysis of national surveys						
Devaney and Moffitt (1991)	1979-80 NFCS-LI	Record of household food use (7 days)	FSP-eligible households (n=2,925)	Dose-response	Benefit amount	Multivariate regression; selection-bias models
Basiotis et al. (1987)	1977-78 NFCS-LI	Aided recall for food use from household supply (7 days)	FSP-eligible households (n~3,000)	Dose-response	Participation dummy; bonus value	Simultaneous equations for food cost/nutrient availability/nutrient intake relationship
Johnson et al. (1981)	1977-78 NFCS-LI	Aided recall for food use from household supply (7 days)	Low-income households (n=4,535)	Dose-response	Participation dummy; bonus value	Multivariate regression

See notes at end of table.

Continued—

Table 12—Studies that examined the impact of the Food Stamp Program on household availability of food energy and nutrients—Continued

Study	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group IIIA: Cashout demonstrations—Experimental design						
Bishop et al. (2000)	Alabama cashout demonstration (1990) and San Diego cashout demonstration (1990)	7-day food use from records and recall	Alabama FSP participants (n=2,184) San Diego FSP participants (n=935)	Random assignment of participants to check or coupon	Group membership dummy	Stochastic dominance methods
Fraker et al. (1992)	Alabama cashout demonstration (1990)	7-day food use from records and recall	FSP participants (n=2,386)	Random assignment of participants to check or coupon	Group membership dummy; benefit amount	Multivariate regression
Ohls et al. (1992)	San Diego cashout demonstration (1990)	7-day food use from records and recall	FSP participants (n=1,143)	Random assignment of participants to check or coupon	Group membership dummy; benefit amount	Multivariate regression
Group IIIB: Cashout demonstrations—Nonexperimental design						
Cohen and Young (1993)	Washington State cashout demonstration (1990)	7-day food use from records and recall	Households participating in AFDC and who applied after FIP[3] implementation (n=780)	Comparison of treatment and matched comparison counties	Group membership dummy; benefit amount	Multivariate regression
Beebout et al. (1985)	1977 Puerto Rico supplement to the NFCS and 1984 Puerto Rico HFCS	7-day food use from records and recall	Participant and FSP-eligible nonparticipant households using 1977 eligibility criteria (n= 3,995)	Pre-cashout compared with cashout (1977 vs. 1984)	Group membership dummy; participation dummy; benefit amount	2-equation selection-bias models

[1] Data sources:
BLS-CES = Bureau of Labor Statistics' Consumer Expenditure Study.
HFCS = Household Food Consumption Survey.
NFCS = Nationwide Food Consumption Survey.
NFCS-LI = Nationwide Food Consumption Survey - Low Income Supplement.
[2] Does not treat FSP as endogenous.
[3] FIP = Family Independence Program.

Scearce and Jensen, 1979) were included in the previous section on impacts on food expenditures. Six of the identified studies (Group I) used participant vs. nonparticipant comparisons. Five of these studies used national survey data, and one used local data. Group II includes three dose-response studies, all of which are based on secondary analysis of national survey data.

The studies in Groups I and II, most of which are described in Fraker's (1990) excellent review, employed a variety of modeling approaches. Some used structural models that estimated the FSP effect on expenditures and then the effect of expenditures on nutrient availability. Other researchers estimated reduced-form models, treating nutrient availability as a function of FSP benefits without regard to any intermediate mechanisms.

Group III includes the four cashout demonstrations described previously, as well as a more recent study that involved secondary analysis of data from the Alabama and San Diego demonstrations.[37] As described in the preceding section, two of the cashout studies used random assignment (Fraker et al., 1992; Ohls et al., 1992), one used matched treatment and control groups (Cohen and Young, 1993), and one used a pre-/ post-design to compare households in Puerto Rico before and after the FSP was cashed out (Beebout et al., 1985). Of the two randomized experiments, the San Diego study (Ohls et al., 1992) is generally considered to be the strongest because it did not suffer from implementation problems encountered in the Alabama study (Fraker et al., 1992).

The estimation approach for the San Diego, Alabama, and Washington cashout studies was to compare regression-adjusted mean nutrient availability for households in the treatment and control or comparison groups. In the Puerto Rico cashout study, a structural modeling approach was used to estimate the effect of cashout on expenditures and then the effect of expenditures on nutrient availability (Beebout et al. 1985).

In interpreting findings from the cashout studies, one should remember that these studies were designed to measure only the effect of the *form* of the FSP benefit—food coupons or cash—rather than the full program impact, including the dollar value of the benefit and the form in which it was delivered. The randomized design used in the San Diego study, in particular, makes that study's evidence particularly powerful when it indicates positive impacts. If one program component has a positive impact, then the program as a whole must have a positive impact. However, when no significant impact is detected, one cannot conclude that the overall program has no impact.

With the exception of the cashout studies, all of the studies that examined the impact of the FSP on household nutrient availability are based on data that were collected between the early 1970s and 1980. Applying findings from these studies to today's FSP population must be done with some caution.

Although the same general caution can be raised about research on food expenditures, a compelling argument can be made that impacts on nutrition-related outcomes are more sensitive to temporal considerations than impacts on food expenditures. For example, the American food supply has changed dramatically in the past 20-25 years, with important implications for both nutrient availability and individual dietary intake. Americans are eating substantially more grains than they were two decades ago, particularly refined grains, as well as record-high amounts of caloric sweeteners and some dairy products, and near-record amounts of added fats (Putnam and Gerrior, 1999).

In addition to myriad new products in the market and changes in food enrichment policies and standards over time, a number of sociodemographic trends may have influenced food-purchasing behaviors. These trends include, for example, a rise in the amount of food eaten away from home, smaller households, more two-earner and single-parent households, an aging population, and increased ethnic and racial diversity (Putnam and Gerrior, 1999).

The data on household nutrient availability are also subject to the limitations that affect much of the available research on nutrition-related impacts of FANPs, as discussed in chapter 2. In assessing impacts on household nutrient availability, most researchers used the "more is better" approach that was the state of the art at the time. However, increased availability of energy or nutrients at the household level may or may not influence the likelihood that individual household members consume adequate diets. And, in the case of food energy, fat, cholesterol, and sodium, increased availability may not be a positive effect. (Only one study examined impacts on the availability of fat, and none looked at availability of cholesterol or sodium.)

[37]Excluded from this table is a recent study of food security and nutrient availability by Cohen et al. (1999). The authors analyzed only variations in nutrient availability among participant households, so program impacts could not be estimated.

Finally, two features of data on household nutrient availability tend to impart a substantial amount of measurement error to the estimates. First, the translation of foods into nutrients is only an approximation. Second, the samples of data on foods withdrawn from stocks and used are small and subject to sampling variability. These characteristics may obscure differences between participant and nonparticipant households.

Research Results

Table 13 summarizes findings of studies that examined the impact of the FSP on household nutrient availability. The table focuses on the question of whether the FSP had any statistically significant impact on the availability of a given nutrient and does not present information on the estimated amount of the FSP impact. Because one cannot assume that increased food expenditures automatically translate into increased availability of any particular nutrient, the first and most important question is whether any significant effect exists. In addition, the variety of ways in which individual study authors analyzed and reported nutrient impacts makes finding a common metric for characterizing results difficult.

Table 13 is divided into four sections: food energy and macronutrients, vitamins, minerals, and summary measures. The text follows this general organization, but discusses findings for vitamins and minerals in one section.

In the interest of providing a comprehensive picture of the body of research, both significant and nonsignificant results are reported in table 13 and in all other "findings" tables. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned, however, to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies. In this case, the greatest weight is given to the study by Devaney and Moffitt (1991) (shown in the table, as with all the studies, by primary author's name (Devaney, 1991). This is the only non-cashout study that is based on data collected after the elimination of the purchase requirement. In addition, the study used a dose-response model to assess FSP impacts, an approach less prone to problems of selection bias than participant vs. nonparticipant comparisons. The authors included tests of selection-bias adjustment models and found that these had little effect on their results.

Substantial weight is also given to *significant* findings from the San Diego cashout study (Ohls et al., 1992). Nonsignificant findings from this study are not given the same weight because, as previously noted, the cashout studies assessed the impact of the form of the FSP benefit rather than of the overall program. Thus, the absence of a significant effect in the cashout studies does not provide convincing evidence than an effect does not exist.

Food Energy and Macronutrients

Findings from the strongest available research suggest that FSP participation increases the amount of food energy available at the household level. The San Diego cashout study found a significant effect of food stamp coupons on the availability of food energy, whether measured as mean percentage of the Recommended Energy Allowance (REA) or as the percentage of households that had less than 100 percent of the REA for energy available in the household food supply (Ohls et al., 1992). Devaney and Moffitt (1991) reported similar results.

Overall findings for the availability of protein (in absolute terms, not as a percentage of total food energy) were quite similar. Both Devaney and Moffitt (1991) and Ohls et al. (1992) found that the FSP significantly increased protein availability. Three of the four other studies that assessed protein availability reported similar results. The only exception was the Alabama cashout study in which implementation was weak (Fraker et al., 1992).

Allen and Gadson (1983) conducted the only study to examine availability of carbohydrates and fat, and they did so in absolute terms rather than as a percentage of total food energy. They found that the FSP significantly increased the availability of both nutrients at the household level.

Given the age of most of the available studies, the paucity of information about the impact of the FSP on the relative availability of carbohydrates and fat is not surprising. Until the 1990s, almost all empirical research on FANPs focused on nutritional adequacy. Since that time, studies have begun to focus on nutritional concerns related to overconsumption of fat, saturated, fat, cholesterol, and sodium, and/or on food consumption patterns (for example, consumption of

Table 13—Findings from studies that examined the impact of the Food Stamp Program on household availability of food energy and nutrients

	Significant impact	No significant impact		Significant impact
Outcome	More energy/nutrients available	More energy/nutrients available	Less energy/nutrients available	Less energy/nutrients available
Food energy and macronutrients				
Food energy	***All households*** Cohen (1993) [1 State; CO] Ohls (1992) [1 city; CO] Devaney (1991) [national; D-R] Basiotis (1983) [national; D-R] Allen (1983) [national; P-N] Johnson (1981) [national; D-R] ***Elderly*** Hama (1988) [national; P-N]	***All households*** Bishop (2000) [Alabama; CO] Fraker (1992) [1 State; CO] Beebout (1985) [Puerto Rico; CO] Scearce (1979) [national; P-N]	***All households*** Bishop (2000) [San Diego; CO]	
Protein	***All households*** Cohen (1993) [1 State; CO] Ohls (1992) [1 city; CO] Devaney (1991) [national; D-R] Allen (1983) [national; P-N] Scearce (1979) [national; P-N]	***All households*** Fraker (1992) [1 State; CO]		
Carbohydrates	***All households*** Allen (1983) [national; P-N]			
Fat	***All households*** Allen (1983) [national; P-N]			
Vitamins				
Vitamin A	***All households*** Devaney (1991) [national; D-R] Allen (1983) [national; P-N]	***All households*** Cohen (1993) [1 State; CO] Fraker (1992) [1 State; CO] Ohls (1992) [1 city; CO] Beebout (1985) [Puerto Rico; CO] Basiotis (1983) [national; P-N] Scearce (1979) [national; P-N]		

See notes at end of table.

Continued—

Table 13—Findings from studies that examined the impact of the Food Stamp Program on household availability of food energy and nutrients—Continued

	Significant impact	No significant impact		Significant impact
Outcome	More energy/nutrients available	More energy/nutrients available	Less energy/nutrients available	Less energy/nutrients available
Vitamin B_6	***All households*** Bishop (2000) [Alabama; CO] Devaney (1991) [national; D-R] Allen (1983) [national; P-N] ***Elderly*** Hama (1988) [national; P-N]	***All households*** Bishop (2000) [San Diego; CO] Cohen (1993) [1 State; CO] Ohls (1992) [1 city; CO] Beebout (1985) [Puerto Rico; CO]	***All households*** Fraker (1992) [1 State; CO]	
Vitamin B_{12}	***All households*** Allen (1983) [national; P-N]	***All households*** Beebout (1985) [Puerto Rico; CO]		
Vitamin C	***All households*** Cohen (1993) [1 State; CO] Devaney (1991) [national; D-R] Allen (1983) [national; P-N] Basiotis (1983) [national; P-N]	***All households*** Ohls (1992) [1 city; CO] Fraker (1992) [1 State; CO] Scearce (1979) [national; P-N]		
Vitamin E	***All households*** Bishop (2000) [Alabama; CO]			
Folate		***All households*** Cohen (1993) [1 State; CO] Ohls (1992) [1 city; CO]	***All households*** Fraker (1992) [1 State; CO]	
Niacin	***All households*** Allen (1983) [national; P-N]	***All households*** Scearce (1979) [national; P-N]		
Riboflavin	***All households*** Devaney (1991) [national; D-R] Allen (1983) [national; P-N]	***All households*** Scearce (1979) [national; P-N]	***All households*** Basiotis (1983) [national; P-N]	
Thiamin	***All households*** Devaney (1991)[national; D-R] Basiotis (1983) [national; P-N] Allen (1983) [national; P-N] Scearce (1979) [national; P-N]			

See notes at end of table.

Continued—

Table 13—Findings from studies that examined the impact of the Food Stamp Program on household availability of food energy and nutrients—Continued

	Significant impact	No significant impact		Significant impact
Outcome	More energy/nutrients available	More energy/nutrients available	Less energy/nutrients available	Less energy/nutrients available
Minerals				
Calcium	***All households*** Cohen (1993) [1 State; CO] Devaney (1991) [national; D-R] Allen (1983) [national; P-N] Scearce (1979) [national; P-N] ***Elderly*** Hama (1988) [national; P-N]	***All households*** Ohls (1992) [1 city; CO] Beebout (1985) [Puerto Rico; CO] Basiotis (1987) [national; D-R]	***All households*** Fraker (1992) [1 State; CO] Basiotis (1983) [national; P-N]	
Iron	***All households*** Cohen (1993) [1 State; CO] Devaney (1991) [national; D-R] Allen (1983) [national; P-N] Scearce (1979) [national; P-N] ***Elderly*** Hama (1988) [national; P-N]	***All households*** Basiotis (1987) [national; D-R] Beebout (1985) [Puerto Rico; CO] Basiotis (1983) [national; P-N]	***All households*** Fraker (1992) [1 State; CO] Ohls (1992) [1 city; CO]	
Magnesium	***All households*** Devaney (1991) [national; D-R] Allen (1983) [national; P-N] ***Elderly*** Hama (1988) [national; P-N]	***All households*** Beebout (1985) [Puerto Rico; CO]		
Phosphorus	***All households*** Devaney (1991) [national; D-R] Allen (1983) [national; P-N]	***All households*** Bishop (2000) [San Diego; CO]		
Zinc	***All households*** Cohen (1993) [1 State; CO]	***All households*** Fraker (1992) [1 State; CO] Ohls (1992) [1 city; CO]		

See notes at end of table.

Continued—

Table 13—Findings from studies that examined the impact of the Food Stamp Program on household availability of food energy and nutrients—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants scored higher	Participants scored higher/same	Participants scored lower	Participants scored lower
Summary measures				
Modified diet score[1]	***All households*** Johnson (1981) [national; D-R]			
Minimum nutrient diet ratio[2]			***All households*** Johnson (1981) [national; D-R]	
100+ % RDA for energy and 10 nutrients[3]	***All households*** Kisker (1988) [national; P-N]			
80+ % RDA for energy and 10 nutrients[3]	***All households*** Kisker (1988) [national; P-N]			

Notes: Cell entries show the senior author's name, the publication date, the scope of the study (for example, national vs. one city or one State), and the research approach (P-N = participant vs. nonparticipant study, D-R = dose response study, and CO = cashout study).

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Data for Lane (1978) not included because study used 24-hour recall rather than 7-day record/recall.

Data for Basiotis et al. (1987) not reported because the estimate was constructed out of a combination of parameter estimates and the statistical significance of the final estimate is not clear.

Bishop et al. (2000) also examined availability of protein, vitamin B_{12}, vitamin C, niacin, thiamin, calcium, magnesium, and iron. They found no significant differences between cash and coupon recipients. However, point estimates were not provided. In addition, while the availability of vitamin E and phosphorus was examined for both Alabama and San Diego samples, point estimates for the former were reported only for Alabama and point estimates for the latter were reported only for San Diego.

[1] Modified diet score is defined as the sum of ratios of actual nutrient values to RDA standards for seven nutrients (protein, vitamin A, vitamin C, riboflavin, thiamin, calcium, and iron).

[2] Lowest nutrient ratio (nutrient per 1,000 calories).

[3] Assessed the proportion of households with household nutrient availability that was above the standard indicated.

fruits and vegetables and whole grains). All of this research, however, has focused on the dietary intakes of individual FSP participants rather than availability at the household level.

Vitamins and Minerals

Evidence of an FSP effect on the availability of vitamins and minerals is weaker than it is for food energy and protein. Some nutrients were not assessed by Devaney and Moffitt (1991) or Ohls et al. (1992), and for the nutrients that were assessed in both studies, significant results were divergent. Devaney and Moffitt reported several significant impacts, while Ohls et al. reported none. As noted, lack of a significant effect in the cashout study (Ohls et al., 1992) is not definitive evidence that an FSP effect does not exist. Therefore, findings from Devaney and Moffitt (1991) provide the strongest available evidence about the impact of the FSP on household availability of vitamins and minerals.

Devaney and Moffit (1991) found that the FSP significantly increased household availability of a broad array of vitamins and minerals: vitamins A, B_6, C, riboflavin, thiamin, calcium, iron, magnesium, and phosphorus. The authors estimated that the FSP increased the amount of these nutrients available to the household by between about 20 and 40 percent of the RDA. The estimated MPS out of food stamp benefits was substantially higher than the MPS out of other income—that is, a dollar of food stamp benefits had a greater impact on nutrient availability than a dollar of cash income.

Using participant vs. nonparticipant comparisons, Allen and Gadson (1983) estimated comparable effects across roughly the same range of nutrients, adding vitamin B_{12} and niacin to the list. The remaining studies in all three groups found a mix of results.

Summary Measures

Three studies used composite indices to assess the overall effect of the FSP on household nutrient availability. The results are inconclusive but generally consistent with the pattern of findings for individual nutrients.

Kisker and Devaney (1988) examined the percentage of households whose at-home food use provided 100 percent of the REA as well as the RDAs for each of 10 nutrients. A comparable summary statistic was computed using a cutoff of 80 percent rather than 100 percent. The authors report a favorable and significant FSP impact for both summary measures. The analysis was limited to bivariate comparisons of participants vs. nonparticipants, however, so the results must be considered suggestive only.

Johnson et al. (1981) constructed two summary measures. The first was a Modified Diet Score (MDS) that aggregated individual RDA “scores” (percentage RDA) for food energy and seven nutrients. Values for each nutrient were truncated at 1.2 to avoid the possibility of large excesses in one nutrient compensating for shortages in another. The authors also assessed the nutrient density of the foods used from the home food supply (nutrients per 1,000 calories), using a measure called the Minimum Nutrient Diet Ratio (MNDR). The first measure showed a statistically significant positive effect in their dose-response analysis, but the effect for the second measure was nonsignificant.

Finally, Basiotis et al. (1987), also using a dose-response approach, found a positive effect on household nutrient availability as measured by an index that was the first principal component of 11 individual nutrients.[38]

Individual Dietary Intake

The food eaten by individuals is primarily determined by the food available in the households to which they belong. However, the relationship between nutrient availability at the household level and nutrient intake at the individual level is weakened by several considerations:

- Household members may unequally consume nutrients from the food supplies, relative to their needs, depending on their tastes and appetites.
- Some household food supplies are consumed by guests or are wasted.
- Some household members may consume food from other sources, including restaurants, school cafeterias, and other nonhome sources.

Moreover, increased availability of food energy and selected nutrients at the household level does not necessarily translate into *better* diets at the individual level—for example, to lower intakes of dietary components overconsumed by many Americans (fat, saturated fat, cholesterol, and sodium) or to healthier patterns

[38]Because the estimate is constructed out of a combination of parameter estimates, the statistical significance of the final estimate is not clear and is therefore not reported in table 13.

of *food* intake (for example, eating more fruits and vegetables or whole grains).[39] For these reasons, one must examine the dietary intakes of individual household members to adequately assess nutrition-related impacts of the FSP.

Research Overview

The literature search identified 26 relevant studies (table 14). Only four of these studies (Kramer-Le Blanc et al., 1997; Fraker et al., 1990; Basiotis et al., 1987; West et al., 1978) were included in the previous sections on impacts on food expenditures and/or household nutrient availability. Most of the identified studies focused on impacts within subgroups of the population, most often children or the elderly.

Sixteen of the identified studies used a participant vs. nonparticipant design (Group I). Of these, 10 involved secondary analysis of data from national surveys. The other six participant vs. nonparticipant studies used State or local samples. Two of these studies used data from the FNS/SSI Elderly Cashout Demonstration (1980-81), but not in the context of a cashout study, per se. The researchers who used these data (Posner et al., 1987; Butler et al., 1985) combined data across cash and coupon sites because no significant differences were detected between the two groups. They defined participants as those receiving FSP benefits, whether in the form of cash or coupons, and nonparticipants as individuals who were income-eligible but not participating in the FSP.

Ten studies used a dose-response approach to estimate FSP impacts (Group II). Seven of these studies used national survey data and the remaining three used State or local data. None of the cashout studies (Group III in the preceding two sections) examined impacts on individual dietary intake.

The data used in studies that assessed impacts of the FSP on individual dietary intake are generally more recent than the data used in studies of impacts on food expenditures and household nutrient availability. For example, all eight studies that used national survey data to estimate impacts of the FSP on household nutrient availability used data collected mainly in the 1970s, with data collection periods ranging from 1972-73 through 1979-80 (table 12). The same is true of 18 of the 20 studies that used national survey data to investigate impacts on food expenditures (data collection periods from 1968-72 through 1979-80) (table 8), although, as noted, temporal considerations are less important for this outcome.

In contrast, 11 of the 17 studies that used national survey data to assess FSP impacts on individual dietary intake used data collected in the mid-1980s through the mid-1990s (data collection periods from 1985 through 1994-96) (table 14). Indeed, the studies by Dixon (2002) and Bhattacharya and Currie (2000), as well as those by Gleason et al. (2000) and Wilde et al. (1999) used national survey data that were the most recent available at the time the literature search was completed (NHANES-III for the Dixon and Bhattacharya and Currie studies and CSFII 1994-96 for the study by Wilde et al.).[40]

In addition, research on the impacts of the FSP on dietary intake addresses, albeit to a limited extent, nutrition-related concerns that were not addressed in the research on household nutrient availability. These concerns include consumption of fat, saturated fat, cholesterol, fiber, and sodium, as well as dietary intake patterns, or the extent to which *food* consumption behaviors conform with recommendations made in USDA's Food Guide Pyramid.

Nonetheless, the majority of research on FSP impacts on dietary intake is subject to the limitations discussed in chapter 2. Ten studies used intake data for a single day and therefore provide weak estimates of individuals' usual dietary intake. Seventeen studies used multiple days of data or food frequency instruments to better capture usual dietary intake behaviors; however, none used the approach to estimating usual intake that was recently recommended by the Institute of Medicine (IOM, 2001).[41] (Some studies used more than one method to assess dietary intake.)

Similarly, in assessing intakes of food energy, vitamins, and minerals, researchers generally compared mean intakes of participants and nonparticipants relative to the RDAs, or compared the proportion of individuals in each group with intakes below a defined cutoff and

[39] At the time most of these data were collected, the FSP offered little to no nutrition education to program participants to encourage such dietary patterns. However, whether providing nutrition education would have led to different results is not clear. For example, Gleason et al. (2000) demonstrated that the dietary knowledge and attitudes of low-income individuals did not mediate the relationship between FSP participation and dietary intake.

[40] In June 2002 and February 2003, data files for NHANES-IV 1999-2000, including the first 2 years of data from the 6-year NHANES data collection cycle, were released by the National Center for Health Statistics.

[41] Gleason et al. (2000) used these methods to describe dietary intakes of low-income populations. However, in assessing differences in the dietary intakes of FSP participants and nonparticipants, they compared regression-adjusted mean intakes rather than usual intakes.

Table 14—Studies that examined the impact of the Food Stamp Program on dietary intakes of individuals

Study	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group IA: Participant vs. nonparticipant comparisons—Secondary analysis of national surveys						
Dixon (2002)	1988-94 NHANES-III	24-hour recall	Adults ages 20 and older (n=10,545)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Bhattacharya and Currie (2000)	1988-94 NHANES-III	24-hour recall and nonquantified food frequency	Youth ages 12-16 (n=1,358)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Wilde et al. (1999)	1994-96 CSFII	2 nonconsecutive 24-hour recalls	Low-income individuals (n=1,901)	Participant vs. nonparticipant	Participation dummy	Maximum likelihood estimation
Weimer (1998)	1989-91 CSFII	24-hour recall followed by 2 days of food records	Elderly individuals (n=1,566)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Cook et al. (1995)	1986 CSFII-LI	24-hour recall followed by 2 days of food records	Children ages 1-5 in households under 125% of poverty[2]	Participant vs. nonparticipant	Participation dummy	Bivariate chi-squared tests
Rose et al. (1995)	1989-91 CSFII	24-hour recall followed by 2 days of food records	Children ages 1-5 (n=800)	Participant vs. nonparticipant	Participation dummy	Multivariate regression (weights not used)
Bishop et al. (1992)	1977-78 NFCS-LI	24-hour recall followed by 2 days of food records	FSP-eligible individuals (n=2,590)	Participant vs. nonparticipant	Participation dummy	Stochastic dominance methods
Fraker et al. (1990)	1985 CSFII	4 nonconsecutive 24-hour recalls	WIC-eligible women ages 19-50 (n=381) and their children ages 1-5 (n=818)	Participant vs. nonparticipant	Participation dummy	Multivariate regression and bivariate selection model
Gregorio and Marshall (1984)	1971-73 NHANES-I	24-hour recall	Preschool children (n=2,774), School-aged children (n=3,509)	Participant vs. nonparticipant	Participation dummy; participation interacted with poverty index ratio	Bivariate and multivariate regression
Lopez and Habicht (1987a, 1987b)	1971-73 NHANES-I and 1976-80 NHANES-II	24-hour recall	Low-income elderly (n=1,684 and n=1,388)	Participant vs. nonparticipant	Participation dummy	Multivariate analysis of variance

See notes at end of table.

Continued—

Table 14—Studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

Study	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group IB: Participant vs. nonparticipant comparisons—State and local studies						
Fey-Yensan et al. (2003)	Low-income areas in Connecticut (1996-97)	Food frequency questionnaire	Low-income elderly living in subsidized housing (82% female) (n=200)	Participant vs. nonparticipant	Participation dummy	Chi-square tests and analysis of variance
Perez-Escamilla et al. (2000)	2 pediatric clinics in low-income areas of Hartford, CT (1999)	24-hour recall and 2 food frequency questionnaires	Children ages 8 months to 5 years who were participating in WIC or who had participated in past year (n=99)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Perkin et al. (1988)	1 urban family practice center in Florida (dates for data collection not reported)	24-hour recall	Women ages 18-45 (n=102)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Posner et al. (1987)	1980-81 FNS SSI/ECD	24-hour recall via telephone	Elderly (n=1,900)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Butler et al. (1985)	1980-81 FNS SSI/ECD	24-hour recall via telephone	Low-income elderly individuals (n=1,684)	Participant vs. nonparticipant	Participation dummy	Multivariate regression with selection-bias technique
Futrell et al. (1975)	1 county in Mississippi (1971)	4-day record	Black children ages 4-5 (n=96)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Group IIA: Dose-response estimates—Secondary analysis of national surveys						
Gleason et al. (2000)	1994-96 CSFII/DHKS	2 nonconsecutive 24-hour recalls	Low-income individuals (n=3,935)	Dose-response	Benefit amount	Comparison of regression-adjusted means
Basiotis et al. (1998)	1989-91 CSFII	24-hour recall followed by 2 days of food records	Low-income households (n=1,379)	Dose-response	Participation dummy; benefit amount	Multivariate regression

See notes at end of table.

Continued—

Table 14—Studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

Study	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Rose et al. (1998a)	1989-91 CSFII	24-hour recall followed by 2 days of food records	Nonbreastfeeding preschoolers (n=499)	Dose-response	Benefit amount	Multivariate regression; investigated selection bias
Kramer-LeBlanc et al. (1997)	1989-91 CSFII	24-hour recall followed by 2 days of food records	FSP-eligible individuals (n=793)	Dose-response	Benefit amount	Multivariate regression
Akin et al. (1987)	1977-78 NFCS elderly supplement	24-hour recall followed by 2 days of food records	Elderly individuals (n=5,615)	Dose-response	Participation dummy; bonus value; participation interacted with social security income	Multivariate regression
Basiotis et al. (1987)	1977-78 NFCS-LI	24-hour recall followed by 2 days of food records	FSP-eligible individuals (n=3,000)	Dose-response	Participation dummy; bonus value	Simultaneous equations for food cost/nutrient availability/ nutrient intake relationship
Akin et al. (1985)	1977-78 NFCS elderly supplement	24-hour recall followed by 2 days of food records	Elderly individuals (n=1,315)	Dose-response	Participation dummy; bonus value	Multivariate switching regression model
Group IIB: Dose-response estimates—State and local studies						
Butler and Raymond (1996)	1980-81 FNS SSI/ECD and 1969-73 RIME	24-hour recall via telephone and in-person	Low-income elderly individuals (n=1,542) Low-income individuals in rural areas (n=1,093)	Dose-response	Participation dummy; bonus value	Multivariate endogenous switching model with selection-bias adjustment

See notes at end of table.

Continued—

Table 14—Studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

Study	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Whitfield (1982)	Tulsa, OK (1978)	24-hour recall	FSP-eligible individuals (n=195)	Dose-response	Participation dummy; bonus value	Multivariate regression
West et al. (1978)	Washington State (1972-73)	Unspecified	Children ages 8-12 (n=728)	Dose-response	Bonus value	Multivariate regression

[1]Data sources:
CSFII = Continuing Survey of Food Intakes by Individuals.
DHKS = Diet and Health Knowledge Survey.
FNS SSI/ECD = Food and Nutrition Service Supplementary Security Income/Elderly Cashout Demonstration.
NFCS = Nationwide Food Consumption Survey.
NFCS-LI = Nationwide Food Consumption Survey - Low Income Supplement.
NHANES = National Health and Nutrition Examination Survey.
RIME = Rural Income Maintenance Experiment.
[2]Sample size not stated.

used a “more is better” approach in interpreting findings. None of the identified studies used the approach recently recommended by the IOM, which calls for use of data on usual intake in conjunction with defined Estimated Average Requirements (EARs) (IOM, 2001).[42]

Consequently, the available research provides an imperfect picture of the substantive significance of observed differences in the dietary intakes of FSP participants and nonparticipants. The available research provides information on whether FSP participants consumed more or less energy and nutrients than nonparticipants. However, this information cannot be used to draw conclusions about whether FSP participants were more or less likely than nonparticipants to have *adequate* intakes.

Finally, previous caveats about measurement error also apply. The estimation of food and nutrient intake is an elaborate process that is subject to significant measurement error. This error may make it difficult to detect differences between participant and nonparticipant groups.

Research Results

Table 15 summarizes findings from available research on impacts of the FSP on dietary intake. Two studies have been omitted from this tabulation because the papers did not present detailed impact estimates (Akin et al., 1987; Akin et al., 1985).

Overall, the literature strongly suggests that the FSP has little to no impact on individuals’ dietary intake. In the discussion that follows, no single study is emphasized because of the general consistency of results across studies. Where results are inconsistent, findings from the study by Gleason et al. (2000), which examined impacts on preschool children, school-age children, and adults, are given the most weight. This study is based on the most recent CSFII data and used a dose-response approach. The authors elected not to estimate selection-correction models because they believed that neither the CSFII nor the companion Diet and Health Knowledge Survey (DHKS), which was also used in the analysis, included good candidates for identifying variables. Instead, the authors included in their model a wide variety of variables that may affect dietary intake and/or may be correlated with FSP participation or benefits. This included a number of variables not used in other research, including measures of dietary knowledge and attitudes, self-assessed general health status, indicators of self-reported health problems, and indicators for exercise frequency, smoking status, and use of vitamin and mineral supplements.

The authors tested the robustness of their results by estimating effects separately for subgroups of the population defined by age, gender, race/ethnicity, health status, income, and (for adults) dietary attitudes. In addition, they estimated several alternative models, including a model that used a quadratic specification of FSP benefit amounts, a model that used a single binary variable to represent FSP participation, and quantile regression models that examined the effects of FSP participation on different parts of the nutrient intake distribution (5th, 10th, 25th, 50th, 75th, and 90th percentiles). Results of all of these alternative analyses were qualitatively similar to results of the main analysis.

Food Energy and Macronutrients

Seventeen different studies assessed the impact of the FSP on the intake of food energy in one or more subgroups of the population. Only 2 of the 17 studies found a significant difference between FSP participants and nonparticipants (Fraker et al., 1990, for preschool children; Butler and Raymond, 1996, for the elderly), and the direction of the effect was not consistent.

A similar pattern was noted for protein. Seventeen different studies assessed the impact of FSP participation on protein intake. Only four studies (Fraker et al., 1990, for preschool children; Bishop et al., 1992, for all FSP households; Butler and Raymond, 1996, for the elderly; Perkin et al., 1988 for women) reported a significant FSP impact, and the direction of the effect was not consistent across studies.[43]

Only a few studies looked at the impact of FSP participation on the intake of carbohydrates, fat, or saturated fat. None of these studies, which assessed intake based on contribution to total energy intake rather than in absolute terms, reported significant differences in mean intakes of FSP participants and nonparticipants. Gleason et al. (2000) found, however, that preschool FSP participants were significantly less likely than comparably aged nonparticipants to meet the *Dietary Guidelines* recommendation of less than 10 percent of total energy from saturated fat.

[42]Gleason et al. (2000) used these methods to describe dietary intakes of low-income populations. However, in assessing differences in intakes of FSP participants and nonparticipants, they compared regression-adjusted percentages of individuals with intakes above specific RDA cutoffs rather than the percentage of individuals with usual intakes below the EAR.

[43]Gleason et al. (2000) found no significant differences in mean intakes of protein, expressed as a percentage of the RDA, for any of the three populations studied (preschool children, school-age children, and adults). For preschool children, however, they found that FSP participants consumed significantly less protein than nonparticipants, as a percentage of total energy intake.

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Food energy and macronutrients				
Food energy	***Children*** Fraker (1990) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {preschool} Perez-Escamilla (2000) [2 sites; P-N] Rose (1998a) [national; D-R] Cook (1995) [national; P-N] Gregorio (1984) [national; P-N] ***Elderly*** Fey-Yensan (2003) [1 State; P-N] Weimer (1998) [national; P-N] Posner (1987) [6 sites; P-N] Lopez (1987a) [national; P-N][1] Butler (1985) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Whitfield (1982) [1 city; D-R] Bishop (1992) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {school-age} West (1978) [1 State; D-R] Futrell (1971) [1 county; P-N] ***Elderly*** Lopez (1987a) [national; P-N][2] ***Women*** Fraker (1990) [national; P-N] Perkin (1988) [1 site; P-N]	***Elderly*** Butler (1996) [6 sites; D-R]

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

Outcome	Significant impact	No significant impact		Significant impact
	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Protein	***Children*** Fraker (1990) [national; P-N] ***All households*** Bishop (1992) [national; P-N]	***Children*** Rose (1998a) [national; D-R] Cook (1995 [national; P-N] Gregorio (1984) [national; P-N] ***Elderly*** Lopez (1987a) [national; P-N][1] Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Fraker (1990) [national; P-N] Perkin (1988) [1 site; P-N] {Blacks} ***Rural*** Butler (1996) [2 sites; D-R] ***All households*** Whitfield (1982) [1 city; D-R]	***Children*** Gleason (2000) [national; D-R][3] Perez-Escamilla (2000) [2 sites; P-N] West (1978) [1 State; D-R] ***Elderly*** Fey-Yensan (2003) [1 State; P-N] Weimer (1998) [national; P-N] Lopez (1987a) [national; P-N][2] ***Adults*** Dixon (2002) [national; P-N]	***Elderly*** Butler (1996) [6 sites; D-R] ***Women*** Perkin (1988) [1 site; P-N] {Whites}
Carbohydrates		***Children*** Gleason (2000) [national; D-R] Gregorio (1984) [national; P-N] {preschool}	***Children*** Gregorio (1984) [national; P-N] {school-age} ***Elderly*** Fey-Yensan (2003) [1 State; P-N] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Perkin (1988) [1 site; P-N]	

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Fat		***Children*** Gleason (2000) [national; D-R] {school-age} Rose (1998a) [national; D-R] Gregorio (1984) [national; P-N] ***Elderly*** Fey-Yensan (2003) [1 State; P-N] Weimer (1998) [national; P-N] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Basiotis (1998) [national; D-R][5]	***Children*** Gleason (2000) [national; D-R] {preschool} Perez-Escamilla (2000) [2 sites; P-N] ***Women*** Perkin (1988) [1 site; P-N]	
Saturated fat		***Children*** Gleason (2000) [national; D-R] {school-age} Rose (1998a) [national; D-R] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Perkin (1988) [1 site; P-N] {Whites} ***All households*** Basiotis (1998) [national; D-R][5]	***Children*** Gleason (2000) [national; D-R] {preschool}[4]	***Women*** Perkin (1988) [1 site; P-N] {Blacks}

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamins				
Vitamin A	***Children*** Rose (1998a) [national; D-R]	***Children*** Perez-Escamilla (2000) [2 sites; P-N] Cook (1995) [national; P-N] Gregorio (1984) [national; P-N] West (1978) [1 State; D-R] ***Elderly*** Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] Fraker (1990) [national; P-N] Futrell (1971) [1 county; P-N] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Perkin (1988) [1 site; P-N]	***Children*** Whitfield (1982) [1 city; D-R] ***Women*** Fraker (1990) [national; P-N]
Vitamin B_6	***Children*** Perez-Escamilla (2000) [2 sites; P-N]	***Children*** Gleason (2000) [national; D-R] {school-age} Rose (1998a) [national; D-R] Cook (1995) [national; P-N] ***Elderly*** Weimer (1998) [national; P-N] ***Women*** Fraker (1990) [national; P-N] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Adults*** Dixon (2002) [national; P-N] Gleason (2000) [national; D-R]	
Vitamin B_{12}	***Children*** Cook (1995) [national; P-N]	***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] Perez-Escamilla (2000) [2 sites; P-N] ***Adults*** Gleason (2000) [national; D-R]	

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamin C		***Children*** Gleason (2000) [national; D-R] Rose (1998a) [national; D-R] Cook (1995) [national; P-N] Fraker (1990) [national; P-N] Gregorio (1984) [national; P-N] {preschool} West (1978) [1 State; D-R] ***Elderly*** Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Perez-Escamilla (2000) [2 sites; P-N] Gregorio (1984) [national; P-N] {preschool} ***Elderly*** Weimer (1998) [national; P-N] ***Women*** Fraker (1990) [national; P-N] Perkin (1988) [1 site; P-N] ***Adults*** Dixon (2002) [national; P-N]	***All households*** Whitfield (1982) [1 city; D-R]
Vitamin E		***Children*** Gleason (2000) [national; D-R] {school-age}[1] Rose (1998a) [national; D-R] Cook (1995) [national; P-N] ***Elderly*** Weimer (1998) [national; P-N] ***Women*** Fraker (1990) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Adults*** Dixon (2002) [national; P-N] Gleason (2000) [national; D-R]	***Children*** Fraker (1990) [national; P-N]

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Folate	***Children*** Gleason (2000) [national; D-R] {school-age} Perez-Escamilla (2000) [2 sites; P-N] Cook (1995) [national; P-N]	***Children*** Rose (1998a) [national; D-R]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Adults*** Dixon (2002) [national; P-N] Gleason (2000) [national; D-R] ***Women*** Fraker (1990) [national; P-N]	
Niacin	***Children*** Rose (1998a) [national; D-R]	***Children*** Perez-Escamilla (2000) [2 sites; P-N] Cook (1995) [national; P-N] West (1978) [1 State; D-R] ***Elderly*** Weimer (1998) [national; P-N] Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Perkin (1988) [1 site; P-N] {Blacks} ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Perkin (1988) [1 site; P-N] {Whites}	***Elderly*** Butler (1996) [6 sites; D-R]
Pantothenic acid		***Children*** Perez-Escamilla (2000) [2 sites; P-N]		

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Riboflavin		***Children*** Gleason (2000) [national; D-R] {school-age} Perez-Escamilla (2000) [2 sites; P-N] Rose (1998a) [national; D-R] Cook (1995) [national; P-N] West (1978) [1 State; D-R] ***Elderly*** Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Women*** Perkin (1988) [1 site; P-N]	***Elderly*** Butler (1996) [6 sites; D-R]
Thiamin	***Children*** Rose (1998a) [national; D-R] Futrell (1971) [1 county; P-N]	***Children*** Gleason (2000) [national; D-R] {school-age} Perez-Escamilla (2000) [2 sites; P-N] Cook (1995) [national; P-N] West (1978) [1 State; D-R] ***Elderly*** Butler (1985) [6 sites; P-N] Posner (1987) [6 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Women*** Perkin (1988) [1 site; P-N] {Blacks}	***Women*** Perkin (1988) [1 site; P-N] {Whites}

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

Outcome	Significant impact	No significant impact		Significant impact
	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Minerals				
Calcium	***Children*** Cook (1995) [national; P-N] ***Elderly*** Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N]	***Children*** Gleason (2000) [national; D-R] {school-age} Rose (1998a) [national; D-R] Gregorio (1984) [national; P-N] Fraker (1990) [national; P-N] West (1978) [1 State; D-R] ***Elderly*** Weimer (1998) [national; P-N] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] {preschool} Perez-Escamilla (2000) [2 sites; P-N] Futrell (1971) [1 county; P-N] ***Elderly*** Butler (1996) [6 sites; D-R] Whitfield (1982) [1 city; D-R] ***Women*** Fraker (1990) [national; P-N] Perkin (1988) [1 site; P-N] ***Adults*** Dixon (2002) [national; P-N]	
Iron	***Children*** Perez-Escamilla (2000) [2 sites; P-N] Rose (1998a) [national; D-R] Rose (1995) [national; D-R] ***Elderly*** Lopez (1987a) [national; P-N][1] ***All households*** Whitfield (1982) [1 city; D-R][6]	***Children*** Gregorio (1984) [national; P-N] Cook (1995) [national; P-N] Fraker (1990) [national; P-N] West (1978) [1 State; D-R] ***Elderly*** Weimer (1998) [national; P-N] Posner (1987) [6 sites; P-N] Butler (1985) [6 sites; P-N] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] {school-age} ***Adults*** Gleason (2000) [national; D-R] ***Women*** Fraker (1990) [national; P-N] Perkin (1988) [1 site; P-N] ***Rural*** Butler (1996) [6 sites; D-R]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Elderly*** Butler (1996) [6 sites; D-R] Lopez (1987a) [national; P-N][2] ***Adults*** Dixon (2002) [national; P-N]

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Magnesium	***Children*** Cook (1995) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {school-age} Rose (1998a) [national; D-R] ***Elderly*** Weimer (1998) [national; P-N] ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Adults*** Dixon (2002) [national; P-N] Gleason (2000) [national; D-R] ***Women*** Fraker (1990) [national; P-N]	
Phosphorus		***Children*** Gleason (2000) [national; D-R] {school-age} Rose (1998a) [national; D-R] Cook (1995) [national; P-N] ***Women*** Perkin (1988) [1 site; P-N] {Blacks} ***All households*** Basiotis (1987) [national; D-R][6]	***Children*** Gleason (2000) [national; D-R] {preschool} West (1978) [1 State; D-R] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Perkin (1988) [1 site; P-N] {Whites} ***Elderly*** Weimer (1998) [national; P-N]	
Zinc	***Children*** Rose (1998a) [national; D-R] Cook (1995) [national; P-N] Fraker (1990) [national; P-N]	***Children*** Perez-Escamilla (2000) [2 sites; P-N] ***Adults*** Gleason (2000) [national; D-R] ***Women*** Fraker (1990) [national; P-N]	***Children*** Gleason (2000) [national; D-R] ***Elderly*** Weimer (1998) [national; P-N] ***Adults*** Dixon (2002) [national; P-N]	

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

Outcome	Significant impact: Participants consumed more	No significant impact: Participants consumed more/same	No significant impact: Participants consumed less	Significant impact: Participants consumed less
Other dietary components				
Cholesterol		***Children*** Rose (1998a) [national; D-R] ***Adults*** Gleason (2000) [national; D-R] ***All households*** Basiotis (1998) [national; D-R][5]	***Children*** Gleason (2000) [national; D-R] ***Elderly*** Posner (1987) [6 sites; P-N]	
Fiber			***Children*** Gleason (2000) [national; D-R]	***Adults*** Gleason (2000) [national; D-R]
Sodium		***Adults*** Gleason (2000) [national; D-R]	***Children*** Gleason (2000) [national; D-R]	***All households*** Basiotis (1998) [national; D-R][5]
Food Intake				
Fruits and fruit juices		***Children*** Gleason (2000) [national; D-R] ***Elderly*** Fey-Yensan (2003) [1 State; P-N] ***Adults*** Gleason (2000) [national; D-R]	***Children*** Perez-Escamilla (2000) [2 sites; P-N] ***Adults*** Dixon (2002) [national; P-N][7] ***All individuals*** Wilde (1999) [national; P-N] ***All households*** Basiotis (1998) [national; D-R]	
Grain products		***Children*** Perez-Escamilla (2000) [2 sites; P-N] ***Elderly*** Fey-Yensan (2003) [1 State; P-N] ***All households*** Basiotis (1998) [national; D-R][5]	***Children*** Gleason (2000) [national; D-R] {school-age} ***Adults*** Dixon (2002) [national; P-N] Gleason (2000) [national; D-R] ***All individuals*** Wilde (1999) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {preschool}

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

Outcome	Significant impact: Participants consumed more	No significant impact: Participants consumed more/same	No significant impact: Participants consumed less	Significant impact: Participants consumed less
Meat, poultry, fish, and meat substitutes	***All individuals*** Wilde (1999) [national; P-N] ***All households*** Basiotis (1998) [national; D-R][5]	***Children*** Perez-Escamilla (2000) [2 sites; P-N] {eggs} ***Elderly*** Fey-Yensan (2003) [1 State; P-N] ***Adults*** Dixon (2002) [national; P-N] Gleason (2000) [national; D-R]	***Children*** Gleason (2000) [national; D-R] Perez-Escamilla (2000) [2 sites; P-N] {fish and meats}	
Milk and milk products	***All households*** Basiotis (1998) [national; D-R]	***Children*** Gleason (2000) [national; D-R] {school-age} Perez-Escamilla (2000) [2 sites; P-N] ***Adults*** Dixon (2002) [national; P-N] Gleason (2000) [national; D-R] ***All individuals*** Wilde (1999) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Elderly*** Fey-Yensan (2003) [1 State; P-N]	
Vegetables	***All households*** Basiotis (1998) [national; D-R][5]	***Children*** Gleason (2000) [national; D-R] {preschool} Perez-Escamilla (2000) [2 sites; P-N] {all others} ***All individuals*** Wilde (1999) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {school-age} Perez-Escamilla (2000) [2 sites; P-N] {starchy} ***Elderly*** Fey-Yensan (2003) [1 State; P-N] ***Adults*** Dixon (2002) [national; P-N]	***Adults*** Gleason (2000) [national; D-R]
Added sugars	***All individuals*** Wilde (1999) [national; P-N]	***Children*** Gleason (2000) [national; D-R] {preschool} ***Adults*** Gleason (2000) [national; D-R]	***Children*** Gleason (2000) [national; D-R] {school-age}	

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Added fats	***All individuals*** Wilde (1999) [national; P-N]		***Children*** Gleason (2000) [national; D-R] ***Elderly*** Fey-Yensan (2003) [1 State; P-N][8] ***Adults*** Gleason (2000) [national; D-R]	
Alcoholic beverages			***Adults*** Gleason (2000) [national; D-R]	
Sweets and desserts		***Children*** Perez-Escamilla (2000) [2 sites; P-N]	***Children*** Bhattacharya (2000) [national; P-N] ***Elderly*** Fey-Yensan (2003) [1 State; P-N][8]	
High-fat snack foods		***Children*** Perez-Escamilla (2000) [2 sites; P-N]		

	Significant impact	No significant impact		Significant impact
Outcome	Participants scored higher	Participants scored higher/same	Participants scored lower	Participants scored lower
Summary measures				
Healthy Eating Index (HEI)	***All households*** Basiotis (1998) [national; D-R][9] ***Able-bodied adults without dependents (ABAWDS)*** Kramer-LeBlanc (1997) [national; D-R]		***Children*** Bhattacharya (2000) [national; P-N][10] Gleason (2000) [national; D-R] ***Adults*** Gleason (2000) [national; D-R]	***Adults*** Dixon (2002) [national; P-N]

See notes at end of table.

Continued—

Table 15—Findings from studies that examined the impact of the Food Stamp Program on dietary intakes of individuals—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants scored higher	Participants scored higher/same	Participants scored lower	Participants scored lower
Diet Quality Index[11]		***Children*** Gleason (2000) [national; D-R] {preschool} ***Adults*** Gleason (2000) [national; D-R]	***Children*** Gleason (2000) [national; D-R] {school-age}	

Notes: Cell entries show the senior author's name, the publication date, the scope of the study (for example, national vs. 1 city or 1 State), and the research approach (P-N = participant vs. nonparticipant study, D-R = dose response study). Where study findings pertain only to a specific subgroup, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Results for Akin (1985) and Akin (1987) not reported because detailed impact estimates were not provided.

Findings reported for Basiotis et al. (1998) are for effect of weekly FSP benefits. Model also included FSP participation dummy. Unless otherwise noted, direction and significance of coefficient for FSP participation was comparable.

Butler and Raymond (1996) reported detailed results only for energy and selected nutrients (protein and iron for the rural sample and protein, calcium, iron, and riboflavin for the elderly sample). The study also assessed vitamin A, thiamin, vitamin C, and phosphorus (rural sample only), and the authors reported that results for these other nutrients "were not qualitatively different" from results that were reported.

Fraker (1990) refers to Fraker, Long, and Post (1990). Findings reported for children are based on a bivariate model that controls for selection bias, one of three models used in the analysis and deemed by the authors to be the preferred model. Findings reported for women are based on OLS model, which was preferred by authors because small sample sizes compromised function of the bivariate selection-adjustment model.

[1] Results for analysis of NHANES-II data.

[2] Results for analysis of NHANES-I data.

[3] For preschool children, difference was not significant for mean protein intake as a percentage of the RDA, but was significant for the percentage of energy provided by protein.

[4] Difference was not significant for mean intake as a percentage of total energy, but was significant for the percentage of individuals who satisfied the *Dietary Guidelines* recommendation of less than 10 percent of total energy, with FSP participants being less likely to meet this goal.

[5] The coefficient for FSP participation was negative but not statistically significant.

[6] Authors reported statistically significant findings but no statistical tests were presented.

[7] Difference was not significant for HEI (24-hour recall) measure of food consumption but was significant for measure based on nonquantified food frequency.

[8] Authors used one measure for fats, oils, and sweets.

[9] The coefficient for FSP participation was negative and significant ($p < 0.05$), but the coefficient for weekly food stamp benefits was positive and significant ($p < 0.001$).

[10] Authors used an adapted HEI measure in which the food-based component scores were based on data from a nonquantified food frequency rather than a 24-hour recall.

[11] The Dietary Quality Index (DQI) is similar to the HEI in that it scores individuals' diets on the basis of how well they meet eight standards: percentage of calories from fat and saturated fat, intake of protein, cholesterol, sodium, and calcium, and intake of fruits and vegetables, grains, and legumes. The lower the score, the higher the quality of the diet.

Vitamins and Minerals

Few studies found that FSP participation was significantly related to intake of vitamins and minerals. Moreover, in keeping with the results observed for energy and protein, the direction of the FSP effect was not consistent across studies that did report significant results.

The largest number of significant effects were reported by authors who focused on preschool children. Three studies (Perez-Escamilla et al., 2000; Rose, et al., 1998a; Cook et al., 1995) reported that FSP participation increased children's intakes of several vitamins and minerals.

The Perez-Escamilla study, based on a small local sample, found that FSP participation was associated with increased energy-adjusted intakes of vitamin B_6, folate, and iron.

Rose and his colleagues analyzed data from the 1989-91 CSFII and found that FSP participation was associated with increased intakes of vitamin A, niacin, thiamin, iron, and zinc. The authors reported that they investigated the possibility of selection bias in their results and found "no evidence" of it. No information is provided, however, on how the issue was investigated and how the authors reached this conclusion.

Cook et al. (1995) analyzed data from the 1986 CSFII low-income supplement and compared the percentage of FSP children and nonparticipating children with average intakes below 70 percent of the RDA. This study did not use multivariate techniques to control for differences between the two groups; however, limitation of the sample to children in households under 125 percent of poverty provides at least some statistical control. The authors reported significant and positive FSP effects for a number of nutrients (vitamin B_{12}, folate, calcium, magnesium, and zinc).

Confidence in the findings from these studies is diminished by the small overlap in the significant effects reported. All three studies examined intakes of vitamin A, vitamin B_6, vitamin C, folate, niacin, riboflavin, thiamin, calcium, iron, and zinc. Of these, conclusions about the impact of the FSP were consistent across all three studies only for vitamin C and riboflavin. In both cases, the conclusion was that the FSP had no effect. For all of the other vitamins and minerals, one or two of the studies—but never all three—reported a significant FSP effect. The only nutrients for which there was any overlap in significant effects were folate (Perez-Escamilla et al., 2000; Cook et al., 1995), iron (Perez-Escamilla et al., 2000; Rose et al., 1998a), and zinc (Rose et al., 1998a; Cook et al., 1995). Rose and colleagues reported the same result for iron in an earlier paper (1995); that paper only looked at iron intake.

Findings from the more recent and methodologically rigorous study by Gleason et al. (2000) also raise doubts about FSP effects on preschool children. The only significant effect reported for preschool children in the Gleason et al. study was that FSP participants had a significantly lower intake of iron.[44,45]

Other Dietary Components

A handful of studies examined impacts of FSP participation on the intake of cholesterol, fiber, and/or sodium. Gleason et al. (2000) found that FSP adults consumed significantly less dietary fiber than nonparticipant adults. Basiotis and his colleagues found that sodium intake was significantly higher in FSP households than in nonparticipant households.

Food Intake

Six studies assessed the impact of FSP participation on food intake patterns on one or more population subgroups. Findings from the available studies are mixed but provide little indication that the FSP has a positive influence on food intake patterns. Using data from the most recent CSFII 1994-96, Gleason and his colleagues (2000) found that receiving FSP benefits was associated with significantly lower consumption of vegetables among adults and of grains among preschoolers.

Wilde and his colleagues (1999) used the same data as Gleason et al. but estimated impacts for all individuals in FSP households rather than for specific subgroups. They found that FSP participation was associated with significantly greater consumption of meat (considered a beneficial effect) as well as significantly greater intakes of added sugar and added fat (not considered beneficial).

Using data from an earlier wave of the CSFII (1989-91), Basiotis et al. (1998) found that the weekly value of FSP benefits was significantly and positively related to consumption of vegetables, milk and milk products, and meat. Other studies that examined FSP impacts on intake of specific types of food found no significant effects.

[44]However, the percentage of FSP and non-FSP preschool children with iron intakes equivalent to 70 percent of the RDA was not significantly different.

[45]Gleason et al. (2000) reported a significant FSP effect for folate intake among school-age children, but intakes among preschool children were not significantly different.

Summary Measures

Several authors examined impact of the FSP on overall diet quality, using the Healthy Eating Index (HEI). The HEI, developed by USDA's Center for Nutrition Policy and Promotion (CNPP), is a summary measure of overall diet quality (Kennedy et al., 1995). The index is comprised of 10 component scores that are weighted equally in the total score. Five of the component scores are food-based and evaluate food consumption compared with the Food Guide Pyramid recommendations. Four component scores are nutrient-based and assess compliance with recommendations for maximum daily intake of fat, saturated fat, cholesterol, and sodium. The 10th component score assesses the level of variety in the diet.[46] Gleason et al. (2000) also examined FSP impacts on an HEI-like summary measure known as the Dietary Quality Index (DQI).

Findings from the available studies are mixed and, giving precedence to the Gleason et al. (2000) study, provide little evidence that FSP participation influences overall dietary quality. Dixon (2002) found that HEI scores for FSP adults were significantly lower than HEI scores for nonparticipant adults. Dixon did not limit her sample to low-income individuals, however, and her model controlled for relatively few measured characteristics.

Other Nutrition and Health Outcomes

The literature search identified a relatively limited number of studies that investigated the impact of the FSP on other nutrition- and health-related outcomes. (Note that studies that examined shopping patterns—such as, types of stores used and food expenditure shares—have been excluded from this review because of their tenuous relationship to nutritional status.) Characteristics of these studies are summarized in table 16.

Outcomes examined in this research include food security (14 studies), birthweight (2 studies), weight and/or height (6 studies), nutritional biochemistries (3 studies), and general measures of nutrition and/or health status (2 studies). (Some studies looked at multiple outcomes). The research on food security includes participant vs. nonparticipant, dose-response, and cashout studies. Research on all of the other outcomes is limited to participant vs. nonparticipant comparisons, although some of these studies included longitudinal as well as cross-sectional data.

The following sections summarize findings for each outcome. Drawing firm conclusions about FSP impacts, with the possible exception of the impact on food security, is not possible. The number of studies available for any given outcome and population subgroup is limited, and each study has important limitations.

Food Security

The relationship between FSP participation and food security is a complex one. Food insecurity is likely to lead households to seek food assistance, and receipt of food stamp benefits may subsequently improve the household's food security. This situation makes estimates of FSP impacts on food security particularly vulnerable to problems of selection bias and reverse causality.

This difficulty is apparent in conflicting findings reported in the literature. Most participant vs. nonparticipant studies found that FSP participants were more likely to be food insecure than nonparticipants. (Jensen, 2002; Cohen et al., 1999; Alaimo et al., 1998; Hamilton et al., 1997; Cristofar and Basiotis, 1992; Kisker and Devaney, 1988).

On the other hand, Rose et al. (1998b), using a dose-response approach, found that food insufficiency was inversely related to the size of the food stamp benefit and the relationship was stronger than the relationship between food insufficiency and other incomes. A comparable pattern was reported by Cristofar and Basiotis (1992) in a model that included all households. (Food stamp benefits did not have a significant effect in a separate model that was limited to households with preschool children).

Three of the cashout studies (Alabama "pure," Alabama ASSETS, and San Diego) also considered food security. In the Alabama ASSETS demonstration, members of the cashout group were significantly more likely to have skipped a meal due to lack of food or money to buy food (Davis and Werner, 1993).

Two recent studies that used sophisticated techniques to control for selection bias help clarify the relationship between FSP participation and food security. Both found that, once one controlled for selection bias, there was no evidence of significantly greater levels of food insecurity (or insufficiency) among FSP participants. The analysis completed by Gundersen and Oliveira

[46]Results for component scores, when reported, have been summarized in preceding sections of table 15.

Table 16—Studies that examined the impact of the Food Stamp Program on other nutrition and health outcomes

Study	Data source[1]	Population sample (sample size)	Design	Measure of participation	Analysis method
Food security: Participant vs. nonparticipant comparisons					
Huffman and Jensen (2003)	1997 longitudinal SPD and 1998 experimental SPD	Low-income households (n=3,733)	Participant vs. nonparticipant	Participation dummy	Simultaneous equation model with 3 probits
Jensen (2002)	2000 April FSS-CPS	FSP and FSP-eligible households (n=6,300)	Participant vs. nonparticipant	Participation dummy	Bivariate ordered probit model
Gunderson and Oliveria (2001)	1991 and 1992 SIPP	Low-income households (n=3,452)	Participant vs. nonparticipant	Participation dummy	Simultaneous equation model with 2 probits
Bhattacharya and Currie (2000)	1988-94 NHANES-III	Youth ages 12-16 (n=1,358)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Perez-Escamilla et al. (2000)	2 pediatric clinics in low-income areas of Hartford, CT (1999)	Children ages 8 months to 5 years who were participating in WIC or had participated in past year (n=99)	Participant vs. nonparticipant	Participation dummy	Chi-square analysis
Cohen et al. (1999)	1996-97 NFSPS	Low-income households (n=3,228)	Participant vs. nonparticipant	Participation dummy	Comparisons of proportions
Alaimo et al. (1998)	1988-94 NHANES-III	Low-income households (n=5,285)	Participant vs. nonparticipant	Participation dummy	Logistic regression (survey weights)
Hamilton et al. (1997)	1995 CPS	Low-income households (n=21,810)	Participant vs. nonparticipant	Participation dummy	Comparison of proportions
Cristofar and Basiotis (1992)	1985-86 CSFII-LI	Low-income women (n=3,398) and low-income children ages 1-5 years (n=1,930)	Participants vs. nonparticipant	Participation dummy; benefit amount	Multivariate regression
Kisker and Devaney (1988)	1979-80 NFCS-LI	Low-income (n~2,900)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests

See notes at end of table.

Continued—

Table 16—Studies that examined the impact of the Food Stamp Program on other nutrition and health outcomes—Continued

Study	Data source[1]	Population sample (sample size)	Design	Measure of participation	Analysis method
Food security: Dose-response estimates					
Rose et al. (1998b)	1989-91 CSFII and 1992 SIPP	All households (n=6,620 and n=30,303)	Dose-response	Annual dollar amount of food stamps	Logistic regression
Food security: Cashout demonstrations					
Fraker et al. (1992)	Alabama cashout demonstration (1990)	FSP participants (n=2,386)	Random assignment of participants to check or coupon	Group membership dummy and benefit amount	Multivariate regression
Ohls et al. (1992)	San Diego cashout demonstration (1990)	FSP participants (n=1,143)	Random assignment of participants to check or coupon	Group membership dummy and benefit amount	Multivariate regression
Davis and Werner (1993)	Alabama ASSETS demonstration (1990)	ASSETS and FSP participants (n=1,371)	Comparison of treatment and matched comparison counties	Group membership dummy and benefit amount	Multivariate regression
Birthweight: Participant vs. nonparticipant comparisons					
Korenman and Miller (1992)	1979-88 NLSY	Infants born to poor women with 2 births between 1979 and 1988 (n~2,568)	Participant vs. nonparticipant	Participation dummy	Multivariate regression; fixed-effects models
Currie and Cole (1991)	1979-87 NLSY	Infants born to poor, young women (n~4,900)	Participant vs. nonparticipant	Participation dummy	Multivariate 2-stage least squares and fixed-effects model
Weight and/or height: Participant vs. nonparticipant comparisons					
Fey-Yensan et al. (2003)	Low-income areas in Connecticut (1996-97)	Low-income elderly living in subsidized housing (82% female) (n=200)	Participant vs. nonparticipant	Participation dummy	Chi-square tests and analysis of variance
Gibson (2003)	1985-96 NLSY	Low-income women, ages 20-40 (n=13,390)[2]	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Jones et al. (2003)	1997 PSID-CDS	Children ages 5-12 from households with incomes <185% of poverty	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Gibson (2001)	1997 NLSY-child supplement	Youth ages 12-17 (n=7,920)	Participant vs. nonparticipant	Participation dummy	Multivariate regression

See notes at end of table.

Continued—

Table 16—Studies that examined the impact of the Food Stamp Program on other nutrition and health outcomes—Continued

Study	Data source[1]	Population sample (sample size)	Design	Measure of participation	Analysis method
Bhattacharya and Currie (2000)	1988-94 NHANES-III	Youth ages 12-16 (n=1,358)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Korenman and Miller (1992)	1986 and 1988 NLSY-child supplement	Children ages 0-7 (n=6,598)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Nutritional biochemistries: Participant vs. nonparticipant comparisons					
Dixon (2002)	1988-94 NHANES-III	Adults ages 20 and older (n=10,545)	Participant vs. nonparticipant (albumin, hemoglobin, serum iron, vitamin C, vitamin E, carotenoids)	Participation dummy	Multivariate regression
Bhattacharya and Currie (2000)	1988-94 NHANES-III	Youth ages 12-16 (n=1,358)	Participant vs. nonparticipant (iron, cholesterol, vitamin A, vitamin C, vitamin E)	Participation dummy	Multivariate regression
Lopez and Habicht (1987b)	1971-73 NHANES-I and 1976-80 NHANES-II	Low-income elderly (n=1,684, NHANES-I) and (n=1,388, NHANES-II)	Participant vs. nonparticipant (iron)	Participation dummy	Multivariate ANOVA
General measures of nutrition or health status: Participant vs. nonparticipant comparisons					
Fey-Yensan et al. (2003)	Low-income areas in Connecticut (1996-97)	Low-income elderly living in subsidized housing (82% female) (n=200)	Participant vs. nonparticipant	Participation dummy	Chi-square tests and analysis of variance
Gibson (2001)	1997 NLSY	Youth ages 12-17 (n=7,920)	Participant vs. nonparticipant	Participation dummy	Multivariate regression

[1]Data sources:
ASSETS = Avenues to Self-Sufficiency through Employment and Training Services.
FSS-CPS = Food Security Supplement of the Current Population Survey.
CPS = Current Population Survey.
CSFII = Continuing Survey of Food Intakes by Individuals.
CSFII-LI = Continuing Survey of Food Intakes by Individuals - Low-Income Samples.
NFCS-LI = Nationwide Food Consumption Survey - Low Income Supplement.
NFSPS = National Food Stamp Program Survey.
NHANES = National Health and Nutrition Examination Survey.
NLSY = National Longitudinal Survey of Youth.
PSID-CDS = Panel Study of Income Dynamics - Child Development Supplement.
SIPP = Survey of Income and Program Participation.
SPD = Survey of Program Dynamics.
[2]Multiple observations for each person, collected annually between 1979 and 1994 and biannually thereafter. Sample size represents person-years.

(2001) used data from the 1991 and 1992 SIPP panels and used a simultaneous equation model with two probits. The analysis examined reported levels of food insufficiency using the so-called "USDA food insufficiency question" that preceded the 18-item Federal food security module, the currently accepted standard for measuring household and individual food security (Price et al., 1997; Bickel et al., 2000). Huffman and Jensen (2003) expanded on the work done by Gundersen and Oliveira, incorporating information on labor force participation decisions and using the more severe outcome of food insecurity with hunger based on the 18-item Federal food security module. These authors also simulated the effects of changes in FSP benefits, unemployment rate, and non-labor income and found that FSP benefits were more effective in reducing levels of food insecurity with hunger than pure cash transfers. Future efforts to understand the impact of FSP participation on food security may benefit from a longitudinal approach that measures changes for households over time.

Birthweight

Two of the identified studies examined the impact of FSP participation on birthweight. Currie and Cole (1991) used data from the National Longitudinal Survey of Youth (NLSY) to investigate effects on infant birthweight of women's participation in the FSP and other means-tested programs during pregnancy. In addition to standard multivariate regressions, the authors estimated fixed-effects models, looking at birthweights of sibling pairs. Using an instrumental variables approach to control for self-selection, they found no significant effect of a mother's FSP participation on the likelihood that her infant would weigh at least 6 pounds.

Korenman and Miller (1992) completed an analysis that used the same data as Currie and Cole and similar analytic techniques. However, they estimated impacts for "very poor" women, those with incomes between zero and 50 percent of the poverty line, and "less poor women," those with incomes between 50 and 100 percent of poverty. In addition, they did not use instrumental variables and they adjusted NLSY income measures to exclude the value of FSP income. Findings from a fixed-effects model indicated that FSP participation was associated with a decreased likelihood of low birthweight (less than 5.5 pounds) among very poor women ($p < 0.10$). The authors reported this as a statistically significant finding, noting that the sample available for the fixed effects logit model of low birthweight (n=153) was small (and therefore had limited statistical power) because the two births in the sibling pair had to differ with respect to the outcome in order to be included in the model.

Weight and/or Height

Six of the identified studies assessed the impact of FSP participation on weight and/or height. Two studies examined linear growth and/or the prevalence of underweight among children. Five studies focused on the prevalence of overweight or obesity among children (1 study), adolescents (2 studies), adults (1 study), and the elderly (1 study). Gibson (2001) examined the prevalence of both underweight and overweight among adolescents.

Children and Adolescents

Korenman and Miller (1992) used NLSY data to examine the prevalence of stunting (defined as height-for-age below the 10th percentile on NCHS growth curves) and wasting (defined as weight-for-height below the 10th percentile) among infants and children up to age 7. The sample included children born between 1981 and 1987 who had height and weight measured in at least one of the NLSY Child Supplements (1986 or 1988).[47] Models, which did not control for selection bias, were estimated to look at both short-term and long-term effects of poverty and FSP participation. In models that controlled only for current income and FSP receipt during the year preceding the measurement, no significant FSP effect was found.

In a model that controlled for long-term poverty (measured by the average income-to-needs ratio of the mother over the 10-year NLSY time span), a modest but significant effect on stunting was found, with FSP participants more likely to be stunted. The authors speculated that the positive relationship between stunting and FSP receipt may reflect aspects of long-term economic deprivation that were not adequately captured in the model. A related analysis lends some credence to this hypothesis: Children who received FSP benefits for a portion of the years they were in poverty were significantly less likely to be wasted than children with a comparable poverty history who never received food stamps.

Bhattacharya and Currie (2000) used data from NHANES-III to examine the relationship between FSP

[47]The researchers pooled data for the 1986 and 1988 supplements, with the result that more than one observation was included for some sample members. They appropriately caution that this feature leads to overstated levels of significance because repeat measures for individual children are likely to be more highly correlated than measurements across children.

participation and obesity among youth between the ages of 12 and 16. They compared the proportion of youth who were obese, based on Body Mass Index (BMI).[48] Cutoffs were adapted from standards defined for adults. No FSP effect was detected.

Gibson (2001) used data from NLSY97 to examine the relationship between FSP participation and the likelihood that youth between the ages of 12 and 18 would be underweight or obese. Like Bhattacharya and Currie, Gibson used BMI to classify subjects and based her cutoffs on standards defined for adults. She estimated models that examined the impact of current FSP receipt and current income as well as models that controlled for long-term poverty. In the models that looked at current FSP participation, FSP participation was associated with a significant decrease in the likelihood that a youth would be obese. In the model that controlled for long-term poverty, this association was no longer significant. The authors did not attempt to control for selection bias because “it is difficult to come up with an appropriate instrument for Food Stamp receipt.”

Jones et al. (2003) looked at the relationship between food security, participation in FANPs, including the FSP, and the risk of overweight among children 5-12 in low-income households (<185 percent of poverty). The authors used data from the 1997 Panel Study of Income Dynamics (PSID) Child Development Supplement. Risk of overweight was defined as BMI-for-age at or above the 85th percentile on BMI-for-age charts designed specifically for use with children and adolescents. Weights were reported by primary caregivers, and heights were measured by field interviewers. The authors indicated that approximately 86 percent of the children had been weighed within the preceding month and that 16 percent of caregivers had to estimate weight because they had no recent reference point.

The analysis compared the risk of overweight among children living in food-secure and food-insecure households, while controlling for participation in a number of FANPs as well as other relevant characteristics. Results showed that FSP participation did not affect the likelihood that males would be overweight, regardless of whether they lived in food-secure or food-insecure households. Among females, however, those who participated in the FSP had a significantly reduced odds of being at risk of overweight, compared with those who did not participate in the FSP. This was true for females living in both food-secure and food-insecure households.

All of these results are subject to selection-bias problems, an important consideration in any attempt to link weight status to participation in a food assistance program. In addition, results of both Bhattacharya and Currie (2000) and Gibson (2001) should be interpreted with caution because the BMI cutoffs used in their analyses were adapted from standards developed for adults rather than from the BMI-for-age charts developed specifically for use with children and adolescents (Kuczmarski, 2000). The use of self-reported weights in the Jones et al. (2003) study is a concern. It is doubtful that cross-sectional studies can adequately address questions about program impacts on children’s weights and heights. Indeed, researchers who attempted to assess the impact of the WIC program on these outcomes concluded that a longitudinal study with serial measurements was essential (Puma et al., 1991).

Adults

Gibson (2003) used panel data from the 1985-96 waves of the NLSY to assess the relationship between FSP participation and obesity (BMI $\geq$ 30) among adults ages 20-40. Her analysis included measures of both current and long-term FSP participation. The sample was restricted to FSP participants and nonparticipating individuals residing in households that were income-eligible for the FSP.[49] Data on height and weight were self-reported.

Ordinary least squares models were estimated with and without fixed effects. Preliminary results showed that current and long-term FSP participation was significantly related to the prevalence of obesity among women, but not among men. For this reason, the detailed analysis focused exclusively on women. Four different fixed effects models were estimated with slightly different specifications. Results were largely consistent across models and indicated that, among low-income women, current participation in the FSP was associated with an increase in the predicted probability of current obesity of 2 percentage points (a 9-percent increase). Participation in the FSP in each of the previous 5 years, compared with no participation

[48]Body Mass Index (BMI) is the accepted standard for classifying adiposity (or fatness) in adults (Barlow and Dietz, 1998). Since 2000, BMI-for-age has also been recommended as a screening tool for children over the age of 2 (Kuczmarski et al., 2000).

[49]The income cutoff for nonparticipants was defined as a family income-to-needs ratio of no more than 2, relative to defined income eligibility criteria. This cutoff ensured that the panel included individuals who crossed in and out of poverty and FSP eligibility (and perhaps FSP participation), but remained near-poor when ineligible.

over that period, was associated with an increase in the predicted probability of current obesity of 4.5 percentage points, or roughly 21 percent.

To test the sensitivity of her results, Gibson reestimated all of the models using two different samples. She also estimated models that included controls for change in FSP eligibility and marital status in the previous calendar year as well as the timing of recent pregnancies-events that might trigger FSP participation. Finally, she examined the impact of current and long-term participation in AFDC (as an alternative indicator of “social program participation”). No detailed data were presented, but the author reported that estimates for all alternative models were similar to the main analysis in both magnitude and significance.

Although carefully designed and implemented, Gibson’s analysis remains open to problems of selection bias and reverse causality. The fact that the analysis did not include information on food security status (because the data are not available in the NLSY) is also a concern. Other research has found a significant and positive association between food insecurity and the prevalence of overweight (see, for example, Townsend et al., 2001). A number of theories have been proposed to explain the apparently paradoxical relationship between food insecurity and overweight (see Gibson, 2003; Townsend et al.), but none has been thoroughly tested.

Elderly

Fey-Yensan et al. (2003) studied a small group of low-income elderly individuals in Connecticut. They reported that a greater percentage of FSP participants than nonparticipants had BMIs $27. The analysis was based on simple chi-square comparisons, however, and data on height and weight were self-reported.

Nutritional Biochemistries

Lopez and Habicht (1987b) examined a variety of measures of iron status among low-income elderly individuals in NHANES-I and NHANES-II. Differences between FSP participants and nonparticipants were not statistically significant. Moreover, differences were inconsistent in direction, in some cases suggesting that elderly FSP participants had better iron status than nonparticipants (total iron binding capacity, free erythrocyte protoporphyrin), and in other cases suggesting the opposite effect (hemoglobin, hematocrit, transferrin saturation, and serum iron).

Bhattacharya and Currie (2000) and Dixon (2002) both used data from NHANES-III to assess the impact of the FSP on a number of different nutritional biochemistries. Bhattacharya and Currie focused on youths ages 12-16 and examined the prevalence of anemia (based on low levels of hemoglobin or hematocrit), as well as the prevalence of high serum cholesterol and low serum levels of vitamins A, C, and E, among FSP participants and nonparticipants. No significant differences were detected.

Dixon’s analysis focused on adults 20 and older. She compared the percentage of individuals with low serum levels of albumin, hemoglobin, iron, vitamin C, vitamin E, and carotenoids. She reported significant differences between FSP participants and nonparticipants for albumin, vitamin C, and carotenoids. As noted previously, however, Dixon did not limit her sample to low-income individuals and her model controlled for relatively few measured characteristics.

General Measures of Nutrition or Health Status

Two of the identified studies assessed the impact of FSP participation on general measures of nutrition or health status. In her 2001 analysis of NLSY97 data, described in a preceding section, Gibson examined self-reported health status and the prevalence of chronic disease (as reported by parents or other primary caregivers) among youths ages 12-18. Results showed that FSP participation was not significantly related to either outcome.

Fey-Yensan et al. (2003) examined self-reported general health status, self-reported functional status, and nutritional risk in a small group of low-income elderly individuals in Connecticut. Nutritional risk was measured using the Nutrition Screening Initiative (NSI) Checklist.[50] The authors found no significant differences between groups in general health status or functional status. They did find, however, that FSP participants had a significantly greater mean score on the NSI checklist (signifying a greater level of nutritional risk) than either income-eligible or higher income nonparticipants. The authors also reported that FSP participants were more likely than nonparticipants to report having fewer than two meals per day or not having enough money to buy food. As noted above, however, this study used simple chi-square analyses. Therefore, findings are suggestive only.

[50]The NSI is a national collaborative effort of professional organizations committed to identifying and treating nutritional problems among the elderly. Leading sponsors include the American Academy of Family Physicians, the American Dietetic Association, and the National Council on Aging. See www.aafp.org/nsi.xml.

Summary

The FSP provides benefits earmarked for at-home food consumption to low-income households of all types. A substantial body of literature establishes firmly that, while the greater part of food stamp benefits given to households are used to free up resources to spend on things other than food, FSP benefits do cause households to spend more on food than they otherwise would. Moreover, the San Diego cashout demonstration established firmly that the use of earmarked food stamp benefits leads to a greater increase in expenditures for at-home food than would occur if households received the same benefit amount as unconstrained cash supplements.

It seems likely that the FSP increases the availability of food energy and protein at the household level. Both of these effects were documented in a number of different studies, including the San Diego cashout study. The FSP may also increase the availability of a number of vitamins and minerals; however, the evidence in this area is weaker. The strongest study that reported significant effects on household availability of vitamins and minerals used data that were collected in the 1970s, before elimination of the purchase requirement. The San Diego cashout study found that FSP coupon households had greater availability of a number of vitamins and minerals than cash households, but the differences were not statistically significant.

The research shows little evidence that the FSP consistently affects the dietary intakes of individuals. There are scattered indications that FSP participation may improve vitamin and mineral intakes of young children, but these findings were not replicated in the most recent and well-conducted analysis. Moreover, limitations in measurement techniques and nutrition standards used in existing research make it impossible to adequately address the critical research question of whether the prevalence of inadequate nutrient intakes differs for FSP participants and nonparticipants.

Only a few studies looked at the impact of FSP participation on the intake of carbohydrates, fat, saturated fat, cholesterol, sodium, or fiber or on patterns of food intake. For the most part, these studies found little evidence of FSP impacts. Gleason et al. (2000), the strongest study completed to date, found that preschool FSP participants ate significantly fewer servings of grains and grain products than comparably aged nonparticipants and were significantly less likely to meet the *Dietary Guidelines* recommendation of less than 10 percent of total energy from saturated fat. This study also found that FSP adults ate significantly fewer servings of vegetables and less dietary fiber than nonparticipating adults.

Studies that looked at the impact of the FSP on food security have reported conflicting results. Some found that FSP participants were more likely than other low-income households to experience food insecurity. Other studies reported an inverse relationship-that FSP participants were less likely than nonparticipants to be food insecure. The relationship between FANP participation and food security is a complex one and is particularly vulnerable to problems of selection bias and reverse causality. Food insecurity is likely to lead households to seek food assistance, and receipt of food stamp benefits may subsequently improve the household's food security.

Two recent studies that used sophisticated techniques to attempt to control for selection bias suggest that, once selection bias is controlled for, FSP participants are no more likely to suffer from food insecurity (or insufficiency) than nonparticipants. Moreover, one of the studies suggested that FSP benefits are more effective in reducing levels of food insecurity with hunger than pure cash transfers.

Relatively little research has considered FSP impacts on other nutrition- and health-related outcomes. Moreover, the number of studies available for any given outcome and population subgroup is limited, and each study has important limitations.

The pattern of extant research suggests some paths for future research. There seems little need to document further the relationship between food stamp benefits and at-home food expenditures. However, given the increasing role that foods consumed away from home play in the diets of most Americans (Lin et al., 1999), a more detailed examination of the impacts of the FSP on expenditures for away-from-home food may be useful.

In general, the impact of the FSP on nutrient availability at the household level is of less interest than the impact on individual intakes. However, household availability is a more stable measure than individual intake and, therefore, has the potential for providing valuable information about the impact of the FSP. Future inquiries in this area should examine impacts associated with food use both at home and away from home.

Updated and improved studies of FSP impacts on individual dietary intakes are also needed because so many of the previous studies are dated, inconclusive, and used dietary assessment methods that are not consistent with currently recommended practices (see IOM, 2001). Improved assessment of dietary intakes will increase the likelihood that studies can detect small but meaningful FSP impacts.

Given the increasing problem of overweight and obesity in the United States, additional research on the relationship between FSP participation and patterns of overweight and obesity is desirable. Ideally, height and weight data should be measured rather than self-reported. Such research should include measures of food security as well as other variables that may be associated with weight status and should include careful attempts to control for self-selection.

In addition, ongoing efforts to expand nutrition education in the FSP should be continued and evaluated. If the FSP is to influence dietary intakes of individual participants and, thus associated outcomes, such as bodyweight and other aspects of nutritional status, the program must provide effective nutrition education to participants or find ways to connect FSP participants with nutrition education activities sponsored by other programs and agencies.

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Chapter 4

WIC Program

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) was established to provide "supplemental nutritious food as an adjunct to good health care during critical times of growth and development, in order to prevent the occurrence of health problems and improve health status..." (P.L. 95-627).[51] The WIC program targets five specific groups: pregnant women, infants, children up to their fifth birthday, breastfeeding women (up to 1 year after an infant's birth), and non-breastfeeding postpartum women (up to 6 months after an infant's birth). In addition to belonging to one of these target groups, WIC participants must be low-income and have one or more documented nutritional risks.

WIC offers a combination of services, including supplemental foods that have been specifically selected to supply nutrients potentially lacking in participants' diets, nutrition education, and referrals to health care and social services. WIC services do not fluctuate by household income. All participants have access to the same basic benefits. The types and amounts of supplemental food provided to each participant are based on participant category, age (for infants), and individual needs and preferences.

WIC is not an entitlement program, so the number of participants served by the program may be affected by Federal funding levels. In FY 2002, WIC served 7.5 million participants per month at an estimated total cost of $4.3 billion (U.S. Department of Agriculture (USDA), Food and Nutrition Service (FNS), 2003a).

Program Overview

A major impetus for the WIC program was the 1969 White House Conference on Food, Nutrition, and Health, which reported nutritional deficiencies among low-income pregnant women and young children. WIC began as a 2-year pilot program in 1972 and was authorized as a permanent program in 1975 (P.L. 94-105). In the intervening years, WIC has grown substantially and has become a key component of the nutrition safety net provided for low-income Americans.

[51]WIC was formerly known as the Special Supplemental Food Program for Women, Infants, and Children. The program name was changed under the Healthy Meals for Healthy Americans Act of 1994 (P.L. 103-448) to emphasize that WIC is a targeted supplemental nutrition program rather than an income supplement program.

Program growth was particularly rapid during the first decade of operation. Between FY 1975 and FY 1985, WIC participation increased from 344,000 participants per month to more than 3.1 million. On average, participation increased about 26 percent per year (USDA/FNS, 2003a). During the next decade, the program continued to grow each year but at a notably slower pace. Total monthly participation increased from about 3.1 million in FY 1985 to 6.9 million in FY 1995. The annual increase during this period averaged about 8 percent. Since the late 1990s, WIC participation has stabilized. Participation actually declined by 1-2 percent in 3 consecutive years between FY 1998 and FY 2000, but has increased modestly (2-3 percent per year) since then.

Much of WIC's growth over the years has been fueled by favorable Congressional funding, which has been influenced at least partially by research suggesting that WIC participation during pregnancy increases infant birthweight and decreases Medicaid costs. In the early- to mid-1990s, program growth was also fueled by infant formula rebate programs, which became mandatory in 1989 (P.L. 101-147). Under the rebate programs, each State awards a competitively bid contract to one infant formula manufacturer. For the exclusive contract on WIC infant formula, manufacturers agree to provide rebates to WIC State agencies for each can of formula purchased by WIC participants. The funds received through the rebate system are used to contain overall costs and to support provision of program benefits to additional participants. In FY 2002, the WIC program recognized $1.5 billion in rebate savings (USDA/FNS, 2003b).

Program Administration

FNS and its seven regional offices provide cash grants to State WIC agencies, issue regulations, and monitor compliance with these regulations. State WIC agencies operate in each of the 50 States, as well as in the District of Columbia, Puerto Rico, Guam, American Samoa, and the American Virgin Islands. Thirty-three Indian Tribal Organizations also serve as State WIC agencies (USDA/FNS, 2003b). State WIC agencies contract with local WIC agencies to provide WIC benefits to participants, monitor compliance with regulations, and provide technical assistance to local agency staff.

Funds allocated to local WIC agencies are used to provide supplemental foods to WIC participants and to pay administrative costs, including the costs of certifying applicants as eligible and providing nutrition education. Each of the roughly 2,200 local WIC agencies operates one or more service delivery sites where participants go to receive WIC services (Bartlett et al., 2002). Most of the local agencies are State, county, or local health departments. Other organizations, however, such as hospitals, State- or locally sponsored maternal and child health programs, and community action agencies, also provide WIC services.

Participant Eligibility

WIC eligibility is based on four factors: State residence, categorical eligibility, income eligibility, and nutritional risk. Unless they are part of a migrant farm worker family, WIC participants must be residents of the State or other jurisdiction (U.S. territory or Indian reservation) supplying the WIC benefits.

Participants must also belong to one of five categorically eligible groups—women during pregnancy and up to 6 weeks after delivery, breastfeeding women (who can participate for up to a year after giving birth), nonbreastfeeding postpartum women (who can participate for up to 6 months after giving birth or other termination of pregnancy), infants (0-12 months), and children up to the age of 5. In April 2002, 50 percent of all WIC participants were children and 26 percent were infants. The remaining 24 percent were women—11 percent pregnant women, 8 percent postpartum nonbreastfeeding women, and 6 percent breastfeeding women (Bartlett et al., 2003; Kresge, 2003).

Income eligibility for the WIC program is defined by each State agency. The cutoff may not be more than 185 percent or less than 100 percent of the Office of Management and Budget's (OMB) poverty income guidelines. As of April 2000, all State agencies used an income eligibility cutoff of 185 percent of poverty (Bartlett et al., 2002). Program regulations allow local WIC agencies to determine that participants are adjunctively income-eligible for WIC if they or certain family members participate in Medicaid, Temporary Assistance for Needy Families (TANF), or the Food Stamp Program (FSP). Since October 1998, applicants not certified under adjunctive income-eligibility provisions must present documentation of income at certification (P.L. 105-336). Before this regulation went into effect, some States allowed applicants to self-report income without documentation.

Since the mid-1980s, several legislative actions have expanded Medicaid income eligibility for pregnant women, infants, and children. As a result, some States have adopted Medicaid income-eligibility limits that exceed the WIC maximum of 185 percent of poverty. In October 2002, 17 States had Medicaid eligibility standards that exceeded the WIC cutoff (National Governor's Association, 2003). In most cases, the expanded income-eligibility cutoff is 200 percent of poverty and is limited to pregnant women and/or infants.

In addition to meeting eligibility requirements associated with residency, participant category, and income, each WIC participant must be at nutritional risk, as documented by a competent professional authority (a physician, nutritionist, nurse, or other health professional). Before 1999, State agencies established their own nutritional risk criteria following broad guidelines in Federal regulations. This autonomy meant that the criteria used to define nutritional risk and, consequently, program eligibility, varied across State agencies. This variability raised concerns about equity. To address these concerns, FNS asked the Institute of Medicine (IOM) to review the scientific basis for the criteria being used to define nutritional risk and to recommend about appropriate criteria for future use (IOM, 1996). The IOM report formed the basis for a standardized list of nutritional risk criteria to be used in all WIC programs nationwide. States are still free to define the specific criteria used to determine program eligibility, but, since April 1999, criteria must be selected from the approved list.

As noted previously, WIC is not an entitlement program. The program must operate within annual funding levels established by Congress. The number of participants served each year depends on available funding and the cost of running the program. To deal with the possibility that local programs may not be able to serve all eligible people, WIC uses a priority system to allocate available caseload slots to eligible applicants. The priority system is designed to ensure that available services go to those most in need. In general, pregnant women, breastfeeding women, and infants are given higher priority than children and nonbreastfeeding postpartum women. In addition, applicants with nutritional risks that are based on hematologic measures, anthropometric measures, or medical conditions are given higher priority than applicants with nutritional risks based on dietary patterns or other characteristics.[52]

[52]See 7 Code of Federal Regulations (CFR), 246.7.

The relative importance of the priority system has declined over time as increasing funds have allowed the program to serve many lower-priority individuals. Between 1988 and 1997, favorable Congressional funding and cost-containment measures (especially formula rebates) fueled an overall increase of 106 percent in WIC participation. Participation increased more substantially for children than for higher-priority groups (128 percent vs. 110 percent for women and 70 percent of infants). The reason for the disparity was that a large percentage of eligible women and infants were already participating because of their higher priority (Oliveira et al., 2002).

Today, the WIC program serves almost half of all infants in the U.S. and about a quarter of the children ages 1-4 years (Hirschman, 2004). The question of how many eligible participants go unserved has been the subject of much debate. Historically, FNS has estimated the number of individuals eligible to participate in WIC in order to predict WIC caseloads. FNS's estimates have been questioned in recent years, however, because estimated coverage rates for some participant categories have exceeded 100 percent. Program advocates argued that FNS underestimates the number of eligible individuals, while others, including members of Congress, raised concerns that the program is serving ineligible individuals. In response to these concerns, FNS completed a number of studies to identify problems with the existing estimation methodology and potential solutions (see, for example, Gordon et al., 1999 and 1997). As a result of these efforts, a new methodology was introduced for estimating the number of WIC eligibles at the State level (Gordon et al., 1999).

Before revising the methodology used at the National level, FNS asked the Committee on National Statistics of the National Research Council to convene a panel of experts to study the existing methodology and make recommendations for improvement. The panel concluded that the existing methodology substantially underestimates the number of individuals eligible to participate in WIC (Ver Ploeg and Betson, 2003). The primary reason for the underestimation is that the methodology does not adequately measure monthly income and adjunctive eligibility. The panel proposed two alternative approaches to estimating WIC eligibility. At the time this report went to press, FNS was in the process of implementing the panel's recommendations.

Program Benefits

WIC was designed to counteract the negative effects of poverty on prenatal and pediatric health (Kresge, 2003). To achieve this goal, the program provides a combination of services, including supplemental foods, nutrition education, and referral to health and social services. Participants are generally certified to receive benefits for 6-month periods and must be recertified to continue receiving benefits. Exceptions to this rule include pregnant women (who are certified for the duration of the pregnancy and up to 6 weeks postpartum), infants (who are generally certified up to 1 year of age), and nonbreastfeeding postpartum women (whose eligibility expires at 6 months postpartum).

Supplemental Foods

The supplemental foods provided by WIC are good sources of many nutrients, including those potentially lacking in the diets of low-income pregnant women and children—protein, iron, calcium, and vitamins A and C. Foods available in WIC food packages include milk, eggs, cheese, dried beans and peas, peanut butter, full-strength (100 percent) fruit or vegetable juices high in vitamin C, and breakfast cereals high in iron and low in sugar. Food packages for infants are limited to iron-fortified infant formula, infant cereals, and, for infants 4 months and older, 100 percent fruit or vegetable juices high in vitamin C. Breastfeeding women whose infants do not receive WIC formula may also receive carrots and canned tuna.

Federal regulations specify minimum nutritional requirements for all WIC foods (USDA/FNS, 2003c). State WIC agencies are not required to authorize every available food that meets minimum nutritional requirements. States may limit authorization to specific brands and types of food based on cost, distribution within the State, participant acceptance, and/or administrative feasibility.

WIC food packages are meant to supplement participants' diets and are not expected to fully satisfy daily nutritional needs. The type and quantity of foods provided to individual participants vary by participant category. Federal regulations define maximum monthly allotments for different types of participants (USDA/FNS, 2003c). Maximum monthly allotments must be made available to participants if medically or nutritionally warranted. However, WIC staff may tailor the content of food packages (within maximum allotments) to meet individual needs and preferences.

Most WIC participants receive vouchers or checks to use in purchasing supplemental foods at authorized retail outlets. In a limited number of geographic areas, foods are delivered to participants' homes or participants pick up foods at warehouses. In recent years, several States have conducted pilot tests on the use of electronic benefits transfer (EBT) systems in disbursing WIC benefits. At least one State has implemented EBT statewide and another State is considering a statewide EBT system.

In mid-2003, FNS launched an initiative to revise existing food packages based on current nutrition recommendations, updated information about the dietary patterns and nutritional needs of low income women, infants and children, and new products in the marketplace (*Federal Register*, 2003). Following a period of public comment, USDA asked the IOM to convene a panel of independent experts to review available science and public comments and to develop recommendations for revising WIC food packages. A preliminary report was released in mid-2004 (IOM, 2004) and the final report is expected in 2005 (Okita, 2004).

Nutrition Education

Because the food package does not meet participants' total nutrient needs, nutrition education is seen as an essential part of the WIC Program. Nutrition education provides a mechanism for teaching WIC participants about recommended eating patterns and for encouraging them to adopt positive food-related attitudes and behaviors. Program regulations define two broad goals for WIC nutrition education:

- To stress the relationship between proper nutrition and good health, with special emphasis on the nutritional needs of the program's target populations.

- To assist individuals at nutritional risk in achieving a positive change in food habits, resulting in improved nutritional status and the prevention of nutrition-related problems (7 CFR, 246.11).

In practice, WIC nutrition education addresses many other topics, such as breastfeeding promotion; the need to avoid cigarettes, alcohol, illicit drugs, and over-the-counter medications during pregnancy; and the importance of childhood immunizations.

State WIC agencies are required to earmark at least one-sixth of annual administrative funds for nutrition education. Local WIC agencies are required to offer all adult participants and caretakers of infant and child participants at least two nutrition education contacts during each certification period. For participants with certifications that extend beyond 6 months, nutrition education must be offered quarterly.

State and local WIC agencies have broad autonomy to develop plans and procedures for providing nutrition education to WIC participants. Consequently, WIC nutrition education is quite diverse and may vary in both quantity and quality from one site to the next. A variety of methods may be used to provide nutrition education. For example, participants may be counseled one-on-one, may attend classes, or may view videos, filmstrips, or slide presentations on a range of nutrition- or health-related topics. Providers are encouraged to ensure that nutrition education messages take into account participants' educational levels, nutritional needs, household situations, and cultural preferences.

Although local WIC agencies are required to offer nutrition education, participants are free to decline these services without affecting receipt of other program benefits. To maximize participation, local agency staff tend to schedule nutrition education activities to coincide with issuance of WIC vouchers (Fox et al., 1998).

Referrals to Health and Social Services

Local WIC agencies are expected to serve as a link between participants and the health care system and to promote routine use of preventive health care services. Local WIC staff are also encouraged to provide referrals, as needed, to appropriate social services, such as the FSP, Medicaid, TANF, and other programs relevant to the participants' needs. The degree to which local WIC agencies actually facilitate linkages to health and social services varies depending on the adequacy of the health and social service infrastructure at the State and local levels and the extent to which participants are already linked into health and social service networks before coming to WIC (Fox et al., 1998).

Research Overview

The WIC program has been studied widely. Indeed, it is the most studied of the Federal FANPs with regard to impacts on nutrition- and health-related outcomes. The available body of research is impressive in size and, in many circles, is seen as solidly convincing that WIC has positive impacts, particularly on birth outcomes. The truth is, however, that much of the available research is clouded by the overarching problem of selection bias. In addition, the complexity of the health outcomes that have been studied has presented unique challenges to WIC researchers, further compromising their ability to obtain clear estimates of program impact.

Over the years, USDA has made a considerable investment in trying to elucidate the impact of WIC on participants' nutrition and health status. The first national evaluation of WIC was completed when the program was still very young (Edozien et al., 1979). The so-called Medical Evaluation of WIC included more than 50,000 WIC participants in 14 States and examined impacts on birth outcomes, child growth, anemia, and other measures of nutritional status. Study authors reported positive impacts, but the study has been widely criticized for, among other things, poor response rates on followup measures and dissimilarities between participant and nonparticipant groups. In addition, the study's dose-response design, which compared newly enrolling participants (nonparticipants) with participants who had been in the program for some time (participants), has come to be regarded as a poor design for studying birth outcomes.

In the early 1980s, USDA sponsored the National WIC Evaluation (NWE) (Rush et al.,1986) which consisted of four substudies, including an historical study of birth outcomes (Rush et al.,1988a); a longitudinal study of pregnant women (Rush et al.,1988d); a cross-sectional study of infants and children (Rush et al., 1988c); and a study of food expenditures (Rush et al.,1988b). Although the NWE is generally regarded as a carefully implemented study and remains the largest and most comprehensive study of WIC ever completed, it also had problems with noncomparability between participant and nonparticipant groups, as well as with crossovers between groups.

In the late 1980s, USDA undertook a feasibility assessment and design effort aimed at developing and fielding a study that would produce reliable estimates of the impact of WIC on infants and children. Outcomes to be examined included dietary intake, anemia, physical and cognitive growth, and use of health care services. Unfortunately, the so-called WIC Child Impact Study was canceled in 1992, at the request of Congress, before the full evaluation could be fielded. Results from a limited field test provide some information about potential impacts on young infants (6 months old) but fall far short of providing valid impact estimates (Burstein et al.,1991). In addition, the field test suffered from some of the same problems with noncomparability and crossovers that affected the NWE.

USDA's most recent efforts to assess WIC impacts have relied on secondary analyses of extant databases, most notably the 1988 National Maternal and Infant Health Survey (NMIHS) (Gordon and Nelson, 1995) and a specially created WIC-Medicaid database that included data on Medicaid expenditures, maternal WIC participation during pregnancy, and birth outcomes for live births in five States in 1987-88 (Devaney et al., 1990/91; Devaney, 1992; Devaney and Schirm, 1993). These secondary analyses have focused almost exclusively on impacts on birth outcomes, including savings in Medicaid costs.

In addition to USDA-sponsored research, many independent researchers have looked at WIC impacts using secondary analyses of existing databases, as well as primary data collected on State or local samples. The remainder of this chapter summarizes findings from all of this research.

The discussion is organized around WIC participant categories. Impacts of prenatal WIC participation are discussed first. The bulk of this research focuses on impacts on birth outcomes, with a much smaller body of work examining impacts on pregnant women themselves. Research that examined the impact of WIC participation on the initiation and/or duration of breastfeeding is also included in this section. The rationale is that the decision to initiate breastfeeding is generally made before an infant leaves the hospital, making the prenatal period a key point for intervention. Although decisions about breastfeeding duration are generally made during the postpartum period, for ease of discussion, all research related to breastfeeding outcomes are discussed in the same section.

The second section summarizes research that assessed impacts of WIC participation on infants and children. The third section describes studies that have assessed impacts on postpartum women (both nonbreastfeeding and breastfeeding). The final section summarizes findings from four studies that examined impacts on all types of WIC participants, without differentiating participant groups, or on household-level outcomes.

Selection Bias

Because use of randomized experiments is considered unethical by many policymakers and program administrators, only one study (Metcoff et al., 1985) used random assignment to study the impact of the WIC program (see chapter 2 for an explanation of the randomized experiment). Random assignment was feasible for these authors because, at the time the study was conducted, the demand for WIC participation at the study site exceeded the available funding. All other studies of WIC impacts have used quasi-experimental designs.

Selection bias, as discussed in chapter 2, is driven by the fact that women who participate in WIC or who enroll their infants or children in WIC may differ in unmeasured ways from women who are eligible but do not participate. These differences may influence the outcomes being studied. This influence could run in either direction, resulting in overestimation or underestimation of the true effect of the program. For example, women who participate in WIC or enroll their children in WIC may be more health-conscious and motivated than women who do not participate, or may be more knowledgeable about and connected to the health care system. These women and their offspring might have better outcomes than nonparticipants, even in the absence of the program. In this case, estimates of WIC impacts would be overstated. On the other hand, because the WIC program specifically targets individuals who are at nutritional risk, WIC participants may be more likely, *a priori*, to have poor outcomes than otherwise comparable individuals who do not enroll in the program. In this case, estimates of WIC impacts would be understated.

The problem of selection bias was largely ignored in the earliest WIC research. The first study to attempt to control from selection bias was the WIC-Medicaid Study, which estimated the impact of prenatal WIC participation on a number of birth outcomes (Devaney et al. (1990/91)). Study authors estimated a number of different selection-bias-adjustment models but ultimately rejected all of them because they produced implausible findings and were extremely sensitive to minor changes in specification and to estimation procedures. Devaney and Schirm (1993) reported comparable experiences in a subsequent analysis of the same dataset. Researchers attributed the problems encountered in attempting to control for selection bias to the limited number of variables available in the administrative (Medicaid and WIC) and birth certificate data included in the WIC-Medicaid database.

Gordon and Nelson (1995) used the NMIHS, a nationally representative dataset that includes information on the characteristics of women who gave birth in 1988 and their offspring, to study the effects of WIC. With access to a much richer data set, Gordon and Nelson were able to control for many more covariates in their basic model, including income and use of cigarettes, alcohol, marijuana, and cocaine. They also had more options for variables (instruments) to include in selection-bias-adjustment models. They estimated several models of the effect of WIC on birthweight, using various combinations of the following variables: per capita State-level WIC food expenditures (a proxy for the availability of WIC services); an indicator of whether the family had income from wages (as an indicator of the level of contact with public assistance agencies); and an indicator of WIC participation during previous pregnancies.

Ultimately, Gordon and Nelson deemed their efforts to control for selection bias to be unsuccessful. After several different estimation procedures and model specifications yielded implausible and highly unstable results, they concluded the following:

> It is possible that the selection-bias-correction models of the effects of WIC on birth outcomes produce unstable and implausible results because the factors affecting WIC participation and birthweight are very nearly identical, since WIC targets low-income women at risk for poor pregnancy outcomes. In this case, modeling the participation decision is not likely to be a useful approach to controlling for selection bias.

Brien and Swann (1999) analyzed data from the NMIHS with the explicit goal of developing strategies to deal with selection bias. To minimize potential bias, they restricted their sample to non-Hispanic Blacks and non-Hispanic Whites and carried out separate analyses for each group. The authors used a two-stage estimation procedure, similar to the basic approach used by Gordon and Nelson (1995). To model the participation decision, the authors used a variety of State-level characteristics that served as proxies for the availability and “generosity” of WIC and other welfare programs. These characteristics included relative ease of the State’s WIC income certification policies, presence of brand-name purchase restrictions for WIC foods, presence of adjunctive income eligibility for AFDC participants, value of the first trimester hemoglobin level used to define nutritional risk, number of WIC clinics per 1,000 low-income persons, number of WIC clinics per 100 square miles, AFDC guarantee for a family of four, and average Medicaid expenditure for a family of four.

Like previous researchers, Brien and Swan estimated several models with different combinations of instruments. They, too, found that results were very sensitive to model specification. In some cases, results showed a negative association between WIC participation and birth outcomes, although the differences were not statistically significant. Moreover, the sensitivity to model specification varied substantially by race, suggesting that the instruments used did a better job of predicting WIC participation among Blacks than among Whites.

Brien and Swan also estimated a fixed-effects model, using a sample of women who had at least one birth before the 1988 NMIHS birth. This approach assumes that critical unmeasured differences between WIC participants and non-WIC participants are mother-specific and do not vary over time. The analysis examined whether differences in WIC participation status for the two births affected outcomes.[53] The fixed-effects model yielded results that were generally smaller in magnitude and stronger in statistical significance than the two-stage model. For Whites, findings for the incidence of low birthweight were completely different (a negative but statistically insignificant effect) than findings for the two-stage model.

As the preceding discussion illustrates, the problem of selection bias has proven especially thorny in research on birth outcomes. Some researchers who have investigated WIC impacts on other outcomes or for other WIC participant groups have reported success in controlling for selection bias. This has not been a universal experience, however, and many of these researchers have also struggled with limited candidates for identifying variables and with models that produce inconsistent, implausible, or unstable results.

Impacts of WIC Prenatal Participation

The prenatal component of the WIC program is, by far, the most studied part of the program. The vast majority of the research in this area focuses on impacts on birth outcomes. Substantially less research has been done on the impact of prenatal WIC participation on the initiation of breastfeeding. Even less research has examined the impact of prenatal WIC participation on the women themselves (for example, on women's dietary intake and/or nutritional status). A small number of studies have examined the relationship between prenatal WIC participation and child development outcomes. Because several of these studies look at WIC participation during infancy or childhood, in addition to prenatally, these studies are discussed later in this chapter-in the section that deals with impacts on infants and children.

[53]The assumption that key unmeasured maternal characteristics do not vary over time is a generous one. There is no guarantee that maternal effects on a pregnancy—for example, the mother's general health, use of cigarettes and alcohol, weight gain, and diet—are constant over time, and the NMIHS had relatively limited information on characteristics associated with earlier pregnancies. Moreover, there is no guarantee that women who had two births are representative of all prenatal WIC participants (Besharov and Germanis, 2001).

Birth Outcomes: Research Overview

The literature search identified 38 studies that examined impacts of prenatal WIC participation on a variety of birth outcomes.[54] The outcomes most frequently studied were mean birthweight and likelihood of low birthweight (defined as an infant weighing less than 2,500 gm, or 5.5 pounds (lb)). Other birth outcomes included mean gestational age (length of gestation at time of delivery), and likelihood of very low birthweight (less than 1,500 gm, or 3.3 lb), premature birth (generally defined as birth before 37 weeks gestation), intrauterine growth retardation (IUGR) or being small-for-gestational age, neonatal mortality, and infant mortality. Several studies also examined the impact of prenatal WIC participation on Medicaid costs associated with delivery and newborn care (up to 30-60 days after birth).

Selected characteristics of these studies are summarized in table 17. The 38 identified studies can be divided into four groups based on scope/generalizability and general methodology. The two national USDA-sponsored WIC evaluations, although substantially different in design, make up Group I. The strongest of the two, the NWE, includes two different components—the Longitudinal Study of Pregnant Women (Rush et al., 1988d) and the Historical Study of Pregnancy Outcomes (Rush et al., 1988a). Although the NWE is the most recent national evaluation of the WIC program, it is based on data collected in 1982 and 1983 (Rush et al. 1988d) and historical data from the mid-70s through 1980 (Rush 1988a), and is therefore quite dated.

Group II includes nine studies that used national survey data, almost always the NMIHS, to examine WIC impacts on birth outcomes. Although some of the research that used the NMIHS was completed recently, all of it is based on births in 1988. One study in this group (Kowaleski-Jones and Duncan, 2002) used data from the 1990-96 waves of the National Longitudinal Survey of Youth (NLSY).

Group III, the largest group, includes 15 studies that linked State-level files of WIC participant information with other State-level files, generally vital statistics

[54]Several very early unpublished papers and reports included in a review prepared by Rush and colleagues (1986) for the NWE are not included because they could not be located. Given the age of the data and the descriptions included in Rush's summary, it is doubtful that these documents would add anything to the present discussion. Most, if not all, of these studies appear to have centered on cross-tabulations that were subjected to few, if any, statistical controls.

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Group I: National evaluations						
Rush et al. (1988a) (NWE)	Birthweight, gestational age, likelihood of low birthweight, very low birthweight, and premature birth, and neonatal and infant mortality rates	Vital statistics records for 1,392 counties in 19 States and DC (1972-80)	N/A (Aggregate data analysis)	Trends analysis relating WIC program penetration over time to birth outcomes	WIC penetration index	Multivariate regression
Rush et al. (1988d) (NWE)	Birthweight, gestational age, likelihood of premature birth, and fetal mortality rate	Record abstractions in 174 WIC sites and 55 prenatal clinics(1983-84)	Nationally representative sample of pregnant WIC participants and income-eligible nonparticipants receiving prenatal care in surrounding public health clinics or hospitals (n=3,935)[3]	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Edozien et al. (1979)	Birthweight, gestational age	Primary data collection in 19 WIC sites in 14 States. Data were collected at time of WIC enrollment, approximately every 3 months until delivery, and once after delivery (1973-76)	Postpartum WIC participants who participated prenatally (n~1,000)	Participants, before vs. after, separate groups	Newly enrolling participants vs. participants with varying lengths of participation	Multivariate regression
Group II: Secondary analysis of national surveys						
Finch (2003)	Likelihood of low birthweight	1988 NMIHS	WIC and non-WIC women who were White, Black, or Hispanic with live singleton births that were at least 22 weeks gestation (n=12,814)	Participant vs. nonparticipant	Participation dummy with short- (<6 months) and long-term (6+ months) WIC participation	Multivariate regression

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Kowaleski-Jones and Duncan (2002)	Birthweight	1990-96 NLSY	(1) NLSY children born between 1990 and 1996 (n=1,984) (2) NLSY children born between 1990 and 1996, with at least 1 other sibling born during the same period (n=453 sibling pairs)	Participant vs. nonparticipant	Participation dummy	(1) Multivariate regression (2) Fixed-effects model
Hogan and Park (2000)	Likelihood of low birthweight and very low birthweight	1988 NMIHS	WIC and non-WIC women (n=8,145)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Brien and Swann (1999)	Birthweight, likelihood of low birthweight and premature birth, and neonatal and infant mortality rates	1988 NMIHS	(1) WIC and income-eligible non-Hispanic women who were at nutritional risk (n=7,778) (2) WIC and income-eligible non-Hispanic women with at least 1 live birth prior to 1988 (n=6,254 pairs of births)	Participant vs. nonparticipant	(1) Participation dummies: 1 for ever participated and 1 for participated during first trimester (2) Participation status for each pregnancy	(1) Multivariate regression, including attempt to control for simultaneity and several selection-bias-adjustment models (2) Fixed-effects model; separate models estimated for Blacks and Whites
Moss and Carver (1998)	Neonatal mortality rate	1988 NMIHS	WIC and income-eligible non-Hispanic women (n=7,796)	Participant vs. nonparticipant	Participation dummy with and without Medicaid	Logit analysis
Frisbie et al. (1997)	Likelihood of intrauterine growth retardation, premature birth, and heavy preemie[4]	1988 NMIHS	WIC and non-WIC women (n=8,424)	Participant vs. nonparticipants	Participation dummy	Multivariate regression analysis to identify determinants of birth outcomes

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Covington (1995)	Likelihood of low birthweight and very low birthweight	1988 NMIHS	WIC and non-WIC African American women who received some prenatal care (n=3,905)	Participant vs. nonparticipant	Participation dummy	Multivariate regression. Separate models for LBW vs. normal weight and VLBW vs. normal weight for each of 4 subgroups based on combinations of income and receipt of Medicaid and/or AFDC
Gordon and Nelson (1995)	Birthweight, gestational age, likelihood of low birthweight, very low birthweight, and premature birth, and neonatal and infant mortality rates	1988 NMIHS	WIC and income-eligible women (n=6,170)	Participant vs. nonparticipant	Participation dummy	Multivariate regression and logit analysis. Birthweight analysis included separate models for Blacks and Whites, as well as several alternative models to control for simultaneity.[5, 6] Attempted, but rejected, selection-bias adjustment.
Joyce et al. (1988)	Neonatal mortality rate	1977 Census data for large counties in the U.S.	Data for 677 counties with 50,000+ residents for White analysis and 357 counties with 5,000+ Blacks for Black analysis	Cost-effectiveness study using aggregate data	State-specific number of pregnant women enrolled in WIC per 1,000 State-specific eligible women	Multivariate regression, including selection-bias adjustment. Separate models for Blacks and Whites.
Group III: State-level studies using WIC participation files matched with Medicaid and/or birth record files						
Roth et al. (2004)	Likelihood of low birthweight, very low birthweight, neonatal mortality, postneonatal mortality, infant mortality[7]	Linked WIC, Medicaid, and vital statistics records for births in Florida between January 1996 and the end of December 2000	WIC and non-WIC Medicaid recipients who did not participate in high-risk obstetrical program (n=295,599)	Participant vs. nonparticipant	Participation dummy	Multivariate regression

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Gregory and deJesus (2003)	Likelihood of low birthweight, very low birthweight, neonatal mortality, and infant mortality, length of infants' hospital stay, Medicaid costs	Linked WIC, Medicaid, birth and death record, and hospital discharge files for births in New Jersey between May 1992 and December 1993	WIC and non-WIC Medicaid recipients with live singleton births (n=19,614)	Participant vs. nonparticipant	Participation dummy	Multivariate regression. Separate models for Blacks and non-Blacks
Buescher and Horton (2000)	Birthweight, likelihood of low birthweight and very low birthweight, Medicaid costs	Linked WIC, Medicaid, and birth record files for 1997 births in North Carolina	WIC and non-WIC Medicaid recipients who were enrolled in prenatal care and had live singleton births (n=42,965)	Participant vs. nonparticipant	Participation dummy	Multivariate regression, including several alternative models to control for simultaneity[8]
Ahluwalia et al. (1998)	Likelihood of low birthweight	Linked WIC and birth record files for 1992 births in Michigan	WIC and non-WIC women with full-term births (n=53,782)	Participant vs. nonparticipant	Dose response: Length of prenatal WIC "exposure"[9]	Multivariate regression
Buescher et al. (1993)	Likelihood of low birthweight and very low birthweight, Medicaid costs	Linked WIC, Medicaid, and birth record files for 1988 births in North Carolina	WIC and non-WIC Medicaid recipients who were enrolled in prenatal care (n=21,900)	Participant vs. nonparticipant	Participation dummy and dose-response: Percentage of gestation on WIC	Multivariate regression, including attempt to control for simultaneity[10]
Devaney and Schirm (1993)	Likelihood of neonatal and infant mortality	FNS WIC/Medicaid (1987-88)	WIC and non-WIC Medicaid recipients (n=111,958)	Participant vs. nonparticipant	Participation dummy: Enrolled by 30 weeks gestation	Probit analysis
Devaney (1992)	Likelihood of very low birthweight	FNS WIC/Medicaid (1987-88)	WIC and non-WIC Medicaid recipients (n=111,958)	Participant vs. nonparticipant	Participation dummy	Probit analysis, including attempts to control for simultaneity[11]
Devaney et al. (1990/91)	Birthweight, gestational age, likelihood of premature birth, and Medicaid costs	FNS WIC/Medicaid (1987-88)	WIC and non-WIC Medicaid recipients (n=111,958)	Participant vs. nonparticipant	Participation dummy	Multivariate regression and probit analysis, including attempt to control for simultaneity.[12] Attempted but rejected selection-bias adjustment.

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
New York State (1990)	Birthweight, gestational age, likelihood of low birthweight, very low birthweight, and premature birth, and Medicaid costs	Linked WIC, birth record, and hospital discharge files for births in New York State in the last 6 months of 1988	Singleton births to WIC and non-WIC women (n=132,994)	Participant vs. nonparticipant within 3 groups defined on the basis of insurance coverage (Medicaid, private, none)	Participation dummy	Multivariate regression
Simpson (1988)	Likelihood of low birthweight	Aggregate county-level data for North Carolina, including vital statistics, demographic and service infrastructure characteristics, and program penetration and expenditures (1980-85)	Data for 75 (of 100) counties, all of which provided WIC and other prenatal care services for all county residents (rather than sharing responsibility with another county)	Trends analysis relating WIC penetration over time to birth outcomes	Program “intensity” variable based on county-level WIC expenditures	Multivariate regression
Stockbauer (1987)	Birthweight, gestational age, likelihood of low birthweight, very low birthweight, premature birth, small-for-gestational-age, and neonatal mortality	Linked WIC, birth and death record files for 1982 births in Missouri	Matched WIC and non-WIC women with singleton births (n=9,411 pairs)[13]	Participant vs. matched control	Participation dummy and dose response: Dollar value of redeemed vouchers	Analysis of covariance
Schramm (1986)	Birthweight, likelihood of low birthweight, neonatal mortality rate, and Medicaid costs	Linked WIC, Medicaid, birth record, hospital care, and death record files for 1982 births in Missouri	WIC and non-WIC Medicaid recipients (n=8,546)	Participant vs. nonparticipant	Participation dummy and dose response: WIC food costs adjusted for length of pregnancy	Multivariate regression

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Stockbauer (1986)	Birthweight, gestational age, likelihood of low birthweight, and neonatal mortality rate	Linked WIC, birth, and death record files for 1980 births in Missouri	WIC and non-WIC Missouri residents with singleton births (n=6,732 WIC; sample for non-WIC not reported)	Participants vs. 3 different nonparticipant groups: (1) all non-WIC births; (2) random sample of non-WIC births; (3) matched group of non-WIC births[14]	Participation dummy and dose-response: Duration of participation and dollar value of redeemed WIC coupons	Analysis of covariance. Separate analyses for White, non-White, and total group.
Schramm (1985)	Birthweight, likelihood of low birthweight, Medicaid costs	Linked WIC, Medicaid, birth, and hospital care records for 1980 births in Missouri	WIC and non-WIC Medicaid recipients (n=7,628)	Participant vs. nonparticipant	Participation dummy and dose response: WIC food costs adjusted for length of pregnancy	Analysis of covariance
Kotelchuck, et al. (1984)	Birthweight, gestational age, likelihood of low birthweight, premature birth, small-for-gestational-age birth, and neonatal mortality rate	Linked WIC, birth, and death records for 1978 births in Massachusetts	Matched WIC and non-WIC women with singleton births (n=4,126 pairs)[15]	Participant vs. matched control	Participation dummy and dose response: Months on WIC and percent of pregnancy on WIC	Bivariate comparisons
Group IV: Other State and local studies						
Reichman and Teitler (2003)	Birthweight, likelihood of low birthweight	Standardized data collected for women enrolled in New Jersey's HealthStart program for pregnant Medicaid recipients between 1988 and 1996	All WIC and non-WIC HealthStart participants who had a live singleton birth (n=90,117)	Participant vs. nonparticipant	Participation dummy	Multivariate regression, including attempt to control for simultaneity[16]
Brown et al. (1996)	Birthweight, likelihood of low birthweight, and infant mortality rate	Medical records, birth, and death certificates for births in 1 Indiana hospital between January 1988 and June 1989	Non-Hispanic women who delivered at the area's primary hospital for the "underserved" (n=4,707)	Participant vs. nonparticipant	Participation dummy	Multivariate regression

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Mays-Scott (1991)	Birthweight	WIC records in 1 county health department in Texas (1987-89)	Prenatal WIC participants who were ≤17 years and had at least 1 previous pregnancy (n=217)	Participants, before vs. after	Dose response: Number of months enrolled, nutrition education contacts, and voucher pickups	Analysis of variance
Collins et al. (1985)	Birthweight	Primary data collection in public health department clinics in 6 Alabama counties (1980-81)	WIC and non-WIC pregnant women (n=519)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Metcoff et al. (1985)	Birthweight	Primary data collection at a prenatal clinic in 1 hospital in Oklahoma (1983-84)	Income-eligible pregnant women selected at mid-pregnancy based on predicted birthweight; roughly equivalent numbers were predicted to have average-size babies vs. small or large babies (n=410)	Randomized experiment	Participation dummy	Multivariate regression
Heimendinger et al. (1984)	Birthweight	WIC and medical records in 3 WIC clinics and 4 non-WIC clinics in the same Boston neighborhoods (1979-81)	WIC and Medicaid-eligible infants and toddlers up to 20 months of age with at least 2 height and weight measurements[17] (n=1,907)	Participant vs. nonparticipant	Participation dummy based on mother's participation in WIC during pregnancy	Multivariate regression

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Kennedy and Kotelchuck (1984)	Birthweight, gestational age, likelihood of low birthweight and small-for-gestational-age birth, and fetal death rate	WIC and medical records in WIC sites and non-WIC health facilities in 4 geographic areas of Massachusetts (1973-78) (Reanalysis of data from Kennedy et al., 1982)	Matched WIC and non-WIC pairs of pregnant women (n=418 pairs)[18, 19]	Participant vs. matched control	Participation dummy and dose response: Number of months vouchers received	Bivariate comparisons
Bailey et al. (1983)	Birthweight	Primary data collection at 1 WIC site and 1 non-WIC site in Florida (Dates not reported)	WIC and income-eligible nonparticipants who were 30 weeks pregnant at time of recruitment and receiving identical prenatal care (n=101)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Paige (1983)	Medicaid costs, health care utilization	Medicaid records in 4 counties in Maryland, 2 in which WIC was available and 2 in which WIC was not available (1979-80)	WIC and income-eligible non-WIC women who were on Medicaid for at least 16 weeks during pregnancy (n=114)	Participant vs. nonparticipant	N/A	Comparisons of means and proportions (no statistical tests reported)
Kennedy, et al. (1982)	Birthweight, likelihood of low birthweight	WIC and medical records in WIC sites and non-WIC health facilities in 4 geographic areas of Massachusetts (1973-78)	WIC and WIC-eligible women (n=1,297)[18]	Participant vs. nonparticipant	Participation dummy and dose response: Number of vouchers received, months on WIC	Multivariate regression

See notes at end of table.

Continued—

Table 17—Studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Silverman (1982)	Birthweight, likelihood of low birthweight	Medical records for random sample of women enrolled in Maternity and Infant Care Project (MIC) in Allegheny County, PA, before (1971-74) and after (1974-77) initiation of WIC	WIC and income-eligible nonparticipants (n=2,514)	Participants, before vs. after, separate groups	Participation dummy	Multivariate regression

Notes: N/A = Not applicable.

[1]Data sources:

FNS WIC/Medicaid = FNS' WIC/Medicaid database.

NLSY = National Longitudinal Survey of Youth.

NMIHS = National Maternal and Infant Health Survey.

[2]Unless the description of the study sample indicates that a comparison group was limited to nonparticipants who were income-eligible for WIC or known to be Medicaid participants, all income levels were included in the comparison group. Income was generally controlled for in the analysis if the information was available.

[3]Maximum analysis sample; sample varies by outcome. Birth outcome data were available for only about 75 percent of women in the study.

[4]Intrauterine growth retardation defined as fetal growth ratio of less than 85 percent (observed birthweight at gestational age by mean for gestational age of sex-specific fetal growth distribution). Heavy preemie defined as birthweight of 2,500 gm or more and gestation of less than 37 weeks. (Authors report that mortality rate for heavy preemies may be twice that of normal birthweight infants).

[5]Used three alternative definitions of WIC participation to control for simultaneity in analyses of impacts on birthweight and gestational age: (1) during first 8 months; (2) during first 7 months; (3) during first 6 months. Also estimated model for birthweight that controlled for gestational age.

[6]For all outcomes, estimated basic model as well as separate models for four different cohorts defined by length of gestation thresholds: 28 weeks, 32 weeks, 36 weeks, and 40 weeks.

[7]Authors also examined impacts on birth defects, C-section, and complications during pregnancy and delivery. No significant differences were noted for birth defects or complications during pregnancy and delivery. The rate of C-section was significantly greater for WIC participants.

[8]Alternative models included (1) women who enrolled in WIC after 33 weeks gestation included in the nonparticipant group, (2) three separate cohorts, based on gestational age (29, 33, and 37 weeks), and (3) gestational age as a control variable.

[9]Exposure for women who did participate in WIC was considered high = enrolled before 12 weeks gestation, medium = enrolled at 12-20 weeks gestation, and low = enrolled at 21-37 weeks gestation.

[10]In addition to basic model, estimated alternative model that included women who enrolled in WIC at 36 weeks gestation or later in the nonparticipant group.

[11]Alternative models defined WIC participants as those who enrolled in WIC (1) before 32 weeks gestation and (2) by 30 weeks gestation.

[12]Estimated two alternative models: (1) basic model with addition of control for first-trimester WIC participation and gestational age, (2) basic model with WIC participants who enrolled after 36 weeks considered nonparticipants.

[13]Pairs matched on age, race, education, gravidity, number of births this pregnancy, and marital status.

[14]Pairs matched on age, race, education, number births this pregnancy, smoking during pregnancy, and pre-pregnancy weight.

[15]Pairs matched within catchment area on age, race, parity, education, and marital status.

[16]Included separate model to control for gestational-age bias, but sample was restricted based on initiation of prenatal care (1st or 2nd trimester) rather than timing of WIC enrollment.

[17]The main focus of study was impact of WIC on children's growth; however, the authors compared birthweights of subjects whose mothers were and were not in WIC.

[18]WIC-eligible women included in the nonparticipant group were wait-listed for WIC during their pregnancy, enrolled in WIC postpartum, or women who received prenatal care at non-WIC health care facilities in same neighborhood but never enrolled in WIC.

[19]Approximately 80 percent of women were matched on race, age, parity, marital status, and income. The remainder were matched on four of the five variables.

files and Medicaid files, to study birth outcomes among WIC participants and nonparticipants. With three exceptions (Devaney et al., 1990/91; Devaney, 1992; Devaney and Schirm, 1993), all of the studies in Group III are based on data from one State. The three excepted studies used the FNS WIC-Medicaid database. This database was assembled by FNS to address a congressional mandate to determine “savings in Medicaid costs for newborns and their mothers during the first 60 days after birth from participating in the WIC program during pregnancy” (Devaney et al., 1990). A secondary objective for the database was to examine effects of participation on birthweight and gestational age. The FNS WIC-Medicaid database includes WIC participation, birth certificate, and Medicaid claims data for five States (Florida, Minnesota, North Carolina, South Carolina, and Texas). For the first four States, the database includes data for all births in 1987. For Texas, the database includes data for all births during the first 6 months of 1988.

Most of the research in Group III is based on data collected in the 1980s. However, four studies are based on more recent data. Roth et al. (2004) analyzed data for Medicaid births in Florida from 1996-2000;[55] Gregory and deJesus (2003) analyzed births in New Jersey for an 18-month period in 1992-93; Buescher and Horton (2000) used 1997 data from North Carolina (this study is an update of a previous study conducted in 1988 (Buescher et al., 1993)); and Ahluwalia et al. (1998) used data for 1992 births in Michigan.

Finally, Group IV includes 11 State or local studies that examined WIC impacts among pregnant women receiving care in particular programs, hospitals, or clinics. All but one of these studies (Reichman and Teitler, 2003) used data that were collected in the 1970s and 1980s. Reichman and Teitler used data that were collected between 1988 and 1996.

Methodological Considerations

Before reviewing findings from the studies presented in table 17, it is important to understand three methodological considerations that, in addition to selection bias, affect interpretation of research on birth outcomes: simultaneity of WIC participation and gestational age, influence of the comparison group used, and use and adequacy of prenatal care.

Simultaneity of WIC Participation and Gestational Age. Women who deliver early have less chance of enrolling in WIC. Women who go to term have a greater chance of enrolling. Consequently, both the decision to participate in WIC and the length of WIC participation are inexorably linked with gestational age, an important predictor of most birth outcomes. This simultaneity means that assessments of WIC impact that rely solely on a binary indicator of participation are likely to *overstate* the impact of the program. Moreover, because the duration of WIC participation is also simultaneous with gestational age, a traditional dose-response approach—estimating WIC impacts based on number of months of WIC participation—although employed in several studies summarized in table 17, is not a satisfactory solution to the problem.

Gordon and Nelson (1995) studied several approaches to addressing the relationship between the timing of WIC enrollment and gestational age (pregnancy duration). These included omitting very late enrollees (enrolled after the eighth month) from the WIC group, including gestational age as an independent variable in the regression, and defining several cohorts of WIC participants based on gestational age (pregnancy duration) at the time of WIC enrollment. All of these approaches decreased estimated impacts to varying degrees. Gordon and Nelson ultimately concluded that each of the approaches to controlling for simultaneity systematically underestimated the impact of WIC because they effectively eliminated any effect WIC might have on extending gestation. The authors suggested that results from analyses using a binary indicator of WIC participation (participant vs. nonparticipant) and those comparing various cohorts of WIC participants (in an effort to control for simultaneity) probably bound the magnitude of the true effect.

Influence of the Comparison Group Used. Research has consistently shown that specific types of women are more likely than other women to participate in WIC. Characteristics associated with increased likelihood of WIC participation include younger age, lower income, lower educational levels, being unmarried, and being African American. Several early studies of the impact of WIC on birth outcomes attempted to control for these differences by creating matched pairs of participants and nonparticipants (Kennedy and Kotelchuck, 1984; Kotelchuck et al., 1984;

[55]This study was first presented in 2000, with a subset of the data (Roth et al., 2000). Just before this report went to press, the author provided an update that includes data for the full 5-year period (1996-2000) (Roth et al., 2004). A manuscript is currently in preparation.

Stockbauer, 1987). Matching was limited, however, to variables that were available on birth certificates, most often maternal age, race, parity, education, and marital status. Researchers were unable to control for other important variables, particularly income and key characteristics related to pregnancy, and generally did not do so in their analyses (for example, analyses for Kennedy and Kotelchuck (1984) and Kotelchuck (1984) were limited to chi-squares and t-tests). Thus, the comparability of treatment and comparison groups in these studies is still open to question, despite the fact that the groups were "matched."

In interpreting findings from these studies, it is important to realize that, to the extent that comparison-group women were higher income or less at-risk than WIC women, the true impact of the WIC program (at the time these studies were conducted) may have been *underestimated*.

In 1985, Schramm studied the impact of WIC on Medicaid costs for newborns in Missouri. By limiting the analysis to Medicaid recipients, all of whom were income-eligible for WIC, Schramm created a ready-made comparison group and minimized (but did not eliminate) the potential influence of noncomparable incomes. The approach used by Schramm has been adapted and used by many other researchers, most notably in the USDA-sponsored WIC-Medicaid studies (Devaney et al., 1990/91; Devaney, 1992; Devaney and Schirm, 1993) (see Group III in table 17). In interpreting results of these Medicaid-based studies, it is important to recognize that they are limited to the lowest income WIC participants. At the time these studies were conducted, WIC eligibility was defined as 185 percent of poverty, while Medicaid eligibility was generally set at 130 percent of poverty or lower. Because lower income women are at higher risk of poor birth outcomes, these studies probably *overstated* the impact of WIC.

Use and Adequacy of Prenatal Care. Receiving adequate prenatal care is expected to independently affect most birth outcomes. Consequently, most recent research has controlled for the adequacy of prenatal care in order to estimate the independent effect of WIC—that is, the impact of WIC *over and above* the impact of receiving adequate prenatal care. However, because encouraging prenatal care and potentially providing a link to such care is a major focus of the WIC program, including adequacy of prenatal care as a covariate effectively *understates* the full impact of the WIC program. Moreover, Currie (1995) argues that including prenatal care in multivariate models may be inappropriate because prenatal care and WIC participation may be simultaneously chosen.[56]

Birth Outcomes: Research Results

Table 18 summarizes findings from the available research by outcome. Results for each study are reported using the primary author's name. In the interest of providing a comprehensive picture of the body of research, both significant and nonsignificant results are reported in table 18 and in all other "findings" tables included in this report. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned, however, to avoid the practice of "vote counting" or adding up all the studies with particular results. Because of differences in research design and other considerations, as discussed in the text, findings from some studies merit more consideration than others.

For the first two outcomes in the table (mean birthweight and mean gestational age), a *higher* value is associated with a positive WIC impact. For the remaining outcomes (for example, the likelihood of low birthweight), a *lower* value is associated with a positive WIC impact. The column headings in table 18 vary accordingly, so that significant *positive* WIC effects are always shown in the far left column of the table.

As noted in the preceding discussion, all of the available studies have limitations that require that their findings be caveated. Thus, no single study provides a definitive answer on WIC's effectiveness, but the body of research provides suggestive evidence. As table 18 illustrates clearly, the majority of studies reported positive differences that favor WIC participants. In most cases, differences were statistically significant.

In 1992, the General Accounting Office (GAO) completed a meta analysis of existing WIC studies that yielded estimates of cost savings attributable to WIC (GAO, 1992). The meta analysis included 17 studies of WIC impacts on rates of low birthweight that were

[56]Some early studies included prenatal care use and/or adequacy as separate outcome measures. While most of these studies found positive associations between WIC participation and measures of prenatal care, these estimates have largely been discounted because cross-sectional studies can not disentangle the direction of the effect. Higher rates and quality of prenatal care among WIC participants may result from either WIC referring women to prenatal care or prenatal care providers referring enrolled women to WIC. Because of this limitation and the fact that prenatal care is now almost universally used as a covariate rather than an outcome, results of analyses that looked at the impacts of WIC on prenatal care are not included in this summary.

Table 18—Findings from studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs

	Significant impact	No significant impact		Significant impact
Outcome	Participants higher	Participants higher/same	Participants lower	Participants lower
Mean birthweight	Reichman (2003) [1 State] Kowaleski-Jones (2002) [national] Buescher (2000) [1 State] Brien (1999) [national] {Blacks} Gordon (1995) [national][1] Mays-Scott (1991) [1 site] Devaney (1990/91) [5 States][2] New York State (1990) [1 State] Rush (1988a) [national] Stockbauer (1987) [1 State] {Blacks} Schramm (1986) [1 State] Stockbauer (1986) [1 State] {non-White}[3] Metcoff (1985) [1 site] {smokers} Heimendinger (1984) [3 neighborhoods] Kennedy (1984) [4 areas in 1 State][4] Bailey (1983) [2 sites] {smokers} Kennedy (1982) [4 areas in 1 State] Edozien (1979) [national] {3+ months on WIC}	Brown (1996) [1 site] Stockbauer (1986) [1 State] {White}[5] Collins (1985) [6 counties] Metcoff (1985) [1 site] {nonsmokers} Schramm (1985) [1 State] Kotelchuck (1984) [1 State][6] Bailey (1983) [2 sites] {nonsmokers} Silverman (1982) [1 county]	Brien (1999) [national] {Whites} Rush (1988d) [national] Stockbauer (1987) [1 State] {White} Edozien (1979) [national] {< 3 months on WIC}	
Mean gestational age	Gordon (1995) [national] Devaney (1990/01) [5 States] New York State (1990) [1 State] Rush (1988a) [national] Stockbauer (1987) [1 State] Stockbauer (1986) [1 State][7] Kennedy (1984) [4 areas in 1 State] Kotelchuck (1984) [1 State] Edozien (1979) [national]	Brien (1999) [national] {Blacks}[8]	Brien (1999) [national] {Whites} Rush (1988d) [national]	

See notes at end of table.

Continued—

Table 18—Findings from studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants lower	Participants lower/same	Participants higher	Participants higher
Likelihood of low birthweight (<2,500 gm)	Roth (2004) [1 State][9] Finch (2003) [national] Gregory (2003) [1 State] {Blacks} Reichman (2003) [1 State] Buescher (2000) [1 State][10,11] Ahluwalia (1998) [1 State] Covington (1995) [national] {except subgroup noted}[12] Gordon (1995) [national] Buescher (1993) [1 State] Devaney (1990/91) [5 States] New York State (1990) [1 State] Stockbauer (1987) [1 State] {Blacks} Schramm (1986) [1 State] Schramm (1985) [1 State] Stockbauer (1986) [1 State] {non-White}[13] Kotelchuck (1984) [1 State][4] Kennedy (1982) [4 areas in 1 State]	Gregory (2003) [1 State] {non-Blacks} Brien (1999) [national] {Blacks}[8] Brown (1996) [1 site] Rush (1988a) [national] Simpson (1988) [1 State] Stockbauer (1987) [1 State] {White} Stockbauer (1986) [1 State] {White} Bailey (1983) [2 sites] Kennedy (1984) [4 areas in 1 State] Silverman (1982) [1 county]	Hogan and Park (2000) [national] Brien (1999) {national] {Whites} Rush (1988d) [national]	Covington (1995) [national] {annual income > 12,000 and no public aid}
Likelihood of very low birthweight (<1,500 gm)	Roth (2004) [1 State][9] Gregory (2003) [1 State] {Blacks} Buescher (2001) [1 State][10,11] Hogan and Park (2000) [national] Covington (1995) [national] Gordon (1995) [national][14] Buescher (1993) [1 State] Devaney (1992) [4 States] New York State (1990) [1 State] Stockbauer (1987) [1 State] {Blacks}	Gregory (2003) [1 State] {non-Blacks} Devaney (1992) [1 State][15] Stockbauer (1987) [1 State] {Whites} Rush (1988d) [national]	Rush (1988a) [national]	

See notes at end of table.

Continued—

Table 18—Findings from studies that examined the impact of prenatal WIC participation on birth outcomes, including associated health care costs—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants lower	Participants lower/same	Participants higher	Participants higher
Mean Medicaid/health care costs	Gregory (2003) [1 State] Buescher (1993) [1 State] New York State (1991) [1 State][16] Devaney (1990/91) [4 States] Schramm (1986) [1 State] Schramm (1985) [1 State]	Devaney (1990/91) [1 State] Paige (1983) [4 counties][17]		
Likelihood of premature birth (<36-37 weeks gestation)	Gordon (1995) [national] Devaney (1990/91) [5 States] New York State (1990) [1 State] Rush (1988a) [national] Stockbauer (1987) [1 State]	Brien (1999) [national] {Blacks}[8] Frisbie (1997) [national][18] Rush (1988d) [national] Kotelchuck (1984) [1 State][6]	Brien (1999) [national] {Whites}	
Likelihood of intrauterine growth retardation/small-for-gestational-age birth	Frisbie (1997) [national] Stockbauer (1986) [1 State][7]	Stockbauer (1987) [1 State] {Blacks} Kennedy (1984) [4 areas in 1 State]		Stockbauer (1987) [1 State] {Whites}
Neonatal mortality (birth through early infancy, approximately 1 month)	Gregory (2003) [1 State] {Blacks} Moss (1998) [national] Devaney (1993) [4 States] Joyce (1988) [national] {Blacks} Stockbauer (1986) [1 State] {non-Whites} Kennedy (1984) [1 State][19] Kotelchuck (1984) [1 State]	Gregory (2003) [1 State] {non-Blacks} Brien (1999) [national] {Whites}[8] Gordon (1995) [national][14] Joyce (1988) [national] {Whites} Rush (1988a) [national] Rush (1988d) [national][19] Stockbauer (1987) [1 State] Schramm (1986) [1 State]	Brien (1999) [national] {Blacks}[8] Devaney (1993) [1 State]	Stockbauer (1986) [1 State] {Whites}

See notes at end of table.

Continued—

Table 18—Findings from studies that examined birth outcomes, including associated health care costs, by prenatal WIC participation status—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants lower	Participants lower	Participants higher	Participants higher
Infant mortality (later infancy through first year of life)	Gregory (2003) [1 State] {Blacks}[20] Devaney (1993) [4 States]	Brown (1996) [1 site] Gordon (1995) [national][14] Devaney (1993) [1 State] Rush (19881) [national]	Brien (1999) [national][21]	

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 1 city or 1 State). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may be indicative of a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

For studies that estimated more than one model, findings reported here reflect results for primary or baseline models. Unless otherwise noted, findings for alternative models were not qualitatively different.

Findings reported for Brien and Swann (1999) are based on two-stage model that controlled for selection bias, which was preferred by authors. The model did not control for variables that the authors considered to be endogenous, including age, income, living situation, use and adequacy of prenatal care, smoking, and use of alcohol and drugs. Unless otherwise noted, significance of effect (but not necessarily the direction) was the same for a model that defined WIC participation on the basis of status during the first trimester and a fixed effects model that estimated differences between pregnancies for the same women.

Findings reported for Kennedy and Kotelchuk (1984) are based on analyses for total sample. The paper also reports results by racial group for some outcomes; however sample sizes for non-Whites are small.

[1] Difference was positive but not significant in model that controlled for gestational age, models for three of the four gestational-age cohorts, and model that limited WIC participants to those who participated in first 6 months of pregnancy. Difference was negative, but not significant, in model for 28-week cohort.

[2] Size of impact was substantially greater among infants born prematurely (< 37 weeks gestation).

[3] Mixed results depending on comparison group used. Two out of three comparisons found positive, significant impact among non-white participants.

[4] Dose-response analyses found no significant impact.

[5] Mixed results depending on comparison group used, but estimates for one comparison were identical and for another were off by one gm.

[6] Authors reported significant difference at $p < 0.10$.

[7] Mixed results depending on comparison group used. Two out of three comparisons found positive, significant impact for both Whites and non-Whites.

[8] Impact was positive and significant in fixed-effects model.

[9] Significant difference noted for each of five annual cohorts (1996-2000), as well as for the full sample.

[10] Difference was favorable to WIC participants but not statistically significant for 37-week cohort. The number of very-low-birthweight infants in this sample was very small (28 vs. 742 in full sample).

[11] Difference was not statistically significant in model that controlled for gestational age.

[12] Impact was positive but not significant for 28- and 32-week cohorts and positive and significant for 36- and 40-week cohorts.

[13] Mixed results depending on comparison group used, but two comparisons showed positive WIC impact and one of these was significant.

[14] No significant impact in models for four gestational-length cohorts.

[15] mpact was not significant in models that limited WIC participants to those who enrolled by 30 weeks and 32 weeks.

[16] Reported significantly shorter hospital stay for WIC infants in all three insurance groups; however, analysis used simple t-tests. Medicaid hospital costs for WIC infants were lower than non-WIC infants, but the statistical significance of the difference was not tested.

[17] No statistical tests performed.

[18] Impact was positive and significant for probability of heavy preemie (WIC participants less likely to have heavy preemie).

[19] Finding reflects impact on *fetal* death rate rather than neonatal death rate because data were available only up to the time of birth.

[20] Difference among non-blacks was not statistically significant, however, because data were not presented, could not determine direction of difference.

[21] Impact was positive and significant, for blacks, in fixed-effects model.

deemed to be adequate in sample size and design. All of these studies are included in tables 17 and 18.[57] By statistically combining the results of these studies, GAO researchers estimated WIC's effect on reducing the incidence of low birthweight as well as the incidence of very low birthweight.[58] They then used this information to estimate the number of infants born in 1990 who would have been born with low birthweights if their mothers had not received WIC benefits. Finally, cost savings attributable to WIC were determined by combining the estimate of averted low birthweight and very low birthweight infants with information on the excess costs associated with caring for these infants. Cost estimates included short-term hospital costs, expected long-term disability costs, and expected special education costs. A substantial proportion of total costs were attributable to medical care costs in the first year of life.

The GAO researchers concluded that prenatal WIC participation reduced the incidence of low birthweight by 25 percent (estimates from the studies examined ranged from 10 percent to 43 percent) and the incidence of very low birthweight by 44 percent (study estimates ranged from 21 to 53 percent). When these estimates were applied to 1990 births and associated costs, result indicated that providing WIC services to mothers who delivered babies in 1990 would ultimately save more than $1 billion in costs for Federal, State, local, and private payers. Savings to the Federal Government were estimated at $337 million. These findings are the source of an oft-cited claim that "every dollar invested in [prenatal] WIC saves $3.50 in other costs."[59]

In commenting on the GAO report, USDA officials raised appropriate concerns that GAO's conclusions overstated the impact of WIC because (1) the reviewed studies used data collected between 1982 and 1988, but both Medicaid and WIC had changed substantially since then, (2) none of the reviewed studies was generalizable to the entire WIC population, and (3) GAO researchers relied most heavily on findings from the WIC-Medicaid Study, which was largely limited to the very lowest income WIC participants (GAO, 1992).[60] USDA officials also stressed that the report did not adequately caveat its findings in recognition of the selection-bias problem.

Since the GAO meta analysis was completed, 13 additional studies have examined WIC's impact on birthweight and/or Medicaid costs using techniques that were comparable to or better than those used in the studies reviewed by GAO. These include studies that involved national datasets (Finch, 2003; Kowaleski-Jones and Duncan, 2002: Hogan and Park, 2000; Brien and Swann, 1999; Covington, 1995; Gordon and Nelson, 1995), as well as studies that focused on one State (Roth et al., 2004; Gregory and deJesus, 2003; Reichman and Teitler, 2003; Buescher and Horton, 2000; Ahluwalia et al., 1998). Two other studies used data from the WIC-Medicaid Study (Devaney, 1992) and data from one hospital (Brown et al., 1996). With the exception of Brown et al. (1996), all of these studies reported a significant WIC impact overall or for at least one subgroup. Moreover, the studies by Kowaleski-Jones and Duncan (2002) and Brien and Swan (1999) included controls for selection bias that the authors deemed successful.

Taken as a whole, the available body of research provides strong, suggestive evidence that WIC has a positive impact on mean birthweight, the incidence of low birthweight, and several other key birth outcomes, and that these positive effects lead to savings in Medicaid costs. Even recognizing the pervasive self-selection problem and the fact that virtually all studies have other limitations that limit generalizability, the consistency of the results across studies is noteworthy. This is especially true when one considers that the bulk of the literature is comprised of relatively large, well-conducted studies, includes both national samples and State-level data that essentially amount to point-in-time censuses, and includes data from a number of different time periods.

Other reviewers have reached similar conclusions (Rossi, 1998; Currie, 1995). Currie (1995) offers the following observation:

> Without knowing more about the selection mechanism underlying participation in the program, it is difficult to

[57]In the GAO meta analysis, each of the five States studied by Devaney et al. (1990/91) in the WIC-Medicaid Study were considered as separate studies. Other studies included in the meta analysis were Silverman (1982), Kennedy et al. (1982), Kennedy and Kotelchuck (1984), Bailey et al. (1983), Metcoff et al. (1985), Stockbauer (1986, 1987), Schramm (1985, 1986), the NWE (Rush et al., 1986, 1988a, 1988d), and Buescher et al. (1993). (The GAO report used a 1991 version of the work Buescher and his colleagues published in 1993).

[58]Estimates related to the incidence of very low birthweight are based on data from 5 of the 17 studies that provided separate estimates for incidence of low and very low birthweight. In estimating reductions in very low birthweight attributable to WIC and the associated cost savings, the authors applied results from these studies to the other 12 studies.

[59]The $3.50 savings (calculated in 1990 dollars and assuming a 2-percent discount rate) accrues over 18 years. Savings in the first year of life were estimated at $2.89 per Federal dollar spent on prenatal WIC participation.

[60]The WIC-Medicaid Study estimated, with the appropriate caveats, that every dollar spent on prenatal WIC participation generated more than $1.00 in Medicaid savings. This analysis considered only Medicaid expenditures during the first 60 days after birth.

> assess the probable direction of the bias. However, the factors governing selection into the WIC Program are likely to vary considerably over time and across sites. "...Hence, the fact that the estimated effects are remarkably consistent across samples drawn from different states and at different times suggests that the positive results are not entirely driven by the selection of women who are likely to have good outcomes into the program." (p. 100).

Thus, the evidence that WIC participation during pregnancy positively influences birth-related outcomes is fairly convincing. Beyond that, however, little else is clear. Because of the design characteristics that contribute to inherent underestimation or overestimation of WIC impacts and the wide range of reported estimates, it is difficult to characterize the relative size of WIC's impact—for example, the estimated reduction in the prevalence of low birthweight infants—with any confidence. Moreover, subgroup analyses by some researchers suggest that WIC impacts may be stronger among Blacks and other minorities than among Whites (Gregory and deJesus, 2003; Brien and Swann, 1999; Stockbauer, 1986, 1987) and among those at the lowest income levels (Finch, 2004; GAO, 1992).

In addition, many important changes have taken place since the data used in most of this research were collected. These changes may influence the extent to which findings from previous research apply to today's WIC program. The most noteworthy changes include the following:

- A substantially higher level of program penetration in most areas of the United States than was present in the mid-to-late 1980s (that is, most eligible prenatal applicants are able to enroll and waiting lists tend to be the exception rather than the rule).
- More generous Medicaid income-eligibility criteria for pregnant women (including some that exceed the WIC cutoff of 185 percent of poverty), which infer automatic income-eligibility for WIC.
- The use of standardized nutritional risk criteria.

Welfare reform legislation, which did not affect WIC directly, may also have affected the circumstances of both WIC participants and nonparticipants. Any of these changes may influence both the presence and size of WIC impacts as well as variation in impacts across subgroups.

Two studies by Buescher and his colleagues illustrate how the prenatal WIC population in one State has changed over time. Both of these studies were limited to Medicaid participants in North Carolina. At the time of the first study in 1988, the Medicaid income-eligibility cutoff was 100 percent of poverty, and a total of 21,900 Medicaid births were included in the study (Buescher et al., 1993). At the time of the second study in 1997, the Medicaid cutoff for pregnant women was 185 percent of poverty, and the number of Medicaid births was almost double, at roughly 43,000 (Buescher and Horton, 2000). Although both studies found that WIC decreased the likelihood of low birthweight and very low birthweight, the magnitude of the differences between WIC participants and nonparticipants was smaller in 1997 than it had been in 1988 (odds ratios of 1.36 vs. 1.45 for low birthweight and 1.90 vs. 2.15 for very low birthweight).

Initiation and Duration of Breastfeeding: Research Overview

Impacts on breastfeeding are discussed in this section because, as mentioned previously, any impact WIC may have on the decision to breastfeed is clearly tied to nutrition education and/or breastfeeding promotion services provided to the mother during pregnancy. (Impacts on breastfeeding duration and other infant feeding practices may be influenced by WIC services provided after birth.)

The literature search identified few studies that assessed the impact of WIC on breastfeeding behaviors. Many identified studies examined the impact of specific breastfeeding promotion strategies/programs on *WIC participants*. However, such studies do not address the impact of the WIC program per se. That is, they provide no information on what breastfeeding initiation and duration rates would look like in the absence of the WIC program.

Official WIC policy has always encouraged breastfeeding. Both programmatic and research interest in this topic grew in the late 1980s and early 1990s, however, when national survey data indicated that breastfeeding rates were declining nationwide (as the WIC program was growing) and that the rate of breastfeeding among WIC participants was less than the national average and less than the rate for low-income nonparticipants.

Many investigators have examined predictors of breastfeeding behaviors. Results have been very consistent and have demonstrated that women who are African American, less educated, low-income, and younger are less likely to breastfeed than other women. These demographic characteristics are also associated with an increased likelihood of WIC participation, so it is not surprising that studies that have included WIC participation among the list of potential breastfeeding predictors have almost invariably found a negative association or no association between WIC participation and breastfeeding.

These negative statistics have prompted substantial commentary and questions over the years, particularly: Does the formula provided by WIC act as a disincentive to breastfeeding? and Does the WIC program devote adequate resources to breastfeeding promotion? Obtaining reliable answers to these questions is complicated by substantial selection bias that makes it more likely that researchers will find a negative association between WIC participation and breastfeeding. As just noted, the demographic characteristics of women who are least likely to breastfeed closely parallel the characteristics of women who are most likely to participate in WIC. In addition, it is reasonable to assume that women who have decided to formula-feed may be more likely to participate in WIC than women who have elected to breastfeed in order to obtain the free formula. The incentive to participate may be substantially reduced for women who have decided to breastfeed.

The literature review identified nine studies that attempted to estimate the impact of WIC participation on breastfeeding behaviors (table 19). Studies that used only t-tests or correlation coefficients to examine this relationship, without controlling for measured differences between groups, are not included. As just noted, these studies are virtually guaranteed to find a negative association or no association between WIC participation and breastfeeding because of the demographic characteristics of WIC participants.

Two components of the NWE examined breastfeeding in a fairly limited way (Rush et al., 1988d; 1988c) (Group I). Five studies used national survey data to study the impact of WIC on breastfeeding (Group II). Two of these studies used the NMIHS, one study used the NLSY, and two studies, including one study conducted by the GAO in response to a congressional request, used the Ross Laboratory Mother's Survey (RLMS). The RLMS, in various forms, has been ongoing for more than 40 years and is used to document national trends in infant feeding. The RLMS includes a mail survey of a large nationally representative sample of mothers of 6-month-old infants. The sample represents 70-82 percent of all new mothers in the United States (Ryan, et al., 1991). Response rates have generally been lower than desired for scientific surveys. Over the years, low-income women have exhibited the lowest response rate and have therefore been oversampled. Weights used in analyzing survey data are specifically designed to account for differences in response rates and coverage of various population subgroups (GAO, 1993; Ryan et al., 1991).

Finally, two State and local studies examined WIC impacts on breastfeeding and infant feeding practices (Group III). Burstein and her colleagues (1991) reported preliminary impact estimates from the field test of the WIC Child Impact Study. A much smaller, local study looked at the impact of multiple spells of participation on breastfeeding rates among Hmong and Vietnamese WIC participants in northern California (Tuttle and Dewey, 1994).

Initiation and Duration of Breastfeeding: Research Results

In the NWE, the Longitudinal Study of Women found that WIC participants were both less likely to plan to breastfeed (breastfeeding intention) and less likely to initiate breastfeeding in the hospital than income-eligible nonparticipants (Rush et al., 1988d) (table 20). However, study investigators discounted the finding about breastfeeding initiation because they believed it was influenced by a substantial amount of missing data in the hospital records that provided data for the analysis.

A study completed by Ryan and his colleagues in 1991, using RLMS data for 1984 and 1989, reported that breastfeeding rates, and extended breastfeeding in particular (6 months or more), declined disproportionately among WIC participants during this period. Even after controlling for measured differences between groups, nonparticipants were 1.5 times more likely than WIC participants to initiate breastfeeding in the hospital. This study contributed substantially to the debate about the role of the WIC program in promoting breastfeeding.

The reliability of these findings was called into question because of concerns about the adequacy of the single survey item used to classify WIC participants and nonparticipants and lack of attention to the issue of selection bias (Tognetti et al., 1991). The survey item used to identify WIC participants asked whether the mother or the target infant participated in WIC at any time since the infant's birth. This composite question did not allow differentiation of women who participated in WIC prenatally (and therefore had the opportunity to be exposed to WIC breastfeeding promotion advice and activities)

Table 19—Studies that examined the impact of the WIC program on breastfeeding

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Group I: National evaluations						
Rush et al. (1988c) (NWE)	Breastfeeding initiation and duration	Primary data collection in 174 WIC sites and 55 prenatal clinics (1983-84)	Random sample of infants and children of women included in the longitudinal study of women (see Rush et al., 1988d below) (n=2,370)	Participant vs. nonparticipant	Participation dummy based on age of inception into WIC, including prenatally	Multivariate regression
Rush et al. (1988d) (NWE)	Breastfeeding intention and initiation	Primary data collection in 174 WIC sites and 55 prenatal clinics (1983-84)	Nationally representative sample of pregnant WIC participants and comparison group receiving prenatal care in surrounding public health clinics or hospitals (n=3,935)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Group II: Secondary analysis of national surveys						
Chatterji et al. (2002)	Breastfeeding initiation and duration	1989-95 NLSY	(1) NLSY children born between 1990 and 1995 (n=1,282) (2) Low-income NLSY children born between 1991 and 1995 (n=517) (3) NLSY children born between 1989 and 1995, with at least one other sibling born during the same period (n=970)	Participant vs. nonparticipant	Participation dummy	(1) (2) Multivariate regression, including attempt to control for selection bias (3) Fixed-effects model

See notes at end of table.

Continued—

Table 19—Studies that examined the impact of the WIC program on breastfeeding—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Balcazar et al. (1995)	Breastfeeding intention	1988 NMIHS live births	Mexican-American and non-Hispanic White women who were not undecided about infant feeding plans prior to the infant's birth (n=4,089)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
GAO (1993)	Breastfeeding initiation	1989-92 RLMS	Nationally representative sample of mothers of 6-month-old babies. Analysis included all respondents with complete data for questions of interest (n=79,428)[3]	Prenatal participants vs. nonparticipants and postpartum-only participants	Participation dummy	Multivariate regression
Schwartz et al. (1992)	Breastfeeding initiation and duration	1988 NMIHS	WIC participants and income-eligible nonparticipants (n=6,170)	Participants who received advice to breastfeed compared with participants who did not receive advice and to income-eligible nonparticipants	Participation dummy and advice dummy	3-stage regression with selection-bias adjustment
Ryan et al. (1991)	Breastfeeding initiation and duration	1984 and 1989 RLMS	Respondents in 1984 and 1989 (n=120,334)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Group III: State and local studies						
Tuttle and Dewey (1994)	Breastfeeding initiation	Primary data collection in WIC clinics and neighborhoods in 1 northern California community	Hmong and Vietnamese WIC participants whose youngest child was less than 1 year (n=122)	Participant vs. nonparticipant	Dose response: Number of times previously participated in WIC	Multivariate regression

See notes at end of table.

Continued—

Table 19—Studies that examined the impact of the WIC program on breastfeeding—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Burstein et al. (1991)	Breastfeeding initiation and duration	Primary data collection in Florida and North Carolina (1990-91)	Random sample of WIC and income-eligible infants (6 months old) stratified by birthweight (n=807)	Participant vs. nonparticipant	Participation dummy	Multivariate regression, including attempt to control for selection bias

[1]Data sources:

NLSY = National Longitudinal Survey of Youth.

NMIHS = National Maternal and Infant Health Survey.

RLMS = Ross Laboratories Mother's Survey.

[2]Unless the description of the study sample indicates that a comparison group was limited to nonparticipants who were income eligible for WIC or known to be Medicaid participants, all income levels were included in the comparison group.

[3]Overall response rate for survey was approximately 50 percent. After excluding cases with incomplete data, analysis sample comprised only 34 percent of the initial survey sample.

Table 20—Findings from studies that examined the impact of the WIC program on breastfeeding

	Significant impact	No significant impact		Significant impact
Outcome	Participants higher	Participants higher	Participants lower	Participants lower
Intention to breastfeed	Balcazar (1995) [national] {with advice}[1]		Rush (1988d) [national]	Balcazar (1995) [national] {overall}[1]
Breastfeeding initiation	Tuttle (1994) [1 community][2] Schwartz (1992) [national] {advice}	Rush (1998c) [national]	Burstein (1991) [2 States] Rush (1998d) [national][3, 4]	Chatterji (2002) GAO (1993) [national][3,5] Schwartz (1995) [national] {no advice} Ryan (1991) [national][3]
Duration of breastfeeding		Schwartz (1992) [national] {no advice} Rush (1998c) [national]	Chatterji (2002)[6] Schwartz (1992) [national] {advice}	Ryan (1991) [national][7]

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 1 city or 1 State). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Findings reported for Chatterji et al. (2002) are based on the single-equation model, which the authors favored (see text).

Findings reported for Burstein et al. (1991) were consistent for single-equation and selection-bias-adjusted models.

[1] Overall, WIC participants were significantly less likely than nonparticipants to plan to breastfeed, either exclusively or in combination with formula feeding. However, women who participated in WIC and reported receiving advice to breastfeed were significantly more likely to plan to breastfeed.

[2] Number of times woman had previously participated in WIC was positively associated with initiation of breastfeeding.

[3] Limited to initiation of breastfeeding before hospital discharge.

[4] Results are highly suspect because data were missing for almost half of the subjects. The authors suspect that the relevant data element on hospital records was disproportionately skipped for women who did not breastfeed.

[5] Result reported is for comparison of prenatal WIC participants vs. nonparticipants. Comparison of prenatal and postpartum-only WIC participants revealed virtually no difference between the two groups.

[6] Difference was statistically significant in fixed-effect model.

[7] Based on odds ratio of breastfeeding when infant is 6 months old.

from those who participated only after the birth of the child. The combination of prenatal and postpartum participants and, potentially, infant-only participants, may have diluted the apparent WIC effect.

GAO (1993) used RLMS data for 1992 to address congressional questions about the effectiveness of WIC's current breastfeeding promotion efforts.[61] GAO's analysis included a multivariate regression to investigate the relationship between prenatal WIC participation and initiation of breastfeeding in the hospital. Results showed that, after controlling for differences in measured characteristics (including education, income, race, and a variety of other characteristics known to be associated with breastfeeding rates), prenatal WIC participants were just as likely as postpartum-only participants to initiate breastfeeding. Moreover, prenatal WIC participants were significantly less likely than nonparticipants to initiate breastfeeding. Study authors cautioned that the analysis did not control for selection bias and that unmeasured characteristics, whether related to the woman herself or her interaction with the WIC program, may have contributed to the observed differences between WIC participants and nonparticipants.

In 1992, Schwartz et al. used data from the NMIHS to examine the impact of WIC on breastfeeding. They estimated three equations jointly and simultaneously to control for self-selection and to model the decision to initiate breastfeeding and, for those who breastfed, the duration of breastfeeding. The analysis looked at the combined influence of participating in WIC and receiving advice and encouragement from WIC staff to breastfeed. In the joint model, the coefficient for WIC participation was significant and negative and the coefficient for receiving breastfeeding advice was significant and positive. The interpretation is that the impact of WIC on breastfeeding was mediated by whether the woman was encouraged by WIC staff to breastfeed her infant. After controlling for socioeconomic differences, prenatal WIC participants who reported having received advice/encouragement to breastfeed were more likely to initiate breastfeeding than either participants who did not receive such advice or income-eligible nonparticipants. In contrast, WIC participants who did not report receiving such advice/encouragement to breastfeed were significantly less likely to initiate breastfeeding than income-eligible nonparticipants. Neither WIC participation nor receipt of breastfeeding advice had a significant impact on the duration of breastfeeding.

Balcazar et al. (1995) used NMIHS data to assess predictors of breastfeeding intentions and found a similar relationship between receiving advice/encouragement to breastfeed and reported breastfeeding intentions. While the relationship between WIC participation and breastfeeding intentions was negative overall, the relationship was positive among women who reported receiving breastfeeding advice/encouragement from WIC staff. In addition, receiving advice/encouragement to formula feed was negatively associated with breastfeeding intentions.

An obvious concern about both of these studies is whether self-reported data about receiving advice are biased in any way. For example, women who breastfed could have been more apt to report having gotten advice to do so. Or, WIC staff could have provided breastfeeding advice/encouragement to women who indicated an interest in breastfeeding. To address this issue, Schwartz and his colleagues estimated an alternative, two-stage equation that omitted the breastfeeding advice variable. The alternative model yielded results that were substantially different from the results (reported above) for the three-stage model. In the two-stage model, the coefficient for WIC participation, which was strongly and significantly negative in the initial three-stage model, was positive and not statistically significant, suggesting that WIC participation had no impact on breastfeeding initiation. The fact that the two models produced such divergent results is somewhat troubling. Given the potential problems with the reliability of data on breastfeeding advice, one must question the authors' uncaveated preference for the three-stage model.

The most recent study of WIC's impact on breastfeeding was completed in 2002 by Chatterji and colleagues. They used data from the NLSY to examine breastfeeding initiation and duration among children born between 1989 and 1995. WIC participation was defined based on the mother's participation during the year of the child's birth.[62] No information was available on whether WIC participants received advice or encouragement to breastfeed.

[61] Although recognizing the potential problem of nonresponse bias in the RLMS data, GAO researchers pointed out that survey weights were specifically designed to deal with this issue and that estimates of national breastfeeding rates derived from the RLMS were consistent with those of key government-sponsored national survey efforts, including the NMIHS and the National Survey of Family Growth (NSFG).

[62] The authors also completed parallel analyses using a variable that defined WIC participation based on participation during pregnancy or at the time of birth. Results of these analyses were reportedly "very similar" but were not presented. In addition, for children born in 1994, WIC participation was proxied based on WIC participation during the year that preceded the child's birth because data on WIC participation were not available for 1994.

The authors used two different approaches to control for selection bias—a two-stage model and a fixed-effects model that used data for sibling pairs. To model the participation decision in the two-stage model, the authors used variables that represented State-level WIC and Medicaid policies. Information on State-level WIC policies were obtained from the biennial WIC Participant and Program Characteristics (WIC PC) Studies, so assigning values to individual sample members was somewhat imprecise. Values for children born in years for which WIC PC data were not available (1991, 1993, and 1995) were assigned based on WIC PC data for the following year. In addition to Medicaid income eligibility cutoffs, State-level factors considered in the model included links between WIC and Medicaid, TANF, and FSP, WIC policies about income documentation, and the presence of nutrition-based restrictions on WIC food packages.

The authors describe several other variables that were considered but ultimately excluded from the model because they did not pass the test of over-identifying restrictions or "were very poor predictors of WIC participation." These variables included monthly (as opposed to less frequent) voucher issuance, nutritional risk criteria, nonnutrition-based food package restrictions (for example, restrictions related to package size or brand), and costs of WIC food packages. The fact that these variables were excluded from the final model suggests that the selection-adjustment model, like those used in research on birth outcomes, was very sensitive to changes in specification.

The authors reported results for a standard regression (baseline model), the selection-adjusted model, and the fixed-effects model. For baseline and selection-adjusted models, impacts were estimated for the full sample as well as for a low-income sample. Outcomes included breastfeeding initiation and whether breastfeeding lasted for 16 weeks. For the fixed-effects model, the dependent variable was the number of weeks the child was breastfed, including zeros for nonbreastfed infants. The fixed-effects model included 970 children who had one or more siblings in the sample; however, only about 15 percent of these children lived in a family where WIC participation varied across siblings.

Results of baseline regressions showed a significant, negative association between WIC participation and breastfeeding initiation in both the full sample and the low-income sample. Coefficients for breastfeeding duration (whether infant was breastfed for at least 16 weeks) were also negative for both samples, but differences between WIC participants and nonparticipants were not statistically significant. Results of the two-stage models yielded no significant findings, although coefficients for WIC participation were consistently negative. The fixed-effects model found that WIC participation had a significant, negative effect on breastfeeding duration (mean number of weeks breastfed).

Although the authors say that their instruments performed fairly well, they ultimately rejected the selection-adjusted results—which found no significant WIC effect—in favor of the baseline regression results-which found a negative WIC effect. The rationale for this decision was that Hausman tests suggested that WIC participation was not endogenous. This conclusion is open to question, given that the Hausman test depends heavily on the availability of good instruments (Carlson and Senauer, 2003). Moreover, the authors clearly stated that their hypothesis was that "despite the important efforts the WIC program has made to increase breastfeeding during the 1990s, WIC participation is still associated with lower rates of breastfeeding because of the valuable infant formula available to participants."

Viewed in concert, the available studies provide no firm ground for making causal inferences about the impact of WIC on breastfeeding initiation or duration. Statistics do show, however, that breastfeeding rates among WIC participants have been increasing. The RMLS shows a 69-percent increase between 1990 and 2000 in the percentage of WIC mothers who initiate breastfeeding and a 145-percent increase in the percentage who were still breastfeeding at 6 months (Oliveira, 2003). This increase cannot be attributed to the WIC program because breastfeeding rates have been climbing for the population overall. However, since the late 1980s, USDA has specifically targeted promotion of breastfeeding in the WIC program (USDA/FNS, 2003a). For example, in 1989, P.L. 101-147 required that USDA develop standards for breastfeeding promotion and support and targeted $8 million for State-level efforts in this area. In 1992, P.L. 102-342 required that USDA establish a national breastfeeding promotion program. That same year, USDA instituted an enhanced food package for women who exclusively breastfeed. The enhanced package has additional amounts of juice, cheese, and legumes and includes carrots and canned tuna. In 1994, P.L. 103-448 increased the amount of money each State was required to devote to breastfeeding promotion and required that all States collect data on the incidence and duration of breastfeeding among WIC participants.

Finally, in 1998, P.L. 105-336 authorized the use of State administrative funds for the purchase or rental of breast pumps.

USDA has also implemented several breastfeeding promotion demonstrations and has disseminated findings and recommendations to State and local WIC agencies.[63] Evaluations of several of these demonstrations have found that breastfeeding promotion efforts during pregnancy can positively effect the initiation of breastfeeding among low-income women and that support during the postpartum period can positively influence breastfeeding duration. It is beyond the scope of this review to summarize these initiatives. However, the interested reader is referred to Weimer (1998), Bronner et al. (1994), and Sanders et al. (1990).

In 1996, USDA entered into a cooperative agreement with Best Start Social Marketing to develop and implement a national breastfeeding promotion campaign. The program was officially launched in August 1997. In 2003, the program was expanded to include training programs for WIC staff in implementing and managing peer counselor programs.

Clearly, WIC's focus on breastfeeding promotion has increased substantially since the NMIHS, which probably provided the best data, was conducted. While it makes sense for USDA to focus research efforts on identifying effective breastfeeding promotion strategies, it would also be useful to obtain updated impact analyses. Additional work with the NLSY data may provide some insights, and the Panel Study of Income Dynamics Child Development Supplement (PSID-CDS), which was implemented in 1997, may also be a useful data source. This longitudinal study collects information on both breastfeeding and WIC participation (Logan et al., 2002).

Nutrition and Health Characteristics of Pregnant Women: Research Overview

Ten of the identified studies looked at the impact of prenatal WIC participation on the nutrition or health characteristics of pregnant women themselves (table 21).[64] These studies include both of the national WIC evaluations (Group I), two studies that were based on secondary analysis of data from NHANES-III (Group II), one large State study (Group III), and five small, local studies (Group IV).

Nutrition and health characteristics examined in these studies include dietary intake (six studies), nutritional biochemistries—most often iron status or the prevalence of anemia (five studies), and weight gain during pregnancy (four studies). Like much of the research on WIC impacts, most of these studies are quite dated. At least three of the studies (Kennedy and Gershoff, 1982; Endres et al., 1981; Edozien et al., 1979) are based on data collected in the 1970s. One study published in 1983 (Bailey et al.) did not report the dates that data were collected. Only three studies (Roth et al., 2004; Mardis and Anand, 2000; Kramer-LeBlanc et al., 1999) are based on data that were collected after 1984. Roth et al. (2004)—the most recent study—focused primarily on impacts on birth outcomes; of the outcomes discussed in this section, only weight gain during pregnancy was included.

Both Mardis and Anand (2000) and Kramer-LeBlanc et al. (1999) examined dietary intakes and used bivariate t-tests to assess differences between participants and nonparticipants. Thus, while these studies are useful in understanding observed differences between dietary intakes of WIC participants and nonparticipants, based on relatively recent data, they do not provide valid estimates of WIC impacts. Both studies used the same dataset (NHANES-III) and the same samples. Mardis and Anand's analysis focused on food group intakes, while Kramer-LeBlanc's analysis looked at nutrient intakes.

None of the studies that examined the relationship between prenatal WIC participation and the nutrition and health characteristics of pregnant women attempted to control for selection bias. However, one of the local studies (Metcoff et al., 1985) is the only study known to have used a randomized design to study WIC impacts. The authors were able to use a randomized design because, at the time data were collected—early in the WIC program's history—the need for WIC services in the area under study exceeded available resources.

Nutrition and Health Characteristics of Pregnant Women: Research Results

Dietary Intake

With the exception of the descriptive analyses completed by Mardis and Anand (2000) and Kramer-LeBlanc et

[63]It is beyond the scope of this review to summarize these initiatives, however, the interested reader is referred to Weimer, 1998, Bronner et al., 1994, and Sanders et al., 1990.

[64]Fraker, Long, and Post (1990) attempted to examine the impact of WIC participation on all types of women (pregnant, breastfeeding, and postpartum combined) using the 1985 CSFII data. However, because of the very small sample of WIC participants (64), the authors recommended that results be considered investigatory only.

Table 21—Studies that examined the impact of the WIC program on nutrition and health outcomes of pregnant women

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Group I: National evaluations						
Rush et al. (1988d) (NWE)	Dietary intake, prevalence of anemia, pregnancy weight gain	Primary data collection and record abstractions in 174 WIC sites and 55 prenatal clinics (1983-84). Data were collected at time of enrollment into WIC or prenatal care and again at about 8 months gestation	Nationally representative sample of pregnant WIC participants and comparison group receiving prenatal care in surrounding public health clinics or hospitals (n=3,473)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Edozien et al. (1979)	Dietary intake, hemoglobin, prevalence of anemia, pregnancy weight gain	Primary data collection in 19 sites in 14 States (1973-76). Data were collected at time of WIC enrollment, approximately every 3 months until delivery, and once after delivery	Pregnant women who enrolled in WIC (n~2,885)[3]	(1) Nutritional biochemistries: Participants, before vs. after, separate groups (2) Dietary intake: Participants, before vs. after, same women	Dose response: Newly enrolling participants vs. participants with varying length of participation	Multivariate regression
Group II: Secondary analysis of national survey data						
Mardis and Anand (2000)	Dietary intake	1988-94 NHANES-III	WIC and income-eligible women (n=242)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Kramer-LeBlanc et al. (1999)	Dietary intake	1988-94 NHANES-III	WIC and income-eligible women (n=242)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests

See notes at end of table.

Continued—

Table 21—Studies that examined the impact of the WIC program on nutrition and health outcomes of pregnant women—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Group III: State-level studies using WIC participation files matched with Medicaid and/or birth record files						
Roth et al. (2004)	Pregnancy weight gain	Linked WIC, Medicaid, and vital statistics records for births in Florida between January 1996 and the end of December 2000	WIC and non-WIC Medicaid recipients who did not participate in high-risk obstetrical program (n=295,599)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Group IV: Other State and local studies						
Collins et al. (1985)	Pregnancy weight gain	Primary data collection in public health department clinics in 6 Alabama counties (1980-81)	WIC and non-WIC pregnant women (n=519)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Metcoff et al. (1985)	Variety of nutritional biochemistries	Primary data collection at a prenatal clinic in 1 hospital in Oklahoma (1983-84)	Income-eligible pregnant women selected at mid-pregnancy based on predicted birthweight; roughly equivalent numbers were predicted to have average-size babies vs. small or large babies (n=410)	Randomized experiment	Participation dummy	Multivariate regression
Bailey et al. (1983)	Dietary intake, nutritional biochemistries	Primary data collection at 1 WIC site and 1 non-WIC site in Florida (Dates not reported)	WIC and income-eligible nonparticipants were 30 weeks pregnant at time of recruitment and receiving identical prenatal care (n=101)	Participant vs. nonparticipant	Participation dummy	Analysis of variance

See notes at end of table.

Continued—

Table 21—Studies that examined the impact of the WIC program on nutrition and health outcomes of pregnant women—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Kennedy and Gershoff (1982)	Hemoglobin and hematocrit levels	WIC and medical records in WIC sites and non-WIC health facilities in 4 geographic areas of Massachusetts (1973-78)	WIC and WIC-eligible women[4] (n=232)	Participants vs. nonparticipants, before and after	Dose response: Number of WIC vouchers received	Multivariate regression
Endres et al. (1981)	Dietary intake	Dietary recalls for sample of pregnant WIC participants in 22 counties in Illinois (1978-79)	Newly enrolling pregnant WIC participants and participants who were on the program for 6 months or more (n=766)	Participants, before vs. after, separate groups	Participation dummy	Bivariate t-tests

[1] Data source: NHANES = National Health and Nutrition Examination Survey.

[2] Unless the description of the study sample indicates that a comparison group was limited to nonparticipants who were income eligible for WIC or known to be Medicaid participants, all income levels were included in the comparison group.

[3] Approximate maximum; sample size varied for each measure and analysis approach.

[4] Subset of participants in larger study focusing on impact of WIC on birthweight (see table 5). WIC-eligible women included in the nonparticipant group were wait-listed for WIC during their pregnancy, enrolled in WIC postpartum, or were women who received prenatal care at non-WIC health care facilities in same neighborhood but never enrolled in WIC.

al. (1999), all of the studies that have assessed the impact of WIC participation on the dietary intake of pregnant women are quite old. Indeed, aside from these two studies, the most recent study is the NWE (Rush et al.,1988d), which used data collected in 1983-84. Findings from such dated studies are subject to concerns about changes in the program and its participant groups over time, as discussed in the preceding section on birth outcomes.

In addition, a compelling argument can be made that impacts on diet-related outcomes are even more sensitive to temporal considerations than impacts on other outcomes. For example, the American food supply has changed dramatically since the early 1980s, with important implications for observed dietary intakes. Americans are eating substantially more grains than they were two decades ago, particularly refined grains, as well as record-high amounts of caloric sweeteners and some dairy products and near-record amounts of added fats (Putnam and Gerrior, 1999). Over time, myriad new products have come onto the market and food enrichment policies and standards have changed. In addition, food purchasing behaviors may have been influenced by including, for example, more food eaten away from home, smaller households, more two-earner and single-parent households, and increased ethnic and racial diversity (Putnam and Gerrior, 1999). These factors make the recent studies by Mardis and Anand (2000) and Kramer-LeBlanc et al. (1999), although strictly descriptive, important for understanding potential WIC impacts in the current environment.

All of the available research on dietary intakes of pregnant women is also subject to the limitations that affect most of the available research on diet-related impacts of FANPs, as discussed in chapter 2. All of the available studies used intake data for a single day and, therefore, provide weak estimates of individuals' usual dietary intake. In addition, in assessing intakes of food energy, vitamins, and minerals, researchers generally compared mean intakes of participants and nonparticipants relative to the Recommended Dietary Allowances (RDAs), or compared the proportion of individuals in each group with intakes below a defined cutoff, using a "more is better" approach in interpreting findings. None of the studies used the approach recently recommended by the IOM, which calls for use of data on usual intake and comparisons to defined Estimated Average Requirements (EARs) (IOM, 2001).

Consequently, the available research provides an imperfect picture of the substantive significance of observed differences in the dietary intakes of prenatal WIC participants and nonparticipants. It provides information on whether pregnant WIC participants consumed more or less energy and nutrients than pregnant nonparticipants, but this information cannot be used to conclude that WIC participants were more or less likely than nonparticipants to have adequate intakes.

Finally, as noted in chapter 2, the estimation of food and nutrient intake is an elaborate process that is subject to significant measurement error. This error may make detecting differences between participant and nonparticipant groups difficult.

Although subject to the above limitations, as well as to potential selection bias, evidence from early studies paints a reasonably consistent picture of WIC's impacts on the dietary intakes of pregnant women (table 22). The evidence suggests that WIC participation increased intakes of food energy and most of the nutrients examined, including four of the five nutrients traditionally targeted by the program—protein, vitamin C, iron, and calcium. Evidence for vitamin A, the fifth WIC nutrient, is less consistent, but vitamin A intake is especially difficult to estimate because the distribution is so skewed (vitamin A is concentrated in large amounts in relatively few foods). The early evidence also suggests that WIC increased intakes of vitamin B_6, which the program has targeted in recent years. The NWE (Rush et al., 1988d) also found that pregnant WIC participants consumed significantly more fat than nonparticipants. However, if intake is translated into percentage contribution to energy intake (using reported means for fat and energy), both groups consumed about 37 percent of energy from fat.

NWE authors (Rush et al., 1988d) pointed out that the relative magnitude of the incremental intakes observed among pregnant WIC participants were plausible in that they were comparable to the levels of supplementation achieved in smaller, intensively controlled clinical trials. Moreover, a thorough analysis of the sources of nutrients in women's diets completed for the NWE confirmed that differences in the diets of WIC participants and nonparticipants were attributable to consumption of WIC foods.

Other authors also found a relationship between observed differences in nutrient intake and the types of food provided in WIC food packages. Endres et al. (1981) found that pregnant WIC participants consumed milk, juice, and fortified cereals more often than pregnant nonparticipants (statistical significance

Table 22—Findings from studies that examined the impact of the WIC program on the dietary intakes of pregnant women

Outcome	Significant impact: Participants consumed more	No significant impact: Participants consumed more/same	No significant impact: Participants consumed less	Significant impact: Participants consumed less
Food energy and macronutrients				
Energy[1]	Rush (1988d) [national] Endres (1981) [1 State]	Bailey (1983) [2 sites]	Kramer-LeBlanc (1999) [national]	
Protein	Rush (1988d) [national] Endres (1981) [1 State] Edozien (1979) [national]		Kramer-LeBlanc (1999) [national]	
Fat	Rush (1988d) [national]		Mardis (2000) [national] Kramer-LeBlanc (1999) [national]	
Saturated fat			Mardis (2000) [national] Kramer-LeBlanc (1999) [national]	
Carbohydrates	Rush (1988d) [national]	Kramer-LeBlanc (1999) [national][2]		
Vitamins				
Vitamin A[1]	Endres (1981) [1 State]	Rush (1988d) [national]	Kramer-LeBlanc (1999) [national]	
Vitamin B_6	Rush (1988d) [national] Bailey (1983) [2 sites] Endres (1981) [1 State]		Kramer-LeBlanc (1999) [national]	
Vitamin B_{12}	Rush (1988d) [national] Endres (1981) [1 State]		Kramer-LeBlanc (1999) [national]	
Vitamin C	Rush (1988d) [national] Endres (1981) [1 State] Edozien (1979) [national]	Kramer-LeBlanc (1999) [national]		
Vitamin D	Endres (1981) [1 State]		Kramer-LeBlanc (1999) [national]	
Vitamin E		Endres (1981) [1 State]	Kramer-LeBlanc (1999) [national]	

See notes at end of table.

Continued—

Table 22—Findings from studies that examined the impact of the WIC program on the dietary intakes of pregnant women—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Folate	Endres (1981) [1 State]	Bailey (1983) [2 sites]	Kramer-LeBlanc (1999) [national]	
Niacin[1]	Rush (1988d) [national] Endres (1981) [1 State]		Kramer-LeBlanc (1999) [national]	
Riboflavin	Rush (1988d) [national] Endres (1981) [1 State] Edozien (1979) [national]		Kramer-LeBlanc (1999) [national]	
Thiamin	Rush (1988d) [national] Endres (1981) [1 State] Edozien (1979) [national]		Kramer-LeBlanc (1999) [national]	
Minerals				
Calcium	Rush (1988d) [national] Endres (1981) [1 State] Edozien (1979) [national]		Kramer-LeBlanc (1999) [national]	
Iron	Rush (1988d) [national] Bailey (1983) [1 site] Endres (1981) [1 State] Edozien (1979) [national]	Kramer-LeBlanc (1999) [national]		
Magnesium	Rush (1988d) [national] Endres (1981) [1 State]		Kramer-LeBlanc (1999) [national]	
Phosphorus	Rush (1988d) [national] Edozien (1979) [national]		Kramer-LeBlanc (1999) [national]	
Zinc	Endres (1981) [1 State]		Kramer-LeBlanc (1999) [national]	
Other dietary components				
Cholesterol			Mardis (2000) [national] Kramer-LeBlanc (1999) [national]	
Fiber			Kramer-LeBlanc (1999) [national]	

See notes at end of table.

Continued—

Table 22—Findings from studies that examined the impact of the WIC program on the dietary intakes of pregnant women—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Sodium			Mardis (2000) [national] Kramer-LeBlanc (1999) [national]	
Added sugars		Kramer-LeBlanc (1999) [national]		

Note: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 1 city or 1 State). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Findings for Mardis and Anand (2000) and Kramer-LeBlanc et al. (1999) are based on the same dataset. Both authors compared intakes of WIC participants and income-eligible nonparticipants in NHANES-III. The former compared means and the latter compared medians. Both authors also presented data for higher income nonparticipants.

Kramer-LeBlanc et al. (1999) also reported data for copper, potassium, retinol, pantothenic acid, selenium, and carotenes. With the exception of selenium (significant, with participants consuming less than nonparticipants) and carotenes (not significant, with participants consuming more than nonparticipants), all differences between participants and nonparticipants were nonsignificant, with participants consuming less than nonparticipants.

Findings reported for Rush et al. (1998d) are based on comparison of 24-hour mean intakes during late pregnancy, adjusted for baseline intake, for non-WIC participants and women who were WIC participants at entry into the study (181 women who moved from treatment to control groups over the course of the study were analyzed separately). Report also included results for analysis of intake from WIC foods, which were identical, except that vitamin A intake was also significant.

[1] Edozien (1979) reported no WIC effect for energy, vitamin A, and niacin, but point estimates are not provided. Text contradicts table in that text refers to significant impacts for vitamin A and niacin.

[2] For carbohydrates as a percentage of total energy intake. For intake in absolute gm, intake was lower among WIC participants, but the difference was not statistically significant.

not reported) and consumed greater total quantities of milk. Bailey et al. (1983) found that pregnant WIC participants ate fortified cereals, a major source of iron, more often than pregnant nonparticipants.

Results from early research do not permit an assessment of the potential impact of WIC on intake of folic acid. All of the available studies were completed before the recent widespread fortification of cereals and grain products with folic acid and before the increased attention to folic acid supplementation during pregnancy. Inadequate intake of folic acid has been associated with neural tube defects (Centers for Disease Control and Prevention (CDC), 1992).

Findings from the recent Kramer-LeBlanc et al. (1999) analysis of data from NHANES-III stand in stark contrast to the patterns described above. In that analysis, the only nutrient for which a significant difference was detected in median intakes of pregnant WIC participants and income-eligible nonparticipants was selenium. A comparison of the nutrient intakes of WIC participants and the maximum nutrient contribution of the WIC food package for pregnant women suggested that WIC participants may not have redeemed all of their vouchers or consumed all of the food provided.

As noted previously, the Kramer-LeBlanc et al. analysis was strictly descriptive and does not constitute a valid assessment of WIC impacts. Moreover, the analysis may have been hampered by small sample sizes (only 71 WIC participants). Nonetheless, the fact that findings from this study show virtually no overlap with findings from earlier studies raises a question about changes in the intakes of pregnant women over time. Consequently, positive findings from earlier studies cannot be assumed to apply to today's prenatal WIC participants.

Only one study (Mardis and Anand, 2000) assessed intakes of prenatal WIC participants and nonparticipants in relation to consumption patterns recommended in the *Dietary Guidelines for Americans*.[65] This analysis found no significant differences in intakes of total fat, saturated fat, cholesterol, or sodium. Moreover, with the exception of cholesterol, intakes of both participants and nonparticipants exceeded recommended levels. With regard to food intake, no significant differences were detected between WIC participants and nonparticipants in consumption of grains, vegetables, fruits, milk, or meats and beans.

[65]Kramer-LeBlanc et al. (1999) also report data for intake of total fat, saturated fat, cholesterol, and sodium, but it is the same data reported in Mardis and Anand (2000).

Given the increasing prevalence of pregnancy-associated obesity (Lederman et al., 2002) and the potential role of the WIC program in curtailing this problem, it is important to obtain valid estimates of WIC's impacts on women's dietary intakes based on more up-to-date information.

Nutritional Biochemistries

Five studies examined the impact of WIC participation on nutritional biochemistries of pregnant women. The most commonly examined outcomes were hemoglobin/hematocrit and the prevalence of anemia. The two national WIC studies looked at hemoglobin levels and reported conflicting results. The NWE (Rush et al., 1988d), which had a comparatively stronger research design, compared final hemoglobin measurements of pregnant women who were and were not participating in WIC. The analysis controlled for length of gestation and a number of other covariates and found no significant difference between the two groups. Edozien et al. (1979) compared hemoglobin levels for newly enrolling pregnant women and women who had been in WIC for less than 3 months and more than 3 months, adjusting for a number of covariates. The authors reported significant differences for both comparisons (women who had been enrolled in WIC for either length of time had significantly lower levels of anemia than newly enrolling pregnant women). This finding was not internally consistent with other measures of iron status included in the study, however, so it must be interpreted with caution.

Kennedy and Gershoff (1982) used multivariate regression techniques to compare final hemoglobin levels (generally measured at 34 weeks gestation or later) among WIC participants and nonparticipants, using the number of WIC vouchers received as the independent variable. The authors reported that WIC participation had a positive, significant effect on final hemoglobin levels.

Using a small sample of women in their 30th week of pregnancy (43 participants and 58 controls), Bailey et al. (1983) looked at biochemical indicators of iron, folate, and vitamin B_6 status. The authors found no significant difference between WIC participants and nonparticipants in mean hematocrit levels. They did find a positive, significant difference for transferrin saturation (a measure of iron status) and a significant, negative difference for serum folacin (a measure of folate status). The authors cautioned, however, that serum folate is very sensitive to short-term dietary intake (foods consumed shortly before the measure is taken) and is therefore not a good indicator of long-term nutriture or tissue stores of folate. The study also examined red blood cell folacin, a better measure of tissue stores, and found no significant difference between WIC participants and nonparticipants.

Finally, Metcoff and his colleagues (1985) examined 16 different nutritional biochemistries, assessing change between mid- and late pregnancy. After controlling for baseline values, the week of gestation at which the first measurements were taken, and the interval between measurements, the authors found no significant differences between pregnant women who were randomly assigned to WIC and non-WIC groups.

The relative paucity of research and the disparity in design and analytic techniques used in the studies that have been completed make it impossible to draw any firm conclusions about the impact of WIC participation on the nutritional biochemistries of pregnant women. The relationship may, indeed, be difficult to elucidate. As Rush et al. (1988d) pointed out, assessment of hemoglobin concentration, arguably the most straightforward and widely used measure of nutritional status among other population groups, is complicated during pregnancy by numerous physiologic processes that are not completely understood. Rush and his colleagues contended that adequate assessment of iron status during pregnancy requires the collection of several, more complex hematologic indices (transferrin saturation and serum iron) that are not readily available in most WIC or medical records.

Weight Gain During Pregnancy

Both of the national WIC evaluations (Edozien et al.,1979; Rush et al.,1988d) examined weight gain during pregnancy, which is known to be associated with adequate birthweight. Edozien et al. reported a positive impact, but Rush and his colleagues found no effect. A study completed in 1985 by Collins et al., as well as a stronger and more recent study by Roth et al. (2004) also found no effect.

Assessing the impact of WIC on weight gain during pregnancy may be subject to considerable measurement error. In order to gauge total weight gain, pre-pregnancy weight must be known, and in many cases, this is self-reported by the woman. If pre-pregnancy weights are not reliable, it is impossible to determine accurately how much weight was gained during pregnancy and to assess the relative adequacy of the weight gain. Widely accepted recommendations published by the IOM specify ranges of pregnancy weight gain and recommend that women who are underweight at the start of pregnancy gain somewhat more weight than the average woman and women who are overweight at the start of pregnancy gain somewhat less weight than the average (IOM, 1990). In most recent studies of WIC impacts, weight gain, if assessed at all, has been used as a covariate in analyses examining impacts on infant birthweight rather than as a main outcome.

Impacts of WIC Participation on Infants and Children

Although infants and children make up more than three-quarters of the total WIC population, very little research has been done on these participant groups until recently. In the late 1980s and early 1990s, FNS undertook a 5-year effort to design and field-test a longitudinal study of the short- and long-term impacts of WIC on infants and children. The University of North Carolina (UNC) and the Research Triangle Institute (RTI) completed a feasibility study in 1989 (Kotch et al., 1989) and a proposed a matched comparison group design. FNS had some concerns about the feasibility of creating adequately matched groups using State vital statistics data, however, and decided to conduct a field test of the proposed design and to develop and test an alternative design.

In 1989, Abt Associates Inc. and the Johns Hopkins University completed a field test of two alternative research designs—the original quasi-experimental design proposed by the UNC/RTI team as well as a modified experimental design (Puma et al., 1991). Researchers used experiences from the field test, including preliminary estimates of program impacts, to propose a design for a national evaluation of the impact of WIC on infants and children. FNS was in the process of reviewing proposals submitted by research organizations interested in implementing the project when Congress canceled the project.

Today, we still do not have solid answers to many of the questions the WIC Child Impact Study would have addressed. But a number of recent studies have begun to fill this critical information gap.

Research Overview

The literature search identified 41 studies that examined the relationship between WIC participation and nutrition and health outcomes of infants and children. Characteristics of these studies are summarized in table 23. The two national WIC evaluations are represented (Group I). Group II includes 16 studies that used

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Group I: National evaluations						
Rush et al. (1988c) (NWE)	Dietary intake, weight, height, head circumference, arm circumference and skinfold thickness, immunization status, use of preventive health care, behavior, vocabulary, and memory	Primary data collection in 174 WIC sites and 55 prenatal clinics (1983)	Random sample of infants and children ages 0-4 of women included in the longitudinal study of women (see Rush et al. (1988d) in table 17) (n=2,370)	Participant vs. nonparticipant	Participation dummy based on age of inception into WIC, including prenatally	Multivariate regression
Edozien et al. (1979)	Dietary intake, blood iron measures, height, weight, and head circumference	Primary data collection in 19 WIC sites in 14 States. Data collected at time of WIC enrollment and again after 6 and 11 months of participation (1973-76)	WIC infants and children ages 6-47 (n=16,000+)[3]	Participants, before vs. after	Participation dummy	Multivariate regression
Group II: Secondary analysis of national surveys						
Cole and Fox (2004)	Dietary intake, infant feeding practices, height, weight, variety of nutritional biochemistries, general health status, and dental health	1988-94 NHANES-III, usual intake	WIC and income-eligible children ages 1-4 (n=3,006)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Ponza et al. (2004)	Dietary intake	2002 FITS, usual intake	WIC and non-WIC infants and children ages 2-24 months (n=3,022)	Participant vs. nonparticipant	N/A	Comparison of means and proportions (no statistical tests reported)

See notes at end of table.

Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Siega-Riz et al. (2004)	Dietary intake	1994-96 and 1998 CSFII	WIC- and income-eligible children ages 2-5 who were not enrolled in school, in 2 income groups: <130% of poverty (n=1,772) and 130-185% of poverty (n=689)	Participant vs. nonparticipant	Participation dummy	Multivariate regression; investigated but did not implement correction for selection bias
Luman et al. (2003)	Immunization status	2000-01 NIS	WIC and non-WIC children ages 19-35 months (n=21,212)	Participant vs. nonparticipant	Participation dummy, with non-WIC children divided by income eligibility and prior WIC participation: Ineligible, eligible and participated in the past, and eligible but never participated	Multivariate regression
Shefer et al. (2001)	Immunization status	1999 NIS	WIC and non-WIC children ages 24-35 months (n=15,500)	Participant vs. nonparticipant	Participation dummy, with non-WIC children divided by income and prior WIC participation: previously on WIC, never on WIC and income-eligible, and never on WIC and not income-eligible	Bivariate t-tests[4]
Carlson and Senauer (2003)	Physician-reported general health status	1988-94 NHANES-III	Children ages 24-60 months (1) WIC sample: WIC and income-eligible (2) Full sample: WIC and non-WIC	Participant vs. nonparticipant	Participation dummy	Ordered probit equations

See notes at end of table.

Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Kranz and Siega-Riz (2002)	Added sugar intake	1994-96 CSFII	WIC and income-eligible children ages 2-5 (n=5,652)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Variyam (2002)	Dietary intake	1994-96 and 1998 CSFII	WIC and income-eligible children ages 1-4 (n=2,509)	Participant vs. nonparticipant	Participation dummy	Multivariate regression; quantile regressions
Burstein et al. (2000)	Dietary intake, height, weight, nutritional biochemistries, immunization status, general health status, dental health, use of preventive health care, and physical, emotional, and cognitive development	1988-94 NHANES-III 1993-95 SIPP 1995-97 CCDP	WIC and income-eligible children NHANES-III = 2,979 (12-59 months) SIPP = 1,302 (1-4 years) CCDP = 2,067 (2 years)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Kowaleski-Jones and Duncan (2000)	Motor skills, social skills, and temperament	NLSY, 1990-96 waves	(1) WIC and non-WIC infants and children (n=1,984)[5] (2) WIC and non-WIC infants and children with at least 1 other sibling born during the same period (n=453 sibling pairs)[5]	Participant vs. nonparticipant	Participation dummy	(1) Multivariate regression (2) Fixed-effects model

See notes at end of table.

Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Oliveira and Gundersen (2000)	Dietary intake	1994-96 CSFII	WIC and income-eligible children ages 1-4 in households where at least 1 other person also participated in WIC (n=180)	Participant vs. nonparticipant	Participation dummy	Multivariate regression[6]
Kramer-LeBlanc et al. (1999)	Dietary intake	1988-94 NHANES-III	WIC and income-eligible infants and children ages 2 months-4 years (n=6,636)	Participant vs. nonparticipant	Participation dummy	Bivariate t-tests
Rose et al. (1998)	Dietary intake	1989-91 CSFII	WIC and non-WIC children ages 1-4 who were not breastfeeding and resided in FSP-eligible households (n=499)	Participant vs. nonparticipant	Dose response: Value of monthly household per capita WIC benefit	Multivariate regression; investigated but did not implement adjustment for selection bias
Centers for Disease Control (1995)	Dietary intake, height, and weight	1988-91 NHANES-III	WIC and income-eligible infants and children ages 2-59 months (n=3,488)	Participant vs. nonparticipant	Participation dummy	Multivariate regression (height and weight) Comparison of means (dietary intake)
Rose et al. (1995)	Iron intake	1989-91 CSFII	WIC and non-WIC children ages 1-4 who were not breastfeeding (n=800)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Fraker et al. (1990)	Dietary intake	1985 CSFII	WIC and income-eligible children ages 1-4 (n=445)	Participant vs. nonparticipant	Dose response: Proportion of 4 recall days on which child was enrolled in WIC; also tested for combined WIC and FSP participation	Multivariate regression with selection-bias adjustment

See notes at end of table.

Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Group III: Secondary analysis of State-level files						
Lee et al. (2004a)	Number of dental visits per year and use of dental services (preventive, restorative, and emergency)	Longitudinal linked data base, including birth, Medicaid, WIC, and Area Resource files for children born in North Carolina in 1992 (1993-97)	WIC and non-WIC Medicaid recipients ages 1-4 (n=49,795)	Participant vs. nonparticipant	Dose-response: Number of months any WIC vouchers redeemed	Multivariate regression and ordered probit analysis, including 2-stage modeling to control for selection bias
Lee et al. (2004b)	Dental-care-related Medicaid costs	Longitudinal linked data base, including birth record, Medicaid, WIC, and Area Resource files for children born in North Carolina in 1992 (1992-96)	WIC and non-WIC Medicaid recipients ages 0-3 (n=49,795)	Participant vs. nonparticipant	Participation dummy (any participation per year)	Multivariate regression
Buescher et al. (2003)	Health care utilization and costs	Longitudinal linked data base, including birth, Medicaid, and WIC records for children born in North Carolina in 1992. Data base includes data through the 5th birthday (1992-97)	WIC and non-WIC Medicaid recipients ages 12-59 months (n=16,335-21,277 for 4 age-specific cohorts)	Participant vs. nonparticipant	Dose response: Cumulative WIC participation defined as none, high, medium, and low[7]	Multivariate regression; investigated but did not implement selection-bias-adjustment models
Lee et al. (2000)	Prevalence of anemia, failure to thrive, nutritional deficiencies, and use of preventive health care services	Longitudinal linked data base, including birth record, Medicaid, AFDC/TANF, FSP, and WIC files for all children born in Illinois from 1990 through 1996	WIC and non-WIC infants and children ages 0-59 months who received Medicaid benefits continuously	Participant vs. nonparticipant	Participation dummy	Multivariate regression and proportional hazards models[8]
Partington and Nitzke (1999)	Dietary intake	CSFII data for Midwest region (1994)[9]	WIC and income-eligible children ages 2-5 (n=183)	Participant vs. nonparticipant	Participation dummy	Bivariate z-tests

See notes at end of table. Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Sherry et al. (2001)	Prevalence of anemia	PedNSS data for Colorado, New Mexico, Oklahoma, Utah, and Vermont (early 1980s-mid-1990s) (most data provided by WIC programs)	Infants and children ages 6-59 months (5,500-48,000 records per State per year)	Prevalence estimates for each State in 5-year intervals overall and by age, race/ ethnicity, gender, birthweight, and type of screening visit	N/A	Trends analysis
Sherry et al. (1997)	Prevalence of anemia	PedNSS data for Vermont (1981-94) (most data provided by WIC programs)	Infants and children ages 6-59 months (n=12,000-19,500 records per year)	Prevalence estimates for each year for overall sample by age	N/A	Trends analysis
Yip et al. (1987)	Prevalence of anemia	(1) PedNSS data for Arizona, Kentucky, Louisiana, Montana, Oregon, and Tennessee (1975-85) (Most data provided by WIC programs) (2) Linked PedNSS and birth records for WIC participants in Tennessee PedNSS database (1975-84)	Infants and children ages 6-60 months (1) (n=499,759) (2) (n=72,983)	(1) Overall and age-specific prevalence estimates for each year: Initial measures vs. followup measures (2) Participant vs. nonparticipant	Participation dummy	(1) Linear regression; angular chi-square (2) Multivariate regression
USDA/FNS (1978)	Hemoglobin, hematocrit, height, and weight	WIC records in PedNSS data for Arizona, Kentucky, Tennessee, and Washington (1974-76)	WIC infants and children ages 0-59 months with 3 or more WIC visits at approximately 6-month intervals (n=5,692)[10]	Participants, before vs. after	Participation dummy	Chi-square tests

See notes at end of table. Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Group IV: Other State and local studies						
Black et al. (2004)	Height, weight, caregiver-perceived health status, and household food security	Primary data collection at urban medical centers in Washington, DC, Baltimore, Minneapolis, Boston, Little Rock, and Los Angeles (1998-2001)	WIC and income-eligible infants younger than 12 months (n=5,923)[11]	Participant vs. nonparticipant	Participation dummy, with non-WIC subjects divided into those who did not participate because of access issues and those who did not perceive a need for WIC	Multivariate regression
Kahn et al. (2002)	Prevalence of anemia	Medical records for 3 WIC sites in Chicago (1997-99)	WIC infants and children ages 6-59 months (n=7,053)	Participants, before vs. after	Participation dummy	Not well described
Shaheen et al. (2000)	Immunization status	Primary data collection (interviews and record abstractions) in a predominantly Hispanic low-income area of Los Angeles (dates not reported)	WIC and non-WIC children ages 2-4 (n=270)	Participant vs. nonparticipant	Participation dummy	Age-adjusted odds ratios
James (1998)	Immunization status	Medical records for 1 health care center in Mt. Vernon, NY	Randomly selected sample (matched on age and gender) of children who were up-to-date on immunizations at 12 months of age; equal size groups (n=150)	Participant vs. nonparticipant	Participation dummy	Chi-square tests

See notes at end of table.

Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Burstein et al. (1991)	Dietary intake, hemoglobin, hematocrit, height, weight, and head circumference	Primary data collection in Florida and North Carolina (1990-91)	Random sample of WIC and income-eligible infants (6 months old) stratified by birthweight (n=807)	Participant vs. nonparticipant	Participation dummy	Multivariate regression, including attempt to control for selection bias
Brown and Tieman (1986)	Dietary intake, hemoglobin, hematocrit, height, and weight	Primary data collection in low-income areas of 1 county in Minnesota (dates not reported)	WIC and income-eligible children ages 1-5 (n=52)	Participant vs. nonparticipant	Participation dummy	Chi-square test
Smith et al. (1986)	Hemoglobin	Medical records for 1 health center in Los Angeles; initial and 6-month followup measures	Subset of random sample of WIC and non-WIC children ages 1-4 who were diagnosed with anemia; matched on age, gender, and ethnicity (n=25 each group)	Participants vs. nonparticipants, before and after	Participation dummy	Analysis of variance
Miller et al. (1985)	Serum ferritin, hematocrit, and hemoglobin	Medical records for 1 child and youth clinic in Minneapolis (1973-74 and 1977)	WIC and income-eligible children ages 16-23 months (n~2,225)	Participants, before vs. after, separate groups	Participation dummy	Chi-square tests

See notes at end of table.

Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Vazquez-Seone et al. (1985)	Hemoglobin	Medical records for children enrolled in an inner-city health center in New Haven, CT, before and after initiation of WIC	WIC and income-eligible infants and children ages 9-36 months (n=583)	Participants, before vs. after, separate groups	Participation dummy	Bivariate t-tests
Hicks and Langham (1985)	IQ scores and school grades	Primary data collection and record abstractions in 3 counties in rural Louisiana (dates not reported)	Sibling WIC pairs ages 8-10; 1 “participated” in WIC prenatally and 1 enrolled after age 1 (n=19 sibling pairs)	Participant vs. sibling control	Participation dummy	Multivariate regression
Heimendinger et al. (1984)	Expected weight gain[12]	Medical records in 3 WIC and 4 non-WIC clinics in the same Boston neighborhoods (1974-79)	WIC- and Medicaid-eligible infants and toddlers up to 20 months with at least 2 height and weight measurements (n=1,907)	Participant vs. nonparticipant, (“value added”or expected growth vs. actual growth)	Participation dummy	Multivariate regression of “value-added” measures by age group (3-month intervals)
Paige (1983)	Medicaid costs and health care utilization	Medicaid records in 4 counties in Maryland, 2 in which WIC was available and 2 in which WIC was not available (1979-80)	WIC and income-eligible infants ages 0-11 months who were on Medicaid for at least 75% of study period (n=138)	Participant vs. nonparticipant	Participation dummy	Comparison of means and proportions (no statistical tests reported)
Hicks et al. (1982)	Hemoglobin, height, weight, and a variety of intellectual and behavioral measures	Primary data collection and record abstractions in 3 rural counties in Louisiana (dates not reported)	Sibling WIC pairs ages 6-8; 1 “participated” in WIC prenatally and 1 enrolled after age 1 (n=21 sibling pairs)	Participant vs. sibling control	Participation dummy	Multivariate regression

See notes at end of table.

Continued—

Table 23—Studies that examined the impact of the WIC program on nutrition and health outcomes of infants and children—Continued

Study	Outcome(s)	Data source[1]	Population (sample size)[2]	Design	Measure of participation	Analysis method
Weiler et al. (1979)	Hemoglobin	WIC records in 1 clinic in Fayette Co, KY (1976-77)	Infants ages 0-6 months initially certified for WIC because of anemia who had followup hemoglobin measure available (n=37)	Participants, before vs. after	Participation dummy	Bivariate t-tests

Note: N/A = Not applicable.

[1] Data sources:

CCDP = Comprehensive Child Development Programs.
CSFII = Continuing Survey of Food Intakes by Individuals.
FITS = Feeding Infants and Toddlers Study.
NHANES-III = Third National Health and Nutrition Examination Survey.
NIS = National Immunization Survey.
NLSY = National Longitudinal Survey of Youth.
PedNSS = Pediatric Nutrition Surveillance System.
SIPP = Survey of Income and Program Participation.

[2] Unless the description of the study sample indicates that a comparison group was limited to nonparticipants who were income-eligible for WIC or known to be Medicaid participants, all income levels were included in the comparison group. Income was generally controlled for in the analysis.

[3] Definition of comparison group varies for different outcomes. Children who never participated in WIC were main comparison group and were compared with former and/or current WIC participants.

[4] Also estimated a multivariate model of the relationship between intensity of WIC immunization activities and immunization coverage rates for WIC participants.

[5] Roughly half of the sample was assessed in the first year of life and half was assessed between their first and second birthdays.

[6] Authors also ran regression for full sample of WIC and income-eligible children. That model resulted in more significant effects.

[7] WIC participation defined based on percentage of months from age 1 through current age in which WIC vouchers had been redeemed. High = more than 66 percent, Medium = 34-66 percent, and Low = 33 percent or less.

[8] To control for the fact that several outcomes under study might be reasons for WIC enrollment, WIC participation was coded as zero if diagnosis of a particular problem preceded the date of WIC enrollment.

[9] CSFII data included two recalls per subject, but authors used only the first recall. Used only data for 1994 because, at the time the study was conducted, only that portion of the 1994-96 data set had been coded for food group consumption.

[10] Maximum sample; sample size varies for each outcome.

[11] Information on income was not collected. Receipt of private health insurance was used as a proxy for income, and the non-WIC sample was limited to infants without private insurance.

[12] A doctoral dissertation completed by Heimendinger in 1981 included data on height and weight-for-height. However, these data were dropped from the peer-reviewed journal article because of substantial problems with missing data.

data from national surveys. Group III includes nine studies that relied on State-level databases or, in one case, a regional database. These include WIC/Medicaid databases similar to those used in the previously summarized research on birth outcomes, State-level files from the Pediatric Nutrition Surveillance System (PedNSS),[66] and regional data from the Continuing Survey of Food Intakes by Individuals. Fourteen of the identified studies are other types of State and local studies (Group IV).

WIC research on infants and children is notably more recent than the previously summarized research on birth outcomes, breastfeeding, and the nutrition and health characteristics of pregnant women. Indeed, as shown in table 23, there have been several very recent contributions to this literature. Of the 41 identified studies, 10 are based on data collected primarily or exclusively in the early to mid-1990s, 10 are based on data collected in the mid- to late 1990s, and 3 used data that were collected exclusively in 2000 or later or had data collection periods that started late in the 1990s and extended beyond 2000. The relative recency of these studies is particularly important because of the increase in child participation experienced during the early 1990s. Studies based on data collected after this time are more likely to be generalizable to the current population of WIC children and are less subject to bias associated with restricted program access.

Although some studies included both infants (under 12 months of age) and children (1-4 years), the available research is heavily slanted toward children. Children were included in all but 4 of the identified studies, and 22 of the identified studies focused exclusively on children. Given that children make up 50 percent of the WIC population overall, this emphasis is not inappropriate (Bartlett et al., 2003).

A number of different outcomes have been examined, with the most common being dietary intake (17 studies), growth (12 studies), and anemia/iron status (16 studies). In addition, four studies looked at general health status, as perceived by caregivers or assessed by physicians, six studies focused on immunization status, and eight studies examined health or dental care use and/or costs. Finally, five studies looked at developmental outcomes, and two studies assessed impacts on household food sufficiency/security. Findings for each of these outcomes are summarized, in turn, in the sections that follow.

[66]PedNSS is a program-based nutrition monitoring system coordinated by the CDC. It includes State-reported data from programs that serve low-income infants and children. A majority of PedNSS data comes from WIC programs.

Research Results

Dietary Intake: Children

Table 24 summarizes findings from the 16 studies that examined the impact of WIC participation on the dietary intakes of children.[67] The table is divided into six sections: food energy and macronutrients, vitamins, minerals, other food components, food group servings, and summary measures of dietary quality. The text follows this general organization but discusses findings for vitamins and minerals and for food group intakes and summary measures in combined sections.

Most of the studies completed to date are subject to the methodological limitations that affect most existing FANP research on dietary intakes, as discussed in chapter 2 and in the preceding section on impacts among pregnant women. However, two very recent studies (Cole and Fox, 2004; Ponza et al., 2004) avoided these limitations. Both used the approach recommended by the IOM (2001) to estimate usual intakes of WIC participants and nonparticipants and, for nutrients with established EARs, used the recommended EAR-cutpoint method to estimate the percentage of children whose usual intakes were *adequate*. Cole and Fox (2004) explored the nutrient intakes of children ages 1-4, using data from NHANES-III.[68] Ponza and his colleagues used data from the Feeding and Infants and Toddlers Study (FITS), which included a national sample of infants and toddlers ages 4-24 months. Compared with national distributions, the FITS sample had slightly higher incomes, a smaller percentage of Hispanics, and a lower rate of WIC participation (Devaney et al., 2004; Ponza et al., 2004). Although neither of these studies was intended to provide valid estimates of the impact of WIC on dietary intakes—Cole and Fox (2004) used bivariate t-tests to assess differences between groups, and Ponza et al. (2004) did not test for statistical significance—findings are useful in providing an up-to-date perspective on the relative likelihood of adequate nutrient intakes among WIC participants and nonparticipants.

In reviewing findings, greatest weight is given to the study by Oliveira and Gundersen (2000), who analyzed data from the 1994-96 CSFII and employed a unique strategy to control for selection bias. To get around the fact that the CSFII does not include variables that provide suitable controls for selection bias,

[67]A total of 17 studies examined dietary intakes, but the study by Burstein et al. (1991) was limited to infants.

[68]Because NHANES-III collected a second day of dietary recall data for only 5 percent of respondents, the authors obtained the estimates of intraindividual variation needed to estimate usual intakes from the 1994-96 CSFII data.

Table 24—Findings from studies that examined the impact of the WIC program on the dietary intakes of children and/or infants

Outcome	Significant impact: Participants consumed more	No significant impact: Participants consumed more/same	No significant impact: Participants consumed less	Significant impact: Participants consumed less
Food energy and macronutrients				
Food energy[1]	Cole (2004) [national] - C {2 years}	Cole (2004) [national] - C {3, 4 years} Ponza (2004) [national] - I+C Siega-Riz (2004) [national] - C Burstein (2000) [national] - C Kramer-LeBlanc (1999) [national] - I {2-3 months} Partington (1999) [1 region] - C Rose (1998) [national] - C Burstein (1991) [2 States] - I Fraker (1990) [national] - C Rush (1988c) [national] - I+C	Cole (2004) [national] - C {1 year} Oliveira (2000) [national] - C Kramer-LeBlanc (1999) [national] - I {4-11 months} + C {1-4 years} CDC (1995) [national] - B Brown (1986) [1 county] - C	
Protein	Rose (1998) [national] - C Edozien (1979) [national] - C Burstein (1991) [2 States] - I	Ponza (2004) [national] - I+C Burstein (2000) [national] - C Oliveira (2000) [national] - C Kramer-LeBlanc (1999) [national] - I+C Fraker (1990) [national] - C	Rush (1988c) [national] - C	Rush (1988c) [national] - I Edozien (1979) [national] - I
Fat	Kramer-LeBlanc (1999) [national] - I {4-11 months} Partington (1999) [1 region] - C	Siega-Riz (2004) [national] - C {130-185% poverty} Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-3 years} Rose (1998) [national] - C CDC (1995) [national] - B {Whites} Rush (1988c) [national] - I	Cole (2004) [national] - C Burstein (2000) [national] - C Kramer-LeBlanc (1999) [national] - C {4 years} CDC (1995) [national] - B {Blacks, Mexican-Americans}	Siega-Riz (2004) [national] - C {<130% poverty}
Saturated fat		Cole (2004) [national] - C Siega-Riz (2004) [national] - C {130-185% poverty} Kramer-LeBlanc (1999) [national] - I+C Rose (1998) [national] - C	Siega-Riz (2004) [national] - C {<130% poverty} Burstein (2000) [national] - C	

See notes at end of table.

Continued—

Table 24—Findings from studies that examined the impact of the WIC program on the dietary intakes of children and/or infants—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Carbohydrates	Siega-Riz (2004) [national] - C {<130% poverty}	Kramer-LeBlanc (1999) [national] - C {4 years} Rush (1988c) [national] - C	Siega-Riz (2004) [national] - C {130-185% poverty} Kramer-LeBlanc (1999) [national] - I {2-11 months} + C {1-3 years} Rush (1988c) [national] - I	
Vitamins				
Vitamin A	Edozien (1979) [national] - I+C	Ponza (2004) [national] - I Burstein (2000) [national] - C Oliveira (2000) [national] -C Kramer-LeBlanc (1999) [national] - I {2-11 months} + C {1-3 years} Rose (1998) [national] - C Burstein (1991) [2 States] - I Rush (1988c) [national] - C	Ponza (2004) [national] - C Kramer-LeBlanc (1999) [national] - C {4 years} Fraker (1990) [national] - C Rush (1988c) [national] - I Brown (1986) [1 county] - C	
Vitamin B_6	Oliveira (2000) [national] - C Rose (1998) [national] - C Fraker (1990) [national] -C Rush (1988c) [national] - C	Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-4 years} Rush (1988c) [national] - I	Kramer-LeBlanc (1999) [national] - I {4-11 months}	
Vitamin B_{12}		Kramer-LeBlanc (1999) [national] - C Rush (1988c) [national] - I+C	Kramer-LeBlanc (1999) [national] - I {2-3 months}	Kramer-LeBlanc (1999) [national] - I {4-11 months}
Vitamin C	Kramer-LeBlanc (1999) [national] - I {4-11 months} + C {1-3 years} Rush (1988c) [national] - I+C Edozien (1979) [national] - I+C	Cole (2004) [national] - C Ponza (2004) [national] - I+C Burstein (2000) [national] - C Oliveira (2000) [national] - C Kramer-LeBlanc (1999) [national] - I {2-3 months} Rose (1998) [national] - C Burstein (1991) [2 States] - I Fraker (1990) [national] - C Brown (1986) [1 county] - C	Kramer-LeBlanc (1999) [national] - C {4 years}	

See notes at end of table.

Continued—

Table 24—Findings from studies that examined the impact of the WIC program on the dietary intakes of children and/or infants—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamin E	Rose (1998) [national] - C	Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-3 years} Fraker (1990) [national] - C	Kramer-LeBlanc (1999) [national] - C {4 years}	Kramer-LeBlanc (1990 [national] - I {4-11 months}
Folate	Burstein (2000) [national] - C Oliveira (2000) [national] - C Rose (1998) [national] - C Edozien (1979) [national] - I+C	Kramer-LeBlanc (1999) [national] - I {2-11 months} + C {1-3 years}	Kramer-LeBlanc (1999) [national] - C {4 years}	
Niacin	Kramer-LeBlanc (1999) [national] - I {4-11 months} Rose (1998) [national] - C Rush (1988c) [national] - C Edozien (1979) [national] - I+C	Kramer-LeBlanc (1999) [national] - I {2-3 months} Rush (1988c) [national] - I	Kramer-LeBlanc (1999) [national] - C	
Riboflavin	Rose (1998) [national] - C Edozien (1979) [national] - C	Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-4 years} Rush (1988c) [national] - C	Rush (1988c) [national] - I	Kramer-LeBlanc (1999) [national] - I {4-11 months} Edozien (1979) [national] - I
Thiamin	Kramer-LeBlanc (1999) [national] - I {2-3 months} Rose (1998) [national] - C Rush (1988c) [national] - C Edozien (1979) [national] - I+C	Kramer-LeBlanc (1999) [national] - I {4-11 months} + C {1-4 years} Rush (1988c) [national] - I		
Minerals				
Calcium	Cole (2004) [national] - C {2 years} Variyam (2002) [national] - C[2] Burstein (2000) [national] - C Edozien (1979) [national] - C	Cole (2004) [national] - C {1, 3, 4 years} Ponza (2004) [national] - I+C Siega-Riz (2004) [national] - C Oliveira (2000) [national] - C Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-4 years} Rose (1998) [national] - C Burstein (1991) [2 States] – I[3] Fraker (1990) [national] - C CDC (1995) [national] - B {Blacks}[4] Rush (1988c) [national] - C	CDC (1995) [national] - B {Whites, Mexican-Americans} Brown (1986) [1 county] - C	Kramer-LeBlanc (1999) [national] - I {4-11 months} Rush (1988c) [national] - I Edozien (1979) [national] - I

See notes at end of table.

Continued—

Table 24—Findings from studies that examined the impact of the WIC program on the dietary intakes of children and/or infants—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Iron	Variyam (2002) [national] - C Siega-Riz (2004) [national] - C Oliveira (2000) [national] - C Kramer-LeBlanc (1999) [national] - I {4-11 months} + C {1-3 years} Rose (1998) [national] - C Rose (1995) [national] - C Burstein (1991) [2 States] - I Rush (1988c) [national] - I+C Brown (1986) [1 county] - C Edozien (1979) [national] - I+C	Cole (2004) [national] - C Ponza (2004) [national] - I+C Burstein (2000) [national] - C Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {4 years} Fraker (1990) [national] - C		
Magnesium	Rose (1998) [national] - C	Kramer-LeBlanc (1999) [national] - C {1-3 years} Rush (1988c) [national] - C	Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {4 years}	Rush (1988c) [national] - I Kramer-LeBlanc (1999) [national] - I {4-11 months}
Phosphorus	Edozien (1979) [national] - C	Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-3 years} Rose (1998) [national] - C	Kramer-LeBlanc (1999) [national] - C {4 years}	Kramer-LeBlanc (1999) [national] - I {4-11 months} Rush (1988c) [national] - I+C Edozien (1979) [national] - I
Zinc	Variyam (2002) [national] - C[5] Rose (1998) [national] - C	Cole (2004) [national] - C Fraker (1990) [national] - C	Oliveira (2000) [national] - C Kramer-LeBlanc (1999) [national] - I+C	
Other dietary components				
Cholesterol	Cole (2004) [national] - C {2 years}	Cole (2004) [national] - C {3, 4 years} Burstein (2000) [national] - C Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-3 years}	Kramer-LeBlanc (1999) [national] - C {4 years} Rose (1998) [national] - C	Kramer-LeBlanc (1999) [national] - I {4-11 months}
Fiber	Cole (2004) [national] - C {2 years}	Cole (2004) [national] - C {3 years} Siega-Riz (2004) [national] - C {<130% poverty}	Siega-Riz (2004) [national] - C {130-185% poverty} Kramer-LeBlanc (1999) [national] - I+C	Cole (2004) [national] - C {4 years}

See notes at end of table.

Continued—

Table 24—Findings from studies that examined the impact of the WIC program on the dietary intakes of children and/or infants—Continued

Outcome	Significant impact: Participants consumed more	No significant impact: Participants consumed more/same	No significant impact: Participants consumed less	Significant impact: Participants consumed less
Sodium	Cole (2004) [national] - C {2 years}	Cole (2004) [national] - C {3 years} Burstein (2000) [national] - C	Cole (2004) [national] - C {4 years} Kramer-LeBlanc (1999) [national] - I {2-3 months} + C {1-4 years}	Kramer-LeBlanc (1999) [national] - I {4-11 months}
Added sugar		Kramer-LeBlanc (1999) [national] - C {4 years}	Kramer-LeBlanc (1999) [national] - I { 2-11 months} + C {1-3 years} Partington (1999) [1 region] - C	Siega-Riz (2004) [national] - C Kranz (2002) [national] - C
Food group servings				
Dairy	Burstein (2000) [10 sites] - C Partington (1999) [1 region] - C	Cole (2004) [national] - C		
Fruit	Cole (2004) [national] - C {3 years}	Cole (2004) [national] - C {2, 4 years} Siega-Riz (2004) [national] - C	Burstein (2000) [10 sites] - C Partington (1999) [1 region] - C	
Fruit juice	Burstein (2000) [10 sites] - C			
Fruit and vegetables	Siega-Riz (2004) [national] - C		Burstein (2000) [10 sites] - C	
Grains		Cole (2004) [national] - C Partington (1999) [1 region] - C		
Meat/bean		Cole (2004) [national] - C	Partington (1999) [1 region] - C	
Vegetables	Partington (1999) [1 region] - C {“other”vegs}[6]	Burstein (2000) [10 sites] - C	Cole (2004) [national] - C Partington (1999) [1 region] - C {total vegs}	

See notes at end of table.

Continued—

Table 24—Findings from studies that examined the impact of the WIC program on the dietary intakes of children and/or infants—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants scored higher	Participants scored higher/same	Participants scored lower	Participants scored lower
Summary measures				
Total HEI score		Cole (2004) [national] - C		

Notes: Cell entries show the senior author's name, the publication date, the scope of the study (for example, national vs. 1 city or 1 State), and the participant group(s) involved: (B = both infants and children, C = children, and I = infants). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Data for Cole (2004) and Ponza (2004) reflect *usual* intakes, estimated using methods recommended by the IOM (2001). For nutrients with established EARs (vitamins A and C, iron, and protein (Ponza, 2004, only)), both studies estimated the *prevalence of adequate usual intakes* using the methods recommended by the IOM (2001). For these nutrients, findings are reported relative to the prevalence of adequate usual intakes. Thus, "participants consumed more" indicates that, relative to nonparticipants, participants had a *greater prevalence of adequate usual intakes* and "participants consumed less" means that participants had a *lower prevalence of adequate usual intakes*.

Ponza (2004) did not test the statistical significance of differences between groups.

Findings reported for Siega-Riz (2004) are for total intakes. A separate snacks-only analysis revealed significant differences for the <130% of poverty group for added sugars (participants consumed less) and iron (participants consumed more). For the 130-185% of poverty group, only the difference in intake of added sugars was significant (participants consumed less).

Findings reported for Variyam (2002) are based on OLS regression model. Variations observed in the quantile regressions, if any, are described in footnotes.

Kramer-LeBlanc (1999) also reported significant differences between WIC participant and nonparticipant infants ages 4-11 months for vitamin D, total carotenes, copper, selenium, and potassium. For the first two, intakes were greater among WIC participants. For the latter three, the observed effect was in the opposite direction.

Findings reported for Burstein et al. (1991) are based on selection-bias-adjusted model. The authors cautioned that both the single-equation model and instrumental variables model produced implausible results.

Findings reported for Rush (1988c) are based on comparison of current WIC participants with children who never participated in WIC and reflect results for analyses that compared 24-hour intakes.

[1] Edozien (1979) reported no significant between-group differences in energy intakes of either infants or children, but did not report point estimates.

[2] In quantile analysis, difference was statistically significant at all quantiles except 0.9.

[3] Based on main analysis, which assessed the percentage of infants with intakes below 77 percent of the RDA. Supplementary analysis that examined mean intakes found that WIC infants consumed less calcium than non-WIC infants.

[4] Reported finding is for calcium per 1,000 kilocalories. For total calcium in gm, mean was lower for WIC participants.

[5] In quantile analysis, difference was statistically significant at the 0.75 quantile only.

[6] Excludes deep-yellow and dark-green vegetables, as well as legumes, white potatoes, and other starchy vegetables.

as reported by previous researchers (Fraker, 1990), Oliveira and Gundersen limited their analysis sample to WIC participants and low-income nonparticipants who lived in households where at least one other member was on the WIC program. The rationale for this restriction was that it effectively controlled for key sources of selection bias, including lack of awareness of the WIC program and resistance to participation because of stigma or other reasons. The authors acknowledge that two important sources of potential bias remain, both of which are associated with rationing rather than self-selection. The income-eligible nonparticipant group may have included (1) children who were not actually eligible for WIC because they did not have a certified nutritional risk and (2) children who were fully eligible but could not participate because the local WIC program had no available slots. Both of these sources of bias would tend to underestimate program impacts.

A downside to the approach used by Oliveira and Gundersen is that it severely limited sample sizes. The final analysis sample included only 180 children, whereas the full 1994-96 CSFII database included 1,206 children who were either enrolled in WIC or were income-eligible. The small sample size means that the analysis was likely able to detect only large differences. This limitation, in combination with the remaining sources of selection bias, means that the study provides a fairly conservative estimate of WIC's effects.

Another limitation to the Oliveira and Gundersen study is that it examined a relatively limited set of nutrients. For most nutrients not examined by Oliveira and Gundersen, the strongest available evidence comes from a study completed by Rose, Habicht, and Devaney (1998), who used data from the 1989-91 round of the CSFII. This study may overstate WIC impacts, however, because the authors did not control for selection bias (they report that they "found no evidence of it") and limited their sample to children in FSP-eligible households (household incomes below 130 percent of poverty). This sample represents the lower end of the income distribution of WIC participants and children in this group may benefit from WIC's supplemental food benefit more than higher income children would.

Food Energy and Macronutrients. The evidence suggests that WIC participation does not affect children's intakes of food energy. Although WIC participants tend to have greater energy intakes than nonparticipants, these differences tend not to be statistically significant. Only Cole and Fox (2004) reported a significant difference (based on bivariate t-tests), and it was limited to 2-year-olds.

Results for protein are equivocal. Oliveira and Gundersen (2000) found no significant difference in intakes of participants and nonparticipants. However, the earlier study by Rose, Habicht, and Devaney (1998) found that WIC participants consumed significantly more protein than nonparticipants. It is possible that the effect on protein intake may be small (and therefore not detected by Oliveira and Gundersen) and limited to the lowest income participants.

Relatively few studies have examined intakes of fat, saturated fat, and carbohydrates, and Oliveira and Gundersen (2000) is not among them. Consequently, the best available data in this area comes from a study by Siega-Riz et al. (2004). Siega-Riz and her colleagues used the 1994-96, 1998 CSFII dataset to assess intakes of children who were not enrolled in school and who had household incomes below 185 percent of poverty. The authors did not attempt to control for selection bias, acknowledging the limitations of the CSFII data. To provide a somewhat greater level of control for unmeasured differences between groups, they completed separate analyses for children with incomes below 130 percent of poverty and children with incomes between 130 percent and 185 percent of poverty. They also included a sizable number of covariates in their models, including variables that controlled for mother's age, television viewing, use of dietary supplements, presence of dietary restrictions, and enrollment in child care.

The analysis revealed no significant differences in intakes of fat, saturated fat, or carbohydrates (expressed as a percentage of total energy intake) between WIC participants and nonparticipants with household incomes between 130 and 185 percent of poverty. However, among lower income children—those residing in households with incomes below 130 percent of poverty—WIC participants consumed significantly less fat and significantly more carbohydrates than nonparticipants. These suggestive findings would be more convincing if they were replicated in the restricted sample analyzed by Oliveira and Gundersen.

Vitamins and Minerals. Giving precedence to Oliveira and Gundersen, there is strong evidence that WIC participation increases children's intakes of vitamin B_6, folate, and iron. The evidence that WIC increases children's iron intake is particularly strong. Almost all of the identified studies assessed iron intake, and all but one of the studies that used multivariate analysis techniques found significantly greater intakes among WIC children. Consistent results for Oliveira and Gundersen (2000) and Rose, Devaney, and Habicht (1998) also strongly suggest that WIC participation does not significantly affect children's intakes of vitamin A, vitamin C, or calcium.

For other vitamins and minerals, evidence of a significant WIC impact is less clear. Rose, Habicht, and Devaney (1998) reported a significant impact on children's intake of zinc, while Oliveira and Gundersen found no such effect. Rose, Habicht, and Devaney also reported significant impacts for several nutrients that were not included in the Oliveira and Gundersen study, including vitamin E, niacin, riboflavin, thiamin, and magnesium. In all cases, mean intakes were greater for WIC participants than for nonparticipants. These findings suggest a WIC impact among the lowest income children but would be more convincing if they were replicated in the restricted sample used by Oliveira and Gundersen.

As noted previously, increased nutrient intake by participants does not necessarily mean that participants are more likely than nonparticipants to have adequate diets. Recent data on usual nutrient intakes of age-eligible children indicate that the vast majority of both WIC and non-WIC children have nutritionally adequate diets. Cole and Fox (2004) found that virtually all children ages 1-4, regardless of WIC participation status, had adequate usual intakes of iron and zinc. Ponza et al. (2004) reported similar findings for iron for children ages 1 and 2.[69]

Neither Cole and Fox nor Ponza et al. assessed intakes of vitamin B_6 or folate (the other two nutrients found to be significant in Oliveira's and Gundersen's analysis). However, findings from the main FITS analysis, which did not differentiate children by WIC participation status, showed that less than 1 percent of all 1- and 2-year-olds had inadequate usual intakes of vitamin B_6, and only 2 percent had inadequate usual intakes of folate (Devaney et al., 2004). The main FITS analysis also provides information on the other nutrients for which Rose, Habicht, and Devaney (1998) reported a significant WIC impact. FITS found that less than 1 percent of children ages 1 and 2 had inadequate usual intakes of riboflavin, thiamin, or magnesium, 3 percent had inadequate usual intakes of niacin, and 58 percent had inadequate usual intakes of vitamin E. The authors urged caution in interpreting the finding for vitamin E, given that clinical data from NHANES-III do not indicate problems with vitamin E status. They suggested that the high prevalence of apparently inadequate vitamin E intakes may be associated with the difficulty of assessing the types and amounts of fats and oils used in cooking and/or with variability in food composition databases.

Data from Cole and Fox (2004), Devaney et al. (2004), and Ponza et al. (2004) suggest that the prevalence of inadequate nutrient intakes among very young children is low, and that today's WIC children are doing as well nutritionally as their nonparticipating counterparts. However, the fact that the descriptive analyses completed by Cole and Fox (2004a) and Ponza et al. (2004) did not reveal meaningful differences in the prevalence of nutrient inadequacy among WIC and non WIC children does not necessarily mean that the WIC program has no impact on children's diets. It may be, for example, that WIC is responsible for bringing intakes of participating children up to the level of other children. The question of WIC impacts cannot be assessed even at a basic level without multivariate analysis techniques that, at a minimum, control for measured differences between the two groups.

Other Dietary Components. Information on the impact of WIC on other dietary components, including cholesterol, sodium, fiber, and added sugars, is very limited. The majority of studies that looked at these components were descriptive studies that assessed differences between groups with bivariate t-tests or did not assess statistical significance.

There is no convincing evidence that WIC participation influences children's intakes of cholesterol, sodium, or fiber. There is suggestive evidence, however, that WIC participation decreases children's consumption of added sugar. Using data from the most recent CSFII, Siega-Riz et al. (2004) and Kranz and Siega-Riz (2002) both found that WIC children consumed significantly less added sugar than non-WIC children. In the Siega-Riz et al. study, this difference was assessed based on the percentage of food energy provided by added sugar and was significant for two different income samples (<130 percent of poverty and 130-185 percent of poverty). In the Kranz and Siega-Riz study, the outcome measure was teaspoons of sugar per 100 kilocalories and the difference was also

[69]As discussed later in this chapter, the adequacy of children's iron intakes is consistent with declining levels of anemia in this population and may reflect an indirect effect of the WIC program on the availability and use of iron-fortified breakfast cereals.

observed for two different income groups (<130 percent of poverty and ≤185 percent of poverty). While suggestive of a positive WIC effect, the evidence would be more convincing if it were replicated in the restricted sample used by Oliveira and Gundersen (2000).

Food Group Intake and Summary Measures of Dietary Quality. Data on the impact of WIC participation on children's food intake or on overall dietary quality are also very limited. Most of the studies that looked at these outcomes used simple bivariate t-tests. And, as table 24 clearly illustrates, the overlap in significant findings across studies is small. The available data are too limited to support even tentative conclusions about WIC impacts in these areas.

Dietary Intake: Infants

Five of the identified studies reported separate estimates of WIC's impact on the dietary intake of WIC and non-WIC infants (table 23). This includes both national WIC evaluations (Rush et al., 1988c; Edozien et al., 1979), the field test of the WIC Child Impact Study (Burstein et al., 1991), and the more recent descriptive studies completed by Kramer LeBlanc et al. (1999) and Ponza et al. (2004). Cole and Fox (2004) looked at reported infant feeding patterns of WIC and non WIC infants but did not examine dietary intake per se.

Of the available studies, the strongest are the field test of the WIC Child Impact Study (Burstein et al., 1991) and the NWE (Rush et al., 1988c), although both have methodological limitations. As shown in table 24, both the NEW and the field test of the WIC Child Impact Study found that WIC infants had significantly higher intakes of iron than non-WIC infants. Ponza et al.'s (2004) recent assessment of usual nutrient intakes found that WIC infants ages 7-11 months had greater mean usual intakes of iron than did nonparticipant infants and, more importantly, that the prevalence of adequate usual iron intakes was greater for WIC infants than for non-WIC infants (99 percent vs. 90 percent). The statistical significance of these differences was not tested.

The NWE also found that WIC infants consumed significantly less calcium, magnesium, and phosphorus than non-WIC infants. Burstein and her colleagues (1991) reported no impact on calcium in their main analysis, which assessed the percentage of infants consuming less than 77 percent of the RDA; however, supplementary analyses that used mean intakes found, like Rush et al., that WIC infants consumed significantly less calcium than non-WIC infants.

For the NWE, Rush and his colleagues completed a detailed analysis of the sources of nutrients in infants' diets and found that the greater iron intakes and lower calcium, magnesium, and phosphorus intakes noted for WIC infants were related. All of these findings were associated with an increased use of cows' milk among non-WIC infants. Because the American Academy of Pediatrics recommends that cow's milk not be fed to infants younger than 12 months, the lower intakes of calcium, magnesium, and phosphorus among WIC infants were not interpreted as negative impacts. Burstein and her colleagues found a similar pattern. Specifically, they found that, among nonbreastfed infants, WIC infants were more likely to receive formula and non-WIC infants were more likely to receive cow's milk. Moreover, among formula-fed infants, WIC infants were more likely to receive iron-fortified formula and non-WIC infants were more likely to receive formula that was not fortified with iron.

Recent descriptive studies provide some evidence that differences between WIC infants and non-WIC infants in the use of cow's milk may persist today. For example, Kramer-LeBlanc and her colleagues (1999) found that, among infants ages 4-11 months, WIC participants consumed significantly less protein, calcium, magnesium, riboflavin, vitamin B_{12}, and sodium. All of these nutrients occur in greater concentrations in cow's milk than in iron-fortified infant formula. In addition, Cole and Fox (2004) analyzed the infant feeding inventory in NHANES-III and found that WIC participants were significantly less likely than nonparticipants to be fed cow's milk before 12 months of age.

In an analysis of 24-hour intakes, Ponza et al. (2004) found no significant difference between WIC infants and non-WIC infants in the percentage consuming cow's milk. In addition, findings from an inventory of feeding practices that assessed whether an infant had ever been fed cow's milk found no difference between WIC and non-WIC infants ages 7-11 months. Reported feeding of cow's milk was rare among younger infants (4-6 months). In this age group, however, significantly more WIC infants than non-WIC infants had been fed cow's milk at some point. These results should be interpreted with caution because the comparison group used in Ponza et al.'s analysis included all income levels, which may obscure differences between WIC participants and income-eligible nonparticipants, who constitute a more appropriate comparison group.

Burstein and her colleagues (1991) also found that WIC participation was associated with more appropriate introduction of solid foods. WIC infant feeding guidelines, which are based on recommendations of the American Academy of Pediatrics and other expert groups, recommend that no solids be introduced until infants are at least 4 months of age. Indeed, the WIC food package for infants younger than 4 months is limited to iron-fortified formula (USDA/FNS, 2003c). Burstein and her colleagues found that nonparticipant infants were significantly more likely than WIC infants to be fed solid foods before 4 months of age.

It is not clear whether this finding still holds for today's WIC infants. Based on the infant feeding inventory in NHANES-III, Cole and Fox (2004) found no difference between WIC participants and nonparticipants in the percentage of infants or children who were fed solid foods before 4 months of age. Similarly, Ponza and his colleagues (2004) found no differences between WIC participants and nonparticipants in the mean ages at which infant cereal and pureed baby foods were introduced. These data may be less reliable than the data from the Burstein et al. study, however, because they are based on a more extended recall period.[70] In addition, as noted previously, the all-income comparison group used by Ponza and his colleagues may obscure differences between WIC participants and income-eligible nonparticipants.

Kramer-LeBlanc et al. (1999) found that carbohydrate and fiber intakes among infants 4-11 months were significantly lower for WIC participants than for income-eligible nonparticipants and suggested that this pattern may be associated with earlier introduction and greater consumption of cereal among non-WIC infants. Data from Ponza et al., suggest that the difference in cereal consumption may be concentrated among older infants and, therefore, not associated with better adherence to infant feeding guidelines, per se. Ponza and his colleagues found no difference between WIC participants and nonparticipants in consumption of either infant cereal or ready-to-eat cereal among infants ages 4-6 months. Among infants ages 7-11 months, however, the percentage consuming ready-to-eat cereal was 77 percent lower for WIC participants than for nonparticipants.

Growth

A total of 12 studies attempted to measure the impact of WIC on the growth of infants and children (table 23). Findings from these studies are summarized in table 25. Note that the far-left column of the table, labeled "Participants higher," includes findings that can be considered both positive and negative. For example, greater height or length-for-age among WIC participants would generally be considered a positive finding, while a greater prevalence of overweight would be considered a negative finding.

Many of the earliest efforts to assess WIC's impact on children's growth were hampered by technical difficulties such as missing or inaccurate data in medical records or WIC files (Heimendinger et al., 1984; USDA/FNS, 1978) and problems with equipment calibration (Burstein et al., 1991). Self-selection issues have also affected this research. In the NWE, Rush and his colleagues (1988d) reported differential recruitment of children with abnormal growth (overweight, underweight, or stunted) into WIC, in keeping with the program's focus on individuals with identifiable nutritional risks. This pattern of self selection is likely the reason for the significantly greater prevalence of underweight and growth retardation among WIC children reported by Cole and Fox (2004) and Burstein et al. (2000) in their more recent descriptive analyses of NHANES-III data.

In the 1991 field test of the WIC Child Impact Study, Burstein and her colleagues (1991) explicitly attempted to control for selection bias. In their final report, however, they present results from both the single-equation models and the instrumental-variables models because there was some concern about the performance of both models. As shown in table 25, the instrumental-variables model found that WIC and a significant *negative* effect on infants' length-for-age. The single-equation model found that WIC participation had a significant and negative effect on head circumfrence.

Heimendinger et al. (1984) attempted to compensate for problems of self selection as well as the potential for regression to the mean in a longitudinal data set by determining the expected rate of growth and comparing "value added" measures for WIC and non WIC children who had at least two weight and height measurements. She demonstrated a positive WIC effect on

[70]The Burstein, et al. (1991) study was limited to 6-month-old infants, so caregivers reported on relatively recent feeding practices. The NHANES-III infant feeding histories analyzed by Cole and Fox (2004) included infants up to 12 months.

Table 25—Findings from studies that examined the impact of the WIC program on other nutrition, health, and developmental outcomes of infants and/or children

	Significant impact	No significant impact		Significant impact
Outcome	Participants higher	Participants higher/same	Participants lower	Participants lower
Growth				
Expected weight gain	Heimendinger (1981) [3 areas in 1 State] - B			
Height/length	Black (2004) [6 cities] {compared with no WIC due to access problems} - I Edozien (1979) [national] - B Hicks (1982) [3 counties] - C	Brown (1986) [county] - C	Rush (1988c) [national] - I	Black (2004) [6 cities] {compared with no WIC due to no need} - I Burstein (1991) [2 States] - I[1] Rush (1988c) [national] - C
Weight	Edozien (1979) [national] - B[2]	Rush (1988c) [national] - I Hicks (1982) [3 counties] - C	Rush (1988c) [national] - C	
Prevalence/ likelihood of underweight[3]	Cole (2004) [national] - C Burstein (2000) [national] - C	Burstein (1991) [2 States] - I	Black (2004) [6 cities] compared with no WIC due to no need} - I	Black (2004) [6 cities] {compared with no WIC due to access problems} - I
Prevalence of failure to thrive				Lee (2000) [1State] - B
Prevalence of growth retardation/stunting		Cole (2004) {national} - C		
Head circumference	Edozien (1979) [national] {enrolled within 1 month of birth} - I	Rush (1988c) [national] - B	Burstein (1991) [2 States] - I	
Prevalence/ likelihood of overweight[3]		Black (2004) [6 cities] - I CDC (1995) [national] - B {Blacks, Whites} Burstein (1991) [2 States] - I Rush (1988c) [national] - B[4]	Cole (2004) [national] - C Burstein (2000) [national] - C CDC (1995) [national] - B {Mexican-Americans}	
Measures of iron status				
Mean hematocrit/ hemoglobin or other measure	Miller (1995) [1 site] - C Smith (1986) [1 site] - B Vazquez-Seone (1985) [1 site] - B Edozien (1979) [national] - B Weiler (1979) [1 site] - I USDA/FNS (1978) [4 States] - B	Brown (1986) [county] - C[5]		

See notes at end of table.

Continued—

Table 25—Findings from studies that examined the impact of the WIC program on other nutrition, health, and developmental outcomes of infants and/or children—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants higher	Participants higher/same	Participants lower	Participants lower
Prevalence/ likelihood of anemia		Burstein (2000) [national] - C Lee (2000) [1 State] - B	Cole (2004) [national] - C Kahn (2002) [3 sites] - B Hicks (1982) [3 counties] - C	Sherry (2001) [national] - B Sherry (1997) [national] - B Burstein (1991) [2 States] – I[6] Yip (1987) [national] – B[7] Vazquez-Seone (1985) [1 site] - B Edozien (1979) [national] - B USDA/FNS (1978) [4 States] - B
Prevalence of iron deficiency anemia				Cole (2004) [national] - C
Other measures of nutrition/health/development				
Health status - caregiver-reported	Black (2004) [6 cities] {compared with no WIC due to access problems} - I		Cole (2004) [national] - C {2-4 years} Black (2004) [6 cities] {compared with no WIC due to no need} - I	Cole (2004) [national] - B {infants and 1 year}
Health status - physician assessed	Cole (2004) [national] - C {4 years} Carlson (2000) [national] - C	Cole (2004) [national] - C {1 and 2 years}	Cole (2004) [national] - B {infants and 3 years} Burstein (2000) [national] - C	
Immunization status	Luman (2003) [national] - C Shefer (2001) [national] - C Burstein (2000) [10 sites] - C {all others} Rush (1988) [national] -C	Burstein (2000) [10 sites] - C {MMR} Shaheen (2000) [1 city] - C James (1998) [1 site] - C		
Dental health status	Cole (2004) [national] - C	Burstein (2000) [national] - C		
Utilization of health care or dental care services	Cole (2004) [national] - C {ever} Lee (2004a) [1 State] - C Buescher (2003) [1 State] - C Lee (2000) [1 State] - B	Cole (2004) [national] - C {past year} Burstein (2000) [national] - C Rush (1988) [national] - B Paige (1983) [1 State] - I		Lee (2004a) [1 State] - C {emergency visits}[8]

See notes at end of table.

Continued—

Table 25—Findings from studies that examined the impact of the WIC program on other nutrition, health, and developmental outcomes of infants and/or children—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants higher	Participants higher	Participants lower	Participants lower
Health care/dental care costs	Buescher (2003) [1 State] - C	Paige (1983) [1 State] - I Lee (2004b) [1 State] - B {age 3}	Lee (2004b) [1 State] - B {age 2}	Lee (2004b) [1 State] - B {ages 0-1}
Household food security		Black (2004) [6 cities] {WIC compared with no WIC due to access problems} - I	Burstein (2000) [all samples] - C	Black (2004) [6 cities] {WIC compared with no WIC due to no need} - I
Developmental outcomes	Rush (1988) [national] - C Hicks (1982) [1 site] - C Hicks (1985) [1 site] - C		Kowaleski-Jones (2000) [national] - C {motor and social skills}	Kowaleski-Jones (2000) [national] - C {difficult temperament}[8]

Notes: Cell entries show the senior author's name, the publication date, the scope of the study (for example, national vs. 1 city or 1 State), and the participant group(s) involved: (B = both infants and children, C = children, and I = infants). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Findings reported for Lee (2004b) reflect results for total dental-care-related Medicaid costs. Separate analysis that examined likelihood of having *any* dental-care-related Medicaid costs showed that WIC participants were more likely than nonparticipants to have dental care costs during infancy (up to 12 months) and as 1- and 2-year-old participants. Difference for3-year-olds was comparable in direction, but not statistically significant.

Findings reported for Kowaleski-Jones and Duncan (2000) are based on a fixed-effects model that also controlled for prenatal FSP participation.

Findings for Burstein (1991) are based on the selection-adjusted model. The authors cautioned that both the single-equation model and the selection-adjusted model produced implausible results.

Paige (1983) did not test the statistical significance of between-group differences.

The USDA/FNS (1978) study found substantial decreases in the number of infants and children who were considered low length/height-for-age or low weight-for-height and a substantial increase in the number considered high weight for height. The authors concluded, however, that these apparent changes, which were largely limited to differences between initial and first followup visits were attributable to errors in initial measurements rather than WIC participation.

[1] Single-equation model found no significant difference between groups in length-for-age.
[2] The difference was apparent only during first 6 months of participation; thereafter returned to baseline levels.
[3] Based on comparison of proportions above or below defined cutoffs or differences in mean z-scores.
[4] Based on Quetelet's Index (weight (kg) x (100/height(cm)^2).
[5] WIC participants had slightly lower mean hemoglobin percentile, but the difference was not statistically significant.
[6] The between-group difference was significant when using the criteria defined by Yip et al. (1987) to measure anemia. The difference fell just short of significance when anemia was defined solely on the basis of hematocrit or hemoglobin.
[7] A significant, positive impact reported for the trends analysis that was not limited to WIC children and did not control for SES as well as the WIC-specific multivariate analysis.
[8] Authors reported significant findings at p <0.10.

the rate of weight gain among infants and toddlers, but had to abandon the analysis of impacts on linear growth because of problems with missing data.

The strongest and most recent data on WIC's impacts on the growth of infants and children come from studies by Black et al. (2003) and Lee et al. (2000). Black et al. reported data from the Children's Sentinel Nutrition Assessment Project (C-SNAP). C-SNAP included low-income infants whose caregivers were recruited at six urban medical centers. WIC participation status was not a criterion for enrollment into the study and was not known at the time of recruitment. The study did not collect information on household income, so the authors used the absence of private health insurance as a proxy to identify a comparison group of income-eligible nonparticipants. The authors did not control for selection bias but collected information on reasons for nonparticipation. They used these data to divide infants who were not participating in WIC into two groups: those who did not participate because their caregivers had problems accessing the program (64 percent of the total) and those who did not participate because their caregivers did not perceive a need for WIC services (36 percent). Reported access problems included being on a waiting last, scheduling difficulties, missed appointments, lack of time to pick up vouchers, relocation, and lack of transportation. There were noteworthy differences in the sociodemographic characteristics of the three groups. Caregivers who reported no need for WIC services were more likely than either caregivers of WIC participants or caregivers who reported WIC access problems to be employed, married, and White and were less likely to be receiving subsidized housing, TANF, or food stamps. Caregivers who did not participate in WIC because of reported access problems had lower rates of employment than WIC participants as well as lower rates of participation in other assistance programs.

The study examined z-scores for weight-for-age, length-for-age, and weight-for-length, using age- and gender-specific norms. Because data were collected in medical settings, information was collected on hydration status, which can dramatically affect an infant's weight. The authors used multivariate models to estimate differences between WIC participants and each of the nonparticipant groups. In addition to sociodemographic characteristics, models included variables to control for maternal depression, birthweight, and breastfeeding status.

For both WIC infants and infants who were not participating in WIC because their caregivers did not perceive a need for WIC services, z-scores for weight-for-age and length-for-age indicated that children were growing normally. In contrast, z-scores for both of these measures were below normal for infants who were not participating in WIC because of access problems, indicating that these infants were both underweight and short, relative to national norms.

Multivariate analyses revealed that WIC infants were significantly longer and significantly less underweight than infants who did not participate in WIC because of access problems. Infants who did not participate in WIC because their caregivers did not perceive a need for WIC services were comparable to WIC infants in weight-for-age, but were significantly longer than WIC infants. There were no differences between WIC participants and either group of nonparticipants in the prevalence of overweight. The unadjusted prevalence for all three groups of infants exceeded the 5 percent that would be expected based on national norms (7-9 percent).

The authors concluded that WIC participation has a positive impact on infant growth. While acknowledging the potential for selection bias, the authors suggest that findings from the comparison of WIC participants with the slightly more advantaged comparison group (infants whose caregivers did not perceive a need for WIC services) strengthen this conclusion. Because of differences in sociodemographic characteristics as well as a difference in household food security (WIC participants were more likely to be food insecure), one might expect this group of nonparticipants to do better than WIC participants. However, both groups had comparable weight-for-age and, although nonparticipants were significantly longer, both groups were growing normally. The authors also point out that infants who did not participate in WIC because of access problems had a higher rate of breastfeeding initiation than WIC participants (62 percent vs. 54 percent). Because healthy breastfed infants generally grow more rapidly than nonbreastfed infants during the first two months of life, the unadjusted z-scores observed between these two groups are contrary to what one would expect to see (WIC infants were growing normally, but infants in the WIC-access-problem group had weight-for-age and length-for-age z-scores that were below national norms).

Lee and his colleagues (2000) assessed the impact of WIC participation on the prevalence of failure to thrive. Failure to thrive is a general diagnosis that can have many causes. The sentinal finding, however, is a failure to gain weight and to grow as expected. The authors used a longitudinal, linked database that included, for all children born in Illinois between 1990 and 1996, birth record, WIC, and Medicaid data for 1990 though 1998.[71] The analysis was limited to children who were born between 1990 and 1994 and were continuously enrolled in Medicaid from birth until their fourth birthday. To control for the fact that children diagnosed with failure to thrive may be more likely to participate in WIC—because it is an accepted nutritional risk—the authors included in the WIC participant group only those children whose WIC enrollment predated their diagnosis. Children whose enrollment in WIC occurred after their diagnosis were considered nonparticipants. Results showed that Medicaid children who had ever participated in WIC were significantly less likely than those who never participated to be diagnosed with failure to thrive (based on primary diagnosis in Medicaid claims data).

In recent years, increasing attention has been paid to problems at the opposite end of the growth spectrum—the problem of overweight among children, including very young children. A 1995 CDC report provides suggestive evidence that WIC participation is not associated with the prevalence of overweight among children. Using NHANES-III data, CDC analysts examined children's weight-for-height, relative to national standards, by age and race/ethnicity. They found that differences between WIC and income-eligible nonparticipant children were neither consistent nor statistically significant (CDC, 1995). White and Black WIC children tended to weigh more than low-income nonparticipants of the same race, and Mexican-American WIC children tended to weigh less than their non-WIC counterparts, but none of the differences were statistically significant. Multivariate analysis (not further described) did not change the pattern or significance of the findings.

Data from Black et al. (2004) and Lee et al. (2000) provide suggestive evidence that WIC may have a positive effect on growth in infants and children, and the 1995 CDC study suggests that WIC participation is not associated with excess weight in children. It is doubtful, however, that studies like these can provide definitive answers to questions about WIC's impact on the growth of infants and children. The researchers involved in designing and implementing the field test of USDA's planned WIC Child Impact Study concluded that the only way WIC's impacts on child growth and development—indeed, WIC's impact on virtually any outcome beyond dietary intake—can be reliably assessed is through a longitudinal study that includes serial measurements, repeated at regular intervals, for both WIC participants and nonparticipants (Puma et al., 1991).

[71]The database also included information on FSP and TANF/AFDC participation, which the authors used to examine patterns of program participation during welfare reform in Illinois (1990-98).

Anemia/Iron Status

A number of different methods were used in the 16 studies that examined the impact of WIC on children's iron status, including typical participant vs. nonparticipant designs that made cross-sectional comparisons of single point in time measurements, longitudinal or followup studies in which initial measures for the same individuals were compared at a later time point (designs referred to as "participants, before vs. after" and "participant vs. nonparticipant, before and after" in chapter 2), and time series analyses that used aggregate data to compare the prevalence of anemia in a particular population over time. Although each of these designs has weaknesses, as described in chapter 2, the consistency of findings is compelling. The majority of studies that examined the relationship between WIC participation and iron status/anemia found that WIC participation was associated with an increase in mean levels of hemoglobin or hematocrit and/or a decrease in the prevalence of anemia (table 25). In most cases, these differences were statistically significant.

The most convincing evidence comes from analyses done by Yip and his colleagues at the CDC using PedNSS data (Yip et al., 1987). The CDC researchers looked at the prevalence of anemia in infants and children ages 6-60 months between 1975 to 1985, a period of substantial growth in the WIC program. They documented a steady decline in the prevalence of anemia, from 7.8 percent in 1975 to 2.9 percent in 1985. Using detailed data from one State, the authors demonstrated that the socioeconomic status of the population had remained stable over this period. The authors also compared initial and followup measures of hemoglobin or hematocrit (taken roughly 6 months apart) for approximately 73,000 WIC children. The analysis revealed decreased levels of anemia at followup.

Another CDC analysis reported on trends between 1980 and 1991 (Yip et al., 1992). During this period, the prevalence of anemia decreased by more than 5 percent for most age and race/ethnicity specific subgroups. Other measures of childhood health monitored in PedNSS, including the prevalence of low birthweight, low height-for-age, low weight-for-height, and high weight-for-height (overweight), generally remained stable. Comparable findings have been reported by Sherry et al. (1997; 2001).

The CDC analyses suggest that WIC has a direct impact on the prevalence of childhood anemia as well a probable indirect effect. WIC requires use of iron-fortified infant formulas and includes iron-fortified breakfast cereals in its food packages. Because more than half of all formula sold in the United States, as well as a large proportion of breakfast cereals, are purchased with WIC vouchers (Batten et al., 1990), manufacturers have consciously focused on bringing to market iron-fortified products that are allowable in WIC food packages. These foods have assumed a leading position in their respective markets and have therefore been increasingly consumed by both WIC and non-WIC children. As a result, the WIC program may have contributed to the observed improvement in the prevalence of anemia in the general population of low-income U.S. children.

The declining prevalence of iron deficiency may have diminished the predictive value of anemia as a screen for iron deficiency. When the prevalence of iron deficiency is high, anemia is a good predictor of iron deficiency. However, when the prevalence is low, the majority of anemia (low hemoglobin or hematocrit levels) is due to other causes (U.S. Department of Health and Human Services (HHS), 2000; Sherry et al., 2001). In young children, a likely cause is infection and inflammation associated with viral illness and, to a lesser extent, hereditary anemias (HHS, 2000; Bogen, 2002). This may be the reason that Kahn et al. (2002), in tracking the prevalence of anemia among infants and children in three Illinois clinics between 1997 and 1999, found a substantial amount of crossover between anemic and nonanemic groups over time (Bogen, 2002). Future efforts to examine WIC's impact on the iron status of infants and children should assess the prevalence of iron deficiency and/or iron-deficiency anemia rather than simple anemia. A recent descriptive analysis of NHANES-III data found that WIC children were significantly less likely than income-eligible nonparticipant children to be iron deficient (Cole and Fox, 2004).

General Health Status

A total of four studies examined the health status of WIC and non-WIC infants and/or children using caregiver reports and/or physician reports (table 24). The strongest data are provided by recent studies by Black et al. (2004) and Carlson and Senauer (2003). Both studies suggest a positive WIC impact, as shown in table 25, but the potential for selection bias remains a concern.

Black and her colleagues (2004) assessed differences in the perceptions of caregivers about the health status of urban low-income infants enrolled in the previously described C-SNAP study. Caregivers were asked to rate their infants' health status as excellent, very good, good, fair, or poor. The question and response options were taken directly from the NHANES-III caregivers interview. Results showed that, relative to WIC infants, infants who were not enrolled in WIC because of access problems were significantly more likely to be rated as having fair or poor health. There was no significant difference in the caregiver-rated health status of WIC infants and infants who were not enrolled in WIC because their caregivers did not perceive a need for WIC services.

Carlson and Senauer (2003) assessed WIC impacts on physician-assessed health status using NHANES-III data. Physicians rated children's health status (excellent, very good, good, fair, or poor) after completing a physical examination. Because so few children were reported to be in poor health, the poor and fair categories were combined for the analysis. The authors used ordered probit models to estimate the likelihood of a child being in excellent health. Two analysis samples were used. The "WIC sample" included children between the ages of 24 and 60 months who either participated in WIC or were income-eligible based on household income or Medicaid participation. WIC participation was defined at the household rather than individual level. The "full sample" included all children between the ages of 24 and 60 months who had complete data. Results for both the full sample and the WIC sample indicated that WIC participation had a significant, positive effect on the likelihood that a child would be in excellent health. The size of the effect (increased probability of being in excellent health) ranged from 4.6 to 11.4 percentage points, depending on the model used. Effects were consistently higher in the "WIC sample," indicating that the impact of WIC on health status was most pronounced for poorer children.

The previously described study by Lee et al. (2000) looked at the prevalence of "nutritional deficiencies" among Medicaid children in Illinois who did and did not participate in WIC. The measure, based on primary diagnosis in Medicaid claims data, is not well described, but is said to "include conditions such as malnutrition and vitamin deficiencies." The authors found that children who had ever participated in WIC were significantly less likely than children who had never participated in WIC to be diagnosed with these conditions (finding is not included in table 25). As previously mentioned, the analysis explicitly excluded from the WIC participant group children whose diagnosis predated their WIC enrollment.

Immunization Status

The literature search identified six studies that examined the immunization status of WIC participants relative to nonparticipants (table 24). Results are summarized in table 25. Although this research suggests that WIC has a positive impact on children's receipt of complete and timely immunizations, the findings are particularly vulnerable to selection bias. Mothers who are motivated to enroll their child in WIC may also be motivated to ensure that the child is properly immunized.

The NWE (Rush et al.,1988c) found that children who were enrolled in WIC after the first year of life were significantly more likely than children in the control group to have an immunization card and to have received a measles vaccination (given after 15 months of age). In addition, children whose mothers participated in WIC prenatally and were enrolled in WIC during early infancy were more likely to have received diphtheria-pertussis-tetanus (DPT) immunization and, to a lesser extent, polio vaccines.

James (1998) studied the immunization status of 150 children receiving care at one health care center in New York State. She randomly selected a sample of WIC children who were up to date on immunizations at 12 months of age and matched them on age and gender (only) with subjects from a randomly selected group of non-WIC children who were receiving care at the same health care center. She then documented immunization status at 24 months of age and used chi-square tests to assess differences between the two groups. No significant differences were found. This result is not surprising in view of the fact that all of the study children were clearly in "immunization aware" households (all children had up-to-date immunizations at 1 year of age) and all had a primary source of health care.

Shaheen and her associates (2000) studied immunization coverage among low-income children in Los Angeles. They completed a household enumeration of children ages 24-47 months in 30 randomly selected clusters (block groups) in and around downtown. A random sample of 300 children was selected and their parents or guardians were interviewed in person. Information on immunization status was abstracted from home immunization cards, if available (81 percent of all cases), or obtained from health care providers. WIC participation was positively but not significantly associated with completing immunization series on time. Among children who had not received their first round of immunizations at the recommended age, the combination of WIC participation and having a home immunization card was significantly and positively associated with being fully immunized by 24 months of age. Sample sizes for the latter analysis were very small and may have obscured any independent WIC effect.

Shefer and her colleagues (2001) analyzed data from the 1999 National Immunization Survey (NIS). The NIS has been conducted by the CDC since 1994 to estimate vaccination coverage rates for U.S. children ages 19-35 months. Children were divided into four groups based on WIC participation status and income: currently on WIC, previously on WIC, never on WIC but income-eligible, and never on WIC and not income-eligible. Bivariate comparisons showed that, among children with household incomes at or below 100 percent of poverty, children who had ever participated in WIC were significantly more likely to have up-to-date immunizations at 24 months than children who had never participated in the program (71 percent vs. 56 percent). Moreover, across all income groups, children who currently participated in WIC were more likely than previous WIC participants to have up-to-date immunizations at 24 months (75 percent vs. 69 percent). However, current WIC participants were less well immunized than higher income children who had never been on WIC (75 percent vs. 83 percent).

Luman and her colleagues (2003) expanded on Shefer's work, analyzing data from the 2000 NIS to identify maternal characteristics associated with children's immunization.[72] Children not currently participating in WIC were divided into three nonparticipant categories comparable to those defined by Shefer et al. (2001). A

[72]Luman et al. (2003) characterized WIC participation as a "maternal characteristic," but the 2000 NIS instrument actually collected information on the child's participation (Abt Associates Inc., 2002).

multivariate regression that controlled for child's age showed that children who were currently participating in WIC were significantly more likely than any of the nonparticipant groups to be fully immunized. Children who had never participated in WIC but were income-eligible were least likely to be fully immunized.

The positive WIC impact suggested by this research, if real, may be influenced by an ongoing collaboration between USDA and the CDC to use the WIC program as a means to improve immunization rates among the Nation's low-income children. Since the early 1990s, a variety of strategies has been used to promote timely and complete immunizations among WIC participants, including the following (Shefer et al., 2001):

- Assessment and referral—May occur at each WIC visit or at each 6-month recertification visit and may include a computerized tracking system.
- Escort programs—Children who need immunizations are escorted to locations where immunizations are given.
- Voucher incentive programs—Restrict access to WIC vouchers until a child is fully immunized (for example, require that caregivers come into the WIC clinic every month rather than every 2 months to pick up WIC vouchers).
- Outreach and tracking programs—May involve mail, telephone, or home visit reminders for underimmunized children.

Randomized trials have demonstrated that some of these strategies can dramatically increase immunization coverage among WIC participants (Birkhead et al., 1995; Hutchins et al., 1999). In addition, Shefer et al. (2001) modeled the relationship between WIC immunization activities and immunization rates among WIC children. Using data from the 1999 NIS and data from an annual survey of WIC directors and State immunization program directors, Shefer and her colleagues found that WIC participants in States with high-intensity immunization activities (50 percent or more of WIC children enrolled in sites that implemented an immunization intervention at every visit) had significantly higher rates of up-to-date immunization at 24 months than WIC participants in States with low-intensity immunization activities (less than 50 percent of WIC children enrolled in sites that used an immunization intervention and the intervention was implemented only at recertification visits). Finally, Dietz et al. (2000) found that a WIC voucher incentive program was one of eight factors that had a positive, significant effect on immunization rates in Georgia's public health clinics.

Use and Costs of Health Care Services

The NWE examined use of preventive health care services among WIC and non-WIC infants and children and found no significant difference between the two groups (Rush et al., 1988c). More recently, three studies examined the relationship between children's WIC participation and the use of health care services (Lee et al, 2000; Buescher et al., 2003) and dental care services (Lee et al., 2004a) using datasets similar to the WIC-Medicaid databases used to assess WIC's impact on birthweight (table 24). All three studies reported that WIC participation had a significant, positive effect on the use of health care/dental care services (table 25). These results suggest a positive WIC impact; however, only the study that looked at dental care services controlled for selection bias. Thus, the two studies that assessed use of health care services are vulnerable to potential selection bias—it is possible that children who have health problems or who use more health care services may be more likely to be referred to WIC. In addition, results of all three studies have limited generalizability because they used datasets for a single State (one study used data from Illinois and two used data from North Carolina) and were limited to WIC participants enrolled in Medicaid.

As described previously, Lee et al. (2000) used a longitudinal database that included birth record, WIC, and Medicaid data for all children born in Illinois between 1990 and 1996. They used a proportional hazards model to estimate the effect of WIC participation on the timing of children's first screening in the Early Periodic Screening, Diagnosis, and Treatment (EPSDT) program.[73] Their analysis included all children new to the Medicaid program between 1991 and 1997. The dependent variable was the time between entry into the Medicaid program and receipt of the first EPSDT screening. WIC participation was coded as a time-varying covariate. Results showed that WIC participation had a significant, positive effect on the likelihood of receiving EPSDT screening.

Buescher et al. (2003) and Lee et al. (2004a) used a database that included linked birth certificate, Medicaid, and WIC records for all children born in North Carolina in 1992. Buescher and his colleagues assessed the

[73]EPSDT is a comprehensive, prevention-oriented child health program that State Medicaid agencies are required to provide for all Medicaid recipients under the age of 21. See www.cms.hhs.gov/medicaid/epsdt.

impact of WIC participation on the use and cost of health care services. Lee and her associates focused on WIC impacts on the use of dental care services. They also completed a separate analysis that assessed impacts on dental care costs (Lee et al., 2004b).

Buescher et al. (2003) studied children ages 1-4 who had been continuously enrolled in Medicaid for at least 1 year of life. Separate analyses were completed for four different age cohorts based on completed year of age. A cumulative measure of WIC participation was defined based on the percentage of months from age 1 through the current age in which WIC food vouchers had been redeemed. Three levels of WIC participation were identified: high = more than 66 percent; medium = 34-66 percent; low = 33 percent or less. Because the analysis looked at cumulative use of health care services and cumulative Medicaid costs, this definition of WIC participation was deemed more appropriate than a definition based on current participation.

Tobit regression was used to analyze Medicaid cost data because of the large number of zero values. Logistic regression was used to estimate the odds of having a well-child visit, being hospitalized, having an emergency room visit, and being diagnosed/treated for a common childhood illness. An array of variables was used to control for sociodemographic characteristics of the child and his/her mother, including prenatal WIC participation for the mother and participation in WIC as an infant for the child. In addition, the authors included a variable to designate whether EPSDT services were received in a public health department. This variable helped control for differences in costs associated with public vs. private health care providers, as well as for co-location of WIC clinics with child health care services. The authors attempted to control for selection bias but, after examining several multi-stage models, were unable to identify good predictors of WIC participation that were not also associated with the outcome measures.

Results showed that WIC participation had a significant, positive effect on the likelihood that children would receive any well-child care and on the likelihood that children would receive at least one EPSDT visit per year, as recommended. This effect was noted for all four age cohorts and, with one exception, for all three levels of WIC participation. Comparable results were noted for the likelihood of visiting an emergency room or of being diagnosed with otitis media or upper respiratory infection. Medium to high levels of WIC participation were positively associated with the likelihood of being diagnosed with a lower respiratory infection. Results for other common childhood illnesses (asthma, gastroenteritis, and allergy) varied by age cohort and/or level of WIC participation. In most cases, however, high WIC participation was associated with a significantly greater likelihood of diagnosis for these conditions. A high level of WIC participation was also associated with a significantly greater likelihood of being hospitalized.

Given these patterns, it is not surprising that WIC participation was associated with increased Medicaid expenditures. WIC participation was consistently associated with greater expenditures for outpatient services (all four age cohorts and all three levels of WIC participation). In addition, medium and high levels of WIC participation were generally associated with significantly greater Medicaid costs overall and for individual types of medical care (EPSDT, well-child care, physician services, drugs, and dental care). The authors concluded that “the bottom line is that children enrolled in Medicaid who participate in WIC are linked to the health care system and are much more likely to receive both preventive and curative care, whereas Medicaid-enrolled children who do not participate in WIC simply are not as connected to the health care system.”

Lee et al. (2004a; 2004b) studied the impact of children’s WIC participation on the use and cost of dental care services. Their analysis of dental care use controlled for selection bias by using a two-stage model that incorporated State-level WIC data to predict WIC participation (Lee et al., 2004a). The model included three variables that the authors found to be correlated with WIC participation but not with the use of dental care services, including the number of WIC clinics per county, the number of full-time WIC workers per county, and WIC hours of operation.

The analysis looked at the annual use of dental care services as well as the types of services used (diagnostic/preventive, restorative, emergency) by children ages 1-4. A categorical rather than continuous outcome measure was used to represent annual use of dental care services (no visits, one visit, two or more visits) because the recommended number of visits is two per year and because visits in excess of two per year may be more indicative of severe dental disease than of access to dental care services. WIC participation was measured based on the number of months when any WIC food vouchers were redeemed during each year of life. In addition to controls for sociodemographic characteristics, the model controlled for length of Medicaid enrollment as well as the relative availability of dental practitioners (ratio of dentists per population).

WIC participation was associated with a significant increase in the use of dental care services. Children who participated in WIC for either 6 months or a full year were significantly more likely than children who did not participate in WIC to have both one dental visit per year and two or more dental visits per year. Children who participated in WIC were also significantly more likely to have used preventive and restorative dental health services and significantly less likely to have had an emergency dental visit. The latter finding was significant at the $p < 0.10$ level. The authors suggest that WIC participation provides children with a "better connection to the health care system that can lead to care that is more planned and less urgent."

Lee and her colleagues (2004b) also assessed the impact of WIC participation on Medicaid costs for dental care, but used a slightly different approach. They did not control for selection bias because they had to use a two-stage model to first estimate the probability of a child having any dental care expenditures (many children had no dental care expenditures). In addition, they estimated separate models for infants and for cohorts of children ages 1, 2, and 3. They excluded 4-year-olds from the analysis to minimize the potential for simultaneous determination. Finally, they defined WIC participation as a dichotomous variable, reflecting whether any of the child's WIC food vouchers had been redeemed in each particular year of life. Age-specific models controlled for WIC participation at other ages.

Results showed that, among infants and children ages 1 and 2, WIC participation was associated with a significantly greater likelihood that a child would have some Medicaid charges for dental care services. This relationship was not observed among 3-year-olds. In addition, among infants and 1-year-olds, WIC participation was associated with significantly lower dental care costs (19-20 percent less). No significant differences were detected in total dental care costs for children ages 2 and 3. Investigation of the sources of dental care costs showed that WIC participants were significantly less likely than nonparticipants to have received dental care services in a hospital setting (as opposed to a primary care setting). This difference in dental care source accounts for at least some of the lower cost observed for WIC participants despite a higher prevalence of dental care charges overall. The authors concluded that observed difference between WIC participants and nonparticipants may be the result of nonparticipants having more dental problems and/or a greater tendency to use emergency room services rather than standard outpatient/primary care services.

Cognitive Development and Behavior

Five of the identified studies assessed WIC impacts on measures of cognitive development or behavior (table 24). The NWE assessed children's attention and cognition using three standardized tests known to be responsive to changes in early life: the Infant Behavior Inventory (IBI), the Peabody Picture Vocabulary Test (PPVT), and the McCarthy Scales of Infant Development (Rush et al., 1988c). The results indicated that infants and children whose mothers had participated in WIC prenatally had significantly higher receptive vocabulary scores than infants and children whose mothers had not participated in WIC (table 25). In addition, children who enrolled in WIC after the first year of life had significantly better digit memory (counting backward and forward) than children in the control group. Rush and his colleagues appropriately cautioned that small sample sizes and substantial differences in the socioeconomic status of WIC and control group members limit the strength of these associations.

Hicks, Langham, and Takenaka (1982) studied 21 sibling pairs in Louisiana. One of the siblings (the younger) had received WIC benefits both "prenatally" (during the third trimester), through the first year of life, and on into childhood, while the other (older) sibling received WIC benefits only in childhood—that is, after 1 year of age. The children were studied when the younger children were about 6 years old and the older children were about 8. The "early supplementers" had participated in WIC an average of 56.1 months (counting the prenatal period), while WIC participation for the "late supplementers" averaged 30.8 months.

Investigators used a battery of measures to assess behavior and cognitive performance and also obtained school grades for reading, writing, and arithmetic. Strong, positive, and statistically significant differences, favoring the child with greater WIC exposure, were found for most of the cognitive and behavioral measures assessed, including IQ, attention span, visual-motor synthesis, and school grade point average. The authors attributed these differences to a superior nutrition environment for the "early supplementers" during critical periods of brain development in the last trimester of pregnancy and the first 6 months of life. A followup study completed 32 months later reported that the findings for IQ scores and school grades, the only two measures replicated, still held (Hicks and Langham, 1985).

The Hicks studies have been heavily criticized by others who have studied the effects of nutrition supplementation on cognitive development (Pollitt and Lorimor, 1983). One of the greatest concerns is the magnitude of the differences reported—which exceed many obtained from controlled trials of nutrition supplementation in populations with high prevalence of malnutrition and infectious diseases. Other problems include the small sample size and the potential that the “late supplementers” differ in important ways from the “early supplementers.”

A stronger and more recent study by Kowaleski-Jones and Duncan (2000) examined the impact of prenatal WIC participation on temperament and the development of motor and social skills. The authors used data from the NLSY, focusing on children born to NLSY participants between 1990 and 1996 (collection of data on WIC participation began in 1990).

Temperament was measured with a composite “difficult temperament” index, which included subscales for predictability, fearfulness, positive affect, and friendliness—factors thought to be precursors of personality development and social adjustment. Motor and social skills were assessed based on an established mother-reported scale that measures motor, social, and cognitive development of young children from birth though age 3. The analysis used the earliest available measures for each child. Measurements for about half of the children were collected during their first year of life. Measures for the other half were collected between their first and second birthdays. Analytic models controlled for the child’s age at the time of measurement.

The authors used both standard regression models and fixed-effects models, based on sibling pairs, to estimate WIC impacts. Three different specifications were used for both models: one that controlled for the child’s sociodemographic characteristics, one that added an array of prenatal and maternal characteristics (many of which dropped out in the fixed-effects model), and one that also controlled for prenatal participation in the FSP.

For the difficult temperament index, the direction of the coefficient for prenatal WIC participation varied between the standard regression models (positive coefficient, indicating that WIC participation was associated with an increased likelihood of having difficult temperament) and the fixed-effects models (negative coefficient, indicating that WIC participation was associated with a decreased likelihood of having a difficult temperament). The only model in which WIC participation was significant at the $p < 0.05$ level or better was the simplest regression model, which did not control for prenatal and maternal characteristics or for FSP participation. However, the authors reported a significant, positive WIC effect based on results from the fixed-effects model that was significant at the $p < 0.10$ level. The direction of WIC coefficients in the models that estimated impacts on motor and social skills was also sensitive to specification, and none of the models found a significant WIC effect, even at $p < 0.10$.

Food Security

Only one of the identified studies used the 18-item USDA food security module to assess the impact of WIC participation on household food security. In the previously described C-SNAP study, Black et al. (2004) assessed household food security among low-income infants who did and did not participate in WIC. The authors found, relative to WIC participants, a significantly lower rate of household food insecurity (or a higher level of food security) among infants who were not participating in WIC because their caregivers did not perceive a need for WIC services. There was no significant difference in household food insecurity between WIC participants and nonparticipants who did not participate because of access problems.

Impacts of WIC Participation on Nonbreastfeeding Postpartum Woman and Breastfeeding Women

The literature search identified two studies that assessed WIC impacts on nonbreastfeeding postpartum WIC participants and only one study that looked at the impact of WIC participation on breastfeeding participants. In addition, the previously described study by Kramer-LeBlanc et al. (1999) compared dietary intakes of breastfeeding and nonbreastfeeding postpartum WIC participants to income-eligible nonparticipants. The analysis of breastfeeding women was hampered by small sample sizes. The analysis of nonbreastfeeding postpartum women showed that WIC participants consumed significantly more calcium, riboflavin, and retinol than nonparticipants.

Nonbreastfeeding Postpartum Women

Pehrsson et al. (2001) assessed iron status among nonbreastfeeding postpartum women. The study took place during a time when some counties in Maryland were certifying only high-risk nonbreastfeeding postpartum women because of funding shortages. The participant group included 57 low-risk WIC participants who were recruited from WIC sites in Baltimore City, which was enrolling all eligible postpartum applicants. The nonparticipant group included 53 WIC-eligible women who were recruited from WIC sites in counties that were enrolling only high-risk women. Subjects were matched on race and age. All were older than 19, had delivered a full-term infant the preceding month, were free of major health problems, and qualified as low-risk nonbreastfeeding postpartum participants.

Women were recruited into the study within 30 days of delivery and were followed at 2, 4, and 6 months postpartum. Major outcome variables included four measures of iron status (hemoglobin, transferrin receptor, ferritin, and ratio of transferrin receptor to ferritin). Dietary iron intake was estimated using a self-administered food frequency questionnaire designed for respondents with low levels of literacy.

Although mean hemoglobin concentrations were comparable at baseline, the mean for WIC participants increased over time. At 6 months postpartum, the difference was statistically significant, after controlling for FSP participation, smoking status, use of iron supplements, and interpregnancy interval (months elapsed between immediately preceding pregnancy and prior pregnancy). Moreover, at 6 months postpartum, significantly more nonparticipants than participants were anemic based on the CDC-recommended cutoff for hemoglobin. No significant differences between WIC participants and nonparticipants were detected for any of the other measures of iron status or for dietary intake of iron.

The authors concluded that nonbreastfeeding postpartum WIC participants who experienced 6 uninterrupted months of participation were significantly less likely to become anemic than comparable women who did not participate in WIC during the postpartum period. Because they observed no difference between groups in means for the other measures of iron status, in the percentage of women classified as iron deficient based on these measures, or in dietary iron intake, the authors concluded that observed differences in hemoglobin concentrations and in the prevalence of anemia may not be associated with improvements in iron status among participants. Rather, these differences may be attributable to increased rates of other nutritional deficiencies, compromised health care, and infection or inflammation among nonparticipants.

This study may have underestimated WIC's effect on the iron status of postpartum women because most of the women, including all but two of the nonparticipants, participated in WIC during pregnancy and because all subjects were relatively iron replete when they entered the postpartum period. In addition, these authors, like Rush et al. (1988d), discussed the myriad factors that can influence iron status and recommend that future studies collect an expansive set of variables that will allow for a more sophisticated analysis. These include biochemical indicators of infection and inflammation, menstrual history, use of oral contraceptives and reestablishment of menses, and medical documentation on the presence of infection, inflammation, and conditions that can affect blood-related measures (for example, sickle-cell anemia and thalassemia).

In the other identified study of nonbreastfeeding postpartum women, Caan et al. (1987) studied the benefits of WIC participation during the interpregnancy interval, looking at women's nutritional status at the start of the second pregnancy and birth outcomes of that pregnancy. The study involved 47 local WIC agencies in California that had different policies for enrolling nonlactating postpartum women between 1981 and 1983. Because of funding shortages during that period, some local agencies were not able to serve these lower priority participants.

In 1983, researchers recruited newly enrolling pregnant women in both types of local agencies (agencies that had and had not served nonbreastfeeding postpartum women between 1981 and 1983). To be eligible to participate in the study, women had to meet the following criteria: (1) given birth to another infant between 1981 and 1983; (2) participated in WIC during the previous pregnancy; and (3) did not breastfeed the first infant. Women were divided into two groups based on their postpartum participation after the previous pregnancy: an extended feeding group, which included women who had received postpartum WIC benefits for 5-7 months, and a limited feeding group, which included women who received postpartum benefits for 0-2 months. Both groups had a comparable interpregnancy period (the time elapsed between first and second pregnancies) of 3 years or less.

The final analysis sample included 642 women (307 limited feeding and 335 extended feeding). The study found a positive, significant impact of extended WIC participation after the first pregnancy on both birthweight and birth length of the second infant. The odds ratio of having a low-birthweight infant approached significance, but, because low birthweight is a rare event, small sample sizes hampered the analysis (24 infants, 5.1 percent of all infants in the limited feeding group and 3.2 percent of all infants in the extended feeding group).

Positive effects from WIC participation were also reported for maternal outcomes. Women in the extended feeding group who had been obese at the start of the first pregnancy were 50 percent less likely than comparable women in the limited feeding group to be obese at the start of the second pregnancy. Although not a statistically significant trend, women in the extended feeding group who had been underweight at the onset of the first pregnancy tended to weigh more at the onset of the second pregnancy. Finally, mean hemoglobin levels (measured at the time of enrollment for the second pregnancy) were significantly higher in the extended feeding group; however, women were no more likely than those in the limited feeding group to be anemic.

Caan and her colleagues point out that evidence from other studies suggests that physiologic and metabolic adjustments associated with pregnancy proceed more normally in women whose nutritional status is good at the beginning and very early stages of pregnancy. Thus, the authors assert, among women with short interpregnancy intervals, WIC participation during the postpartum period may have an even stronger positive impact on birth outcomes than prenatal participation alone, especially for women who might not enroll in WIC until the second trimester of pregnancy. The authors appropriately acknowledge that their study has several limitations, including exclusion of a subgroup of recruited women because of problems with missing data and the potential bias associated with the fact that women in each study group were drawn from mutually exclusive sets of local agencies.

While further research is needed to replicate or expand the work done by Pehrsson et al. and Caan et al., these studies are important in that they provide the only source of information on potential WIC impacts among nonbreastfeeding postpartum women, the program's lowest priority group. If the hypotheses outlined by these researchers, particularly those of Caan et al., are valid, there may be reason to rethink the low priority assigned to this participant group.

Breastfeeding Women

In addition to the descriptive study by Kramer-LeBlanc et al. (1999), the only study that focused specifically on breastfeeding women examined nutrient intakes in a very small convenience sample of breastfeeding WIC participants (n=11) and an even smaller sample of middle-class breastfeeders not participating in WIC (n=5) (Argeanas and Harrill, 1979). Researchers collected 24-hour dietary recalls from each subject at approximately 6 weeks postpartum and again 2 months later. The authors reported that the middle class non-WIC women had higher mean intakes of energy and nutrients at the time of the first interview. WIC participants significantly increased their intakes over the 2-month study period, while nonparticipants' intakes decreased, resulting in comparable intakes for the two groups. While the results are intriguing, little can be concluded from this dated, poorly designed study.

Impacts of WIC Participation on WIC Households or Undifferentiated WIC Participants

Five of the identified studies looked at the impact of WIC participation on all types of WIC participants (without differentiating women, infants, and/or children) or on household-level outcomes. Two studies looked at dietary quality, one study examined household food use, and two studies estimated impacts on household food expenditures. Both of the latter studies used data from the NWE.

Dietary Quality

Basiotis, LeBlanc, and Kennedy (1998) used data from the 1989-91 CSFII to look at dietary quality among low-income households eligible to participate in the FSP (income less than 130 percent of poverty) and assessed the impact of participation in the FSP (entire household) and WIC (any household member). Dependent measures used in the study were the Healthy Eating Index (HEI) and its component scores.

The authors found that having one or more household members participate in WIC was associated with a very strong positive impact on dietary quality. Specifically, overall HEI scores for households with one or more WIC participants were 23 points higher than scores for other households, a substantial effect given that the mean score for all of the low-income households included in the sample was 62 points. WIC participation was also associated with significant increases in all component scores except those associated with vegetable consumption and intake of saturated fat.

Wilde et al. (2000) used more recent CSFII data (1994-96) to estimate the impact of WIC participation on dietary quality. This study used data on Food Group Pyramid servings to examine impacts on intake of meats, fruits, vegetables, grains, dairy, added sugars, and added fats. The only significant effect noted for WIC was a positive one for intake of added sugars (fewer teaspoons). Results did not differentiate between women participants and child participants. The authors suggest that this effect may result from participants substituting WIC-supplied fruit juices and cereals for higher-sugar soft drinks and cereals.

While these studies are important, in that they are the only ones to examine WIC impacts on dietary quality, expanding the focus to impacts on particular types of WIC participants would be useful. Although the number of pregnant and postpartum women is likely to be small in the CSFII database, the combined CSFII 1994-96, 1998 CSFII database includes a substantial number of age-eligible children and a reasonable number of infants.

Household Food Use

Taren et al. (1990) studied food use among low-income households. The sample included families participating in food cooperatives for low-income families in Hillsborough County, FL, as well as families participating in the local Expanded Food and Nutrition Education Program (EFNEP). Data were collected on the number of servings of 27 different foods used in the household the previous week. A serving was defined as the preparation and offering of a particular food, without attention to portion size or multiple helpings during the same meal.

Multiple regression analysis was used to identify factors associated with the number of family food servings used per week. Participation in WIC had a positive, significant impact on the number of servings of food used per week. The report does not provide details about the food list used in the study or about relative impacts for different categories of food, making it impossible to determine whether the list gave substantially more weight to WIC foods than to other foods or the relative contribution of WIC foods to the overall total.

Household Food Expenditures

The NWE included a substudy to examine the impact of WIC participation on household food expenditures (Rush et al., 1988b).[74] The researchers concluded, however, that the study could provide little useful information because of three key problems: disparities in family income and food expenditures between WIC and non-WIC households that actually became worse after statistical controls for several sociodemographic characteristics; disparate results for data collected using different methods (diary vs. recall); and a devaluing of the WIC food package by WIC participants.[75]

Arcia, Crouch, and Kulka (1990) reanalyzed the NWE data and estimated empirical models of WIC impacts on monthly food expenditures; the degree to which WIC benefits substituted for foods that would have been purchased anyway; and the degree to which WIC benefits were shared with unintended family members. These researchers concluded that participation in the WIC program by a pregnant woman has a more significant impact on the type of foods households purchase than on how much is spent. Participation by a child, on the other hand, was reported to have a positive effect on both food expenditures and food purchases.

It is difficult to draw conclusions about WIC impacts on household food expenditures given the two divergent conclusions drawn from the same data set. However, Rush and his colleagues make a compelling argument that limitations of both the sample drawn for this substudy and the data collected make it difficult to have confidence in the findings from any analysis (Rush et al., 1988b).

Summary

The preceding discussion clearly illustrates that an extensive amount of research has been conducted on the WIC program. At the same time, it demonstrates that coverage of the five different participant groups is very uneven in the existing research and that important gaps remain in information about potential program impacts.

Research is most extensive in the area of birth outcomes. Although concerns about self selection persist, the sheer volume of studies that have reported significant impacts—in different subgroups of WIC participants, at different points in time, and using different research designs and analysis methods—suggests that WIC does have a positive impact on birthweight, as well as a number of other birth-related outcomes, and

[74]Fraker, Long, and Post (1990) attempted to study the impact of WIC and FSP participation on household food expenditures using data from the 1985 CSFII. The authors concluded that they could not reliably estimate the impact of WIC, however, because of small sample sizes, the apparent complexity of the relationship between WIC participation and food expenditures, and a lack of relevant variables for use in modeling. There was evidence that the effect of the WIC program on household expenditures may vary, depending on the number and type of WIC participants in the household.

[75]WIC vouchers and checks specify particular types and amounts of food that can be purchased but do not include information on currency value (like food stamps do). Therefore, food expenditures estimated by simple recall are more prone to underestimation (devaluing) than those estimated by the diary method where every item is reported and valued individually. Moreover, WIC participants may be less aware of the actual cost of WIC food items than their nonparticipant counterparts.

significantly lowers birth-related Medicaid costs. Because of the design characteristics that contribute to inherent underestimation or overestimation of WIC impacts and the wide range of reported estimates, characterizing the relative size of WIC's impact with any confidence is difficult (for example, the estimated reduction in the prevalence of low-birthweight infants). Moreover, subgroup analyses completed by some researchers suggest that WIC impacts are likely to be greatest among Blacks and the lowest income women, groups with the highest incidence of low birthweight.

In addition, many important changes have taken place since most of the available research was conducted. These changes may influence the extent to which findings from previous research apply to the WIC program as it operates today. Some of the most noteworthy changes include: a substantially higher level of program penetration in most areas of the United States than was present in the mid- to late 1980s when most of the research was completed (that is, most eligible prenatal applicants are able to enroll in the program); more generous Medicaid income-eligibility criteria for pregnant women (including some that exceed the WIC cutoff of 185 percent of poverty), which infers automatic income-eligibility for WIC; and the use of standardized nutritional risk criteria. Furthermore, welfare reform legislation, which did not affect WIC directly, may have affected the circumstances of both WIC participants and nonparticipants. Any of these changes may influence both the presence and size of WIC impacts, as well as variations in impacts across subgroups.

In an ideal world, USDA would be able to periodically complete studies like the WIC/Medicaid study *at the national level*. The seeds for such an undertaking have been planted. The 2003 revisions to the U.S. Standard Certificate of Live Birth include the addition of an item to collect information about WIC participation during pregnancy.[76] Some States already collect this information and the hope is that all States will do so by 2009 (Sondik, 2003). Until data are available nationwide, an updated WIC/Medicaid study that uses data from States that do include WIC information on birth certificates is an attractive option.

Research on WIC impacts on pregnant women (other than impacts on birth outcomes) is scarce and relatively dated. Even less is known about impacts on breastfeeding and nonbreastfeeding postpartum women. Exploration of impacts on postpartum participants seems especially important to pursue. The limited available research suggests that postpartum WIC participation may be associated with improved birth outcomes in the subsequent pregnancy and with improved nutrition, health, and/or weight status for the women. If these relationships are confirmed through more definitive research, there may be reason to rethink the lower priority assigned to nonbreastfeeding postpartum women. In view of the ongoing obesity epidemic, the potential for WIC to play a role in addressing pregnancy-related weight retention, which seems to be especially prevalent among minority women (Gore et al., 2003; Abrams et al., 2000), seems particularly important.

There is no solid evidence about the impact of WIC on the initiation and duration of breastfeeding. Moreover, most of the studies that are available were completed prior to a considerable expansion of breastfeeding promotion efforts in the WIC program. Some early studies suggested that WIC participants may follow recommended infant feeding guidelines more closely than nonparticipants, delaying introduction of cow's milk until infants are 1 year and delaying the introduction of solid foods until 4-6 months. However, recent descriptive studies raise doubts about whether these differences persist today. An updated study of WIC's impacts on infant feeding practices would fill these important information gaps. Attention to selection-bias issues will be especially critical for such a study.

Finally, although recent research has begun to fill an information gap that existed for many years, the basis is small for drawing definitive conclusions about WIC's short- and long-term impacts on infants and children, the majority participant groups. The evidence is fairly strong that WIC improves children's iron status. Recent studies suggest that WIC participation positively affects children's use of health care services, immunization status, and overall health, however, potential selection bias remains a concern. Little is known about the impact of WIC on children's long-term health and development. Moreover, while evidence is convincing that WIC participation increases children's intakes of selected nutrients, the influence of these increases on the extent to which WIC children consume adequate diets is not clear. An updated version of the WIC Child Impact Study planned some years ago seems overdue.

[76]The item reads, "Did mother get WIC food for herself during this pregnancy?" Revised birth certificate available at www.cdc.gov/nchs/data/dvs/birth11-03final-ACC.pdf. Accessed June 2004.

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Chapter 5

National School Lunch Program

The National School Lunch Program (NSLP) is the oldest and second-largest food and nutrition assistance program (FANP) in the U.S. Department of Agriculture (USDA) nutrition safety net. Targeted specifically to school-age children, the NSLP is the cornerstone of the largely school-based child nutrition programs, which also include the School Breakfast Program (SBP), the Child and Adult Care Food Program (CACFP), the Summer Food Service Program (SFSP), and the Special Milk Program (SMP).

Schools that participate in the NSLP receive Federal reimbursement for each program meal served to students. USDA does not reimburse schools for adult meals, second meals, and a la carte items (including extra servings of components of program meals). Since 1998, the program has also covered snacks served to children in after-school programs. Any child in a participating school is eligible to receive NSLP meals; in FY 2002, more than 28 million children participated on an average school day. The program served more than 4.7 billion lunches and 123 million after-school snacks, at a cost of $6.9 billion (USDA, Food and Nutrition Service (FNS), 2003a).

Program Overview

The NSLP was established in 1946 to "safeguard the health and well-being of the Nation's children and to encourage the domestic consumption of nutritious agricultural commodities and other foods...."[77] A major impetus for the program was the prevalence of nutrition-related health problems identified during the screening of young men for military service in World War II.

Today, almost 99 percent of public schools and 83 percent of all public and private schools combined participate in the NSLP. Nationally, the program is available to about 92 percent of all students (Burghardt et al., 1993; Burghardt and Devaney, 1995). On an average school day, about 60 percent of children in schools that offer the NSLP participate in the program (Fox et al., 2001). Participation varies with household income, age, and gender. For example, studies have shown that students certified to receive free or reduced-price lunches are more likely to participate than students who are not certified for meal benefits, elementary school students are more likely to participate than secondary school students, and males are more likely to participate than females (Fox et al., 2001; Gleason, 1996; Maurer, 1984; Akin, 1983a).

[77]National School Lunch Act of 1946, Public Law 79-396.

Since 1998, when the NSLP was expanded to include after-school snacks, this component of the program has been growing steadily. Between FY 2000 and FY 2002, the number of after-school snacks provided through the NSLP increased from 70 million to 123 million (USDA/FNS, 2003a).[78]

The NSLP is administered by the Food and Nutrition Service (FNS) and its regional offices. At the State level, the program is administered by State agencies, most often departments of education. State agencies oversee Federal reimbursements, provide technical assistance, and monitor program performance. At the local level, the program is operated by school food authorities (SFAs). Most SFAs are individual school districts; however, regional school unions and residential childcare institutions also serve as SFAs.

Federal Subsidies

Participating SFAs receive two types of Federal assistance: cash reimbursements and commodities. Cash reimbursements are based on the number of lunches and after-school snacks served, established reimbursement rates, and the poverty level of participating students. A cash subsidy is provided for every program lunch and snack served. Additional cash subsidies are provided for meals and snacks served to children who qualify for free or reduced-price meal benefits. Currently, students eligible for free lunches and snacks are those from families with incomes at or below 130 percent of the Federal poverty level. Students from families with incomes between 130 and 185 percent of poverty are eligible to receive reduced-price lunches and snacks.[79]

[78]To be eligible to receive reimbursement for after-school snacks, school districts must participate in the NSLP and must sponsor or operate an after-school program that provides children with regularly scheduled educational or enrichment activities in a supervised environment (USDA/FNS, 2003b).

[79]Federal regulations allow schools that operate in high-poverty areas (areas where 50 percent or more of school-age children are eligible for free or reduced-price meals) to receive the "free" reimbursement rate for all after-school snacks, regardless of the students' family incomes.

Basic cash reimbursement rates for the 2002-03 school year were $2.14 for free lunches, $1.74 for reduced-price lunches, and $0.20 for lunches served to children who purchased meals at the full price (referred to as "paid meals").[80] Snacks were reimbursed at rates of $0.58, $0.29, and $0.05, respectively.

Children eligible for reduced-price lunches cannot be charged more than $0.40 per lunch. SFAs set their own prices for full-price/paid lunches, but must operate their school meal service program on a non-profit basis (USDA/FNS, 2003c). Of the 4.7 billion lunches served in FY 2002, 48 percent were served to children eligible for free meals and 9 percent to children eligible for reduced-price meals (USDA/FNS, 2003a).[81]

Schools receive agricultural commodities on an entitlement basis and may also receive bonus commodities. Entitlement commodity assistance is based on the number of reimbursable lunches served the previous school year. For the 2002-03 school year, the cash value of entitlement commodities was $0.1525 per meal (USDA/FNS, 2003c). Schools may elect to receive cash in lieu of commodity foods. In addition to entitlement commodities, schools may request bonus commodities—commodities that become available through agricultural surplus—in amounts that can be used without waste. The types and amounts of bonus commodities available vary from year to year depending on purchasing decisions made by USDA.

Nutrition Standards

To be eligible for Federal subsidies, NSLP meals must meet defined nutrition standards. Program regulations have long stipulated that lunches should provide one-third of the children's Recommended Dietary Allowances (RDAs). To ensure that these standards are met, program regulations have historically included food-based menu planning guidelines. These guidelines, originally known as the "Type A meal pattern," define specific types of food to be offered as well as minimum acceptable portion sizes.

[80]Reimbursement rates for both lunches and snacks are higher in Hawaii and Alaska. In addition, lunch reimbursement rates are higher for schools that operate in high-poverty areas (60 percent or more of students eligible for free or reduced-price meals).

[81]Information on the percentage of after-school snacks served to children eligible for free or reduced-price meal benefits is not available in publicly available summaries of administrative data.

The components of the traditional NSLP meal pattern are:

- Meat or meat alternate: 1 serving per meal
- Vegetables, fruits, and/or full-strength juices: 2 or more servings per meal
- Grains/breads: 1 or more servings per meal/8 servings per week
- Milk: 1 serving per meal.

Over the years, research has shown that, with few exceptions, the meals offered in the NSLP provide students the opportunity to satisfy one-third of their daily needs for food energy and an array of essential vitamins and minerals (Burghardt et al., 1993; Wellisch et al., 1983).

In the early 1990s, however, USDA's first School Nutrition Dietary Assessment Study (SNDA-I) examined the nutrient content of school lunches in comparison with recommendations included in the *Dietary Guidelines for Americans* (USDA and U.S. Department of Health and Human Services, 1990) and the National Research Council's *Diet and Health* report (National Research Council, 1989). SNDA-I found that, in comparison with these guidelines, NSLP meals were high in fat, saturated fat, and sodium, and low in carbohydrates (Burghardt et al., 1993). At the time the SNDA-I data were collected (the 1991-92 school year), schools were not required to offer meals that were consistent with these guidelines.

The School Meals Initiative for Healthy Children

In response to the SNDA-I findings, USDA made a commitment to implement the *Dietary Guidelines* in the NSLP. The embodiment of this commitment is the School Meals Initiative for Healthy Children (SMI). The SMI, launched in 1995, is designed to improve the nutritional quality of school meals by providing schools with educational and technical resources that can be used to (1) assist foodservice personnel in preparing nutritious and appealing meals and (2) encourage children to eat more healthful meals. Key components of the SMI include revised nutrition standards for school meals, a major restructuring of menu planning requirements, and a broad-based nutrition education program.

The nutrition standards established under the SMI maintain the longstanding goal of providing one-third of students' daily needs for food energy and nutrients. In addition, the standards include goals for fat and saturated fat content that are consistent with the *Dietary Guidelines* recommendations.[82] The Healthy Meals for Healthy Americans Act (P.L. 103-448) formally required that school meals be consistent with the *Dietary Guidelines* and that schools begin complying with SMI nutrition standards in the 1996-97 school year unless a waiver was granted by the relevant State agency. The regulatory requirement that school meals be consistent with the *Dietary Guidelines* has been incorporated into the FNS strategic plan. The current goal is for all schools to satisfy these standards by 2005 (USDA/FNS, 2000a).

Under SMI and the Healthy Meals for Healthy Americans Act, menu planning requirements were restructured to offer schools several alternatives to the traditional food-based NSLP meal pattern. These include a computer-based menu planning approach known as Nutrient Standard Menu Planning (NSMP). NSMP focuses on the nutrient content of meals rather than the specific types of food offered. School districts may implement NSMP on their own or may contract with an outside agency. An enhanced food-based meal pattern was also developed. This is similar to the traditional pattern but requires more servings of breads and grain products over the course of a week and larger servings of fruits and vegetables. School districts may also use any other reasonable approach to menu planning, subject to State agency guidelines. Implementation of new menu planning systems has proved to be a lengthy process. In the 1999-2000 school year, only 63 percent of all SFAs reported that they had “fully implemented” the menu planning system of their choice (Abraham et al., 2002). Eighty-five percent indicated that their plans were at least three-quarters implemented.

The nutrition education component of the SMI is the Team Nutrition Initiative (TN) (see chapter 16). TN provides technical assistance, educational resources, and training to school foodservice personnel, children, parents, teachers, and school administrators. TN uses behavior-oriented strategies to (1) assist school foodservice personnel in preparing and serving meals that meet the SMI nutrition standards without sacrificing taste or attractiveness, (2) promote healthful eating habits and regular physical activity among both children and parents, and (3) build a support base among school administrators and other school and community partners for healthy patterns of eating and physical activity (USDA/FNS, 2002).

[82]Goals for sodium and cholesterol content are not included in SMI nutrition standards. However, schools are encouraged to monitor levels of these dietary components.

Related Program Changes

The SMI has been supported by several parallel initiatives. For example, considerable effort has been devoted to improving the nutrient profile of the commodity foods provided to NSLP schools (Buzby and Guthrie, 2002). In addition, under the Nutrition Title of the 2002 Farm Act, USDA received $6 million for a pilot program to provide fresh and dried fruits and fresh vegetables to children in elementary and secondary schools. The pilot program, which was implemented in the 2002-03 school year, was very well received (Buzby et al., 2003) and was expanded under the Child Nutrition and WIC Reauthorization Act of 2004 (P.L. 108-265).

Most recently, policymakers have begun to focus on the “school nutrition environment” (Ralston et al., 2003; American School Food Service Association (ASFSA), 2003; USDA/FNS, 2000b). A school’s nutrition environment includes the nutritional quality of reimbursable school meals, the availability and nutritional quality of competitive (non-NSLP) foods, meal scheduling, physical characteristics of the cafeteria, nutrition education and marketing activities, and the school’s commitment to nutrition and physical activity.

Major attention has been focused on the issue of “competitive foods” in school meal programs—foods other than those included in NSLP and SBP meals (USDA/FNCS, 2001). In 1998-99, USDA’s *second* School Nutrition Dietary Assessment Study (SNDA-II) found that more than half of all elementary schools had a la carte programs that offered items other than milk, juice, and desserts (Fox et al., 2001). The same was true of roughly 90 percent of middle schools and high schools. Many schools had a la carte programs that made it possible for students to purchase complete meals on an a la carte basis. SNDA-II also demonstrated that revenue from a la carte sales was inversely related to rates of student participation in the NSLP.

The CDC-sponsored School Health Policies and Programs Study (SHPPS) found that more than a quarter of elementary schools, 62 percent of middle/junior high schools, and 95 percent of senior high schools had vending machines (Wechsler et al., 2001). A substantial number of schools also had school stores, canteens, or snack bars available to students during meal time. Some school districts have entered into potentially lucrative “pouring rights” contracts that may lead to increased availability and marketing of soft drinks (Lin and Ralston, 2003; Nestle, 2000).

Although not part of the reimbursable NSLP meal, competitive foods contribute to students' in-school dietary intake. Currently, there are no Federal nutrition-related standards governing competitive foods, and research has shown that foods offered through these alternative sources tend to be high in fat, sodium, and/or sugar (French et al., 2003; Kubik et al., 2003; Zive et al., 2002; USDA/FNCS, 2001; Wechsler et al., 2001; Glengdahl and Seaborn, 1999). Concerns about the negative impact of competitive foods has prompted calls for action at local, State, and Federal levels to limit their availability and/or to establish nutrition standards for them.

Implications for Interpreting Available Research

The vast majority of the research reviewed in this chapter is based on data that were collected *before* the SMI was launched or in the very early stages of its implementation. This includes all of the studies that were national in scope, those with the strongest designs and analysis methods, and all of the studies that looked at impacts on students' dietary intakes over 24 hours.

Given the nature and extent of the changes associated with the SMI—changes that specifically targeted the nutrient content of school lunches and students' consumption of healthful lunches—it is important that results of the available research be interpreted in the proper context. Existing research provides a comprehensive picture of past and potential impacts of the NSLP; however, because of the major changes that have been implemented under SMI and related ongoing changes, it cannot be assumed that these findings apply to today's NSLP.

Indeed, there is evidence that the nutrient content of meals offered in the NSLP has changed since the implementation of the SMI. The SNDA-II study found that, relative to lunches offered in 1991-92 (as reported in SNDA-I), lunches offered in 1998-99 were significantly lower in total fat, saturated fat, and sodium (although, on average, lunches continued to exceed *Dietary Guidelines* and NRC recommendations for those nutrients). Moreover, SNDA-II demonstrated that reductions in fat and saturated fat content could be achieved without sacrificing overall nutrient content. That is, although lower in fat, NSLP lunches continued to meet the goal of providing one-third of the RDAs for key nutrients.

The SNDA-II data were collected relatively early in the implementation of the SMI and, since that time, efforts to implement the SMI nutrition standards have continued at the Federal, State, and local levels. Consequently, even these relatively recent data may not provide an accurate picture of the nutrient content of meals currently offered in the NSLP.

It is important to keep the changing nature of the NSLP in mind when reviewing the summary of NSLP research that follows. The existing research provides information on previous and potential impacts of the NSLP; however, new research is essential to understanding the impact of the NSLP as it operates today (Guthrie, 2003).

Research Overview

The literature search identified a total of 26 studies that examined the impact of the NSLP on nutrition- and health-related outcomes of participating children. Among these are two USDA-sponsored national evaluations that included student-level outcomes: the National Evaluation of School Nutrition Programs (NESNP) conducted in 1980-81 and the SNDA-I study, conducted in 1991-92. A third USDA-sponsored national evaluation, the SNDA-II study, conducted in the 1998-99 school year, is not included in this summary because it did not collect student-level data.

Most studies examined impacts on dietary intake at lunch and/or over 24 hours. A smaller number considered impacts of NSLP participation on other measures of nutrition and health status or on household food expenditures. One study also looked at impacts on school attendance and cognitive performance.

The majority of the available research is quite dated. Fifteen of the 26 studies used data that were collected during or prior to the early 1980s. And, as noted, almost all studies used data that were collected prior to implementation of the SMI.

Measures of Participation

Measures of program participation varied across studies (and sometimes within studies, depending on the outcome being evaluated). Most studies equated the purchase or consumption of a school lunch on the day(s) of dietary assessment with program participation. This entails some risk that children who usually eat a school lunch did not do so on the day of the survey, or that some who ate the school lunch on that day usually do not participate. However, NESNP researchers evaluated the extent of this problem and concluded that defining participation on the basis of one day's behavior gave an accurate picture of participation, at least with the large sample available in that study and with data collected on all 5 weekdays (Wellisch et al., 1983).

Several studies defined program participation on the basis of usual NSLP participation practices, such as whether a student usually ate a school lunch a minimum number of times per week or the proportion of potential lunches eaten during the study period. In evaluating the impact of the NSLP on students' linear growth, the NESNP used historical information on students' participation from grade 1 through the current grade, and computed an average weekly NSLP participation rate for each student (Wellisch et al., 1983).

Definition of nonparticipant comparison groups also differed across studies. Most researchers used nonparticipants in the same schools as NSLP participants. Hoagland (1980) distinguished between students who did and did not have the NSLP available to them. The NESNP oversampled schools that did not offer the NSLP. However, investigators ultimately concluded that schools not offering the NSLP did not constitute an appropriate comparison group, so they used nonparticipants in NSLP schools instead.

The following two sections summarize major findings from existing research on the impact of the NSLP on nutrition- and health-related outcomes. The first section summarizes studies that assessed impacts on students' dietary intake. The second section discusses studies that examined impacts on other nutrition and health outcomes, including weight and height, nutritional biochemistries, household food expenditures, and school performance.

Impacts on Dietary Intake

Estimates of NSLP impacts on dietary intake are subject to the limitations discussed in chapter 2. Most studies used data for a single day or meal, and therefore provide weak estimates of individuals' usual intake. Some studies used multiple days of data or other means (such as a food frequency checklist) to better capture usual intake. However, none of the available studies used the approach to estimating usual intake that was recently recommended by the Institute of Medicine (IOM, 2001).[83]

Similarly, in assessing intakes of food energy, vitamins, and minerals, researchers generally compared mean intakes of participants and nonparticipants or compared the proportion of individuals in each group with intakes below a defined cutoff. Again, none used the approach recommended by the IOM, which calls for use of data on usual intake in conjunction with defined Estimated Average Requirements (EARs) (IOM, 2001).

Consequently, the available research presents an imperfect picture of the substantive significance of differences observed in the dietary intakes of NSLP participants and nonparticipants. It provides information on whether NSLP participants consumed more or less energy and nutrients than nonparticipants. However, this information cannot be used to draw conclusions about whether NSLP participants were more or less likely than nonparticipants to have adequate intakes.

In addition, research has shown that assessing the dietary intakes of children presents unique challenges (Medlin and Skinner, 1988; Baranowski and Simons-Morton, 1991) and that recall-based data collection methodologies do not necessarily work well with young children (Baxter, 2000 and 2003). As a result, recall-based measures of the dietary intakes of NSLP participants and nonparticipants are subject to increased measurement error.

Research Overview

The literature search identified 19 studies that examined the impact of NSLP participation on students' dietary intakes. Characteristics of these studies are summarized in table 26. Most studies examined impacts on intake of food energy and nutrients. However, three studies (Devaney et al., 1993; Gleason and Suitor, 2001; Rainville, 2001) looked at impacts on food consumption as well as energy and nutrient intake, and four studies (Cullen et al., 2000; Melnick et al.,1998; Wolfe and Campbell, 1993; Yperman and Vermeersh, 1979) looked only at impacts on food consumption. Five studies focused exclusively on impacts on dietary intake at lunch, seven studies looked at both lunch and 24-hour intakes, and seven studies focused exclusively on 24-hour intake.

The available research can be divided into three groups. Group I includes the two national evaluations that examined student-level outcomes: SNDA-I (1991-92 school year) and NESNP (1980-81 school year). Group II includes five studies that are based on secondary analysis of data from national cross-sectional surveys. Two of these studies (Gleason and Suitor, 2001 and 2003) used data collected between 1994 and 1996. The other three studies in this group are based on data from the late 1970s or early 1990s. Group III consists of 11

[83] Gleason and Suitor (2001) used these methods to describe dietary intakes of U.S. children. However, in assessing differences in intakes of NSLP participants and nonparticipants, they used regression-adjusted mean intakes that were based on 1 or 2 days of data.

Table 26—Studies that examined the impact of the National School Lunch Program on students' dietary intakes

Study	Outcome(s)	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group I: National evaluations							
Devaney et al. (1993) (SNDA-I)	Nutrient intake at lunch and over 24 hours Food intake at lunch	Nationally representative sample of students from 329 public and private schools (1991-92)	Single 24-hour recall	Children and adolescents in grades 1-12 (n~3,350)	Participant vs. nonparticipant	Ate NSLP lunch on recall day	Multivariate regression with selection-bias-adjustment (nutrients) Bivariate t-tests (foods)
Wellisch et al. (1983) (NESNP)	Nutrient intake at lunch and over 24 hours	Nationally representative sample of students from 276 public schools (1980-81)	Single 24-hour recall	Children and adolescents in grades 1-12 (n=6,556)	Participant vs. nonparticipant	Ate NSLP lunch on recall day	Multivariate regression
Group II: Secondary analysis of national surveys							
Gleason and Suitor (2003)	Nutrient intake at lunch and over 24 hours	1994-96 CSFII	2 nonconsecutive 24-hour recalls	Children and adolescents ages 6-18 with 2 days of intake data (n=1,614)	Participant vs. nonparticipant	Ate NSLP lunch on recall day	Multivariate regression with fixed-effects model to control for selection bias
Gleason and Suitor (2001)	Nutrient intake at lunch and over 24 hours Food intake at lunch and over 24 hours	1994-96 CSFII	2 nonconsecutive 24-hour recalls	Children and adolescents ages 6-18 with 1 or 2 school days of intake data (n=1,866)	Participant vs. nonparticipant	Ate NSLP lunch on recall day	Comparison of regression-adjusted means
Fraker (1987)	Nutrient intake at lunch and over 24 hours	1980-81 NESNP	Single 24-hour recall	Children and adolescents in grades 1-12 (n=6,556)	Participant vs. nonparticipant	Ate NSLP lunch on recall day	Bivariate t-tests for full sample and low-income sample

See notes at end of table.

Continued—

Table 26—Studies that examined the impact of the National School Lunch Program on students' dietary intakes—Continued

Study	Outcome(s)	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Akin et al. (1983a)	Nutrient intake over 24 hours	1977-78 NFCS	24-hour recall plus 2-day food record	Children and adolescents ages 6-18 (n=1,554)	Participant vs. nonparticipant[2,3]	Ratio of number of days ate school lunch to number of days of dietary data	Multivariate regression
Akin et al. (1983b)	Nutrient intake over 24 hours	1977-78 NFCS	24-hour recall plus 2-day food record	Children and adolescents ages 6-18 (n=1,554)	Participant vs. nonparticipant[4]	Ratio of number of days ate school lunch to number of days ate any lunch	Switching regression; Chow tests
Hoagland (1980)	Nutrient intake over 24 hours	1971-74 NHANES-I	Single 24-hour recall	Children and adolescents ages 6-21 (n=3,155)	Participant vs. nonparticipant[2]	Ate school lunch on recall day	Analysis of variance
Group IIIA: State and local studies with large samples							
Rainville (2001)	Nutrient intake at lunch Food intake at lunch	Students in 10 schools in southeastern Michigan (1998)	Visual observation of food selection and waste	Children in grades 2-4 (n=570)	Participant vs. nonparticipant	Ate school lunch on observation day (vs. sack lunch)	Analysis of variance
Melnick et al. (1998)	Food intake over 24 hours	All students in randomly selected classrooms in 25 sampled public and private schools in New York City (1989-90)	Single 24-hour recall (nonquantitative)	Children in grades 2 and 5 (n=1,397)	Participant vs. nonparticipant[2]	Ate school lunch on recall day	Gender-adjusted anlaysis of covariance
Wolfe and Campbell (1993)	Food intake at lunch	Students in 51 schools in New York State, excluding New York City (1987-88)	Single 24-hour recall (nonquantitative)	Children in grades 2 and 5 (n=1,797)	Participant vs. nonparticipant	Ate school lunch on recall day	Bivariate t-tests and chi-square tests

See notes at end of table.

Continued—

Table 26—Studies that examined the impact of the National School Lunch Program on students' dietary intakes—Continued

Study	Outcome(s)	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Price et al. (1978)	Nutrient intake over 24 hours	Students in schools/districts in 8 regions in Washington State, Blacks and Mexican-Americans were oversampled (1971-73)	3 nonconsecutive 24-hour recalls, including 1 weekend day	Children ages 8-12 (n=728)	Participant vs. nonparticipant	Participation dummies based on usual frequency: 0-1 time per week, 2-3 times per week, 4-5 times per week	Multivariate regression
Emmons et al. (1972)	Nutrient intake at lunch and over 24 hours	All students in selected grades in 1 district in rural New York State (1970-71)[5]	Single 24-hour recall	Children in grades 1-4 (n=512)	Participants, before vs. after[6]	Took 70% or more of school meals offered during study period	Comparison of means (type of statistical test not reported)
U.S. Department of Health, Education, and Welfare (HEW) (10-State Nutrition Survey)	Nutrient intake over 24 hours	Sample of children from 10 States, plus volunteers (1972)	Single 24-hour recall	Children and adolescents ages 10-16 (n=8,495)	Participant vs. nonparticipant[2]	Usually ate school lunch at least 3 times/week	Comparison of means (no statistical tests reported)
Group IIIB: State and local studies with small samples							
Cullen et al. (2000)	Food intake at lunch	Students in 1 middle school in Texas (dates not reported)	5 consecutive daily food records	Children in grade 5 (n=282)	Participant vs. nonparticipant	Ate NSLP lunch (vs. home lunch or snack bar lunch) on food record days	Analysis of variance
Ho et al. (1991)	Nutrient intake at lunch	Students in 1 middle school in Salt Lake City (1989)	Visual observation of food selection and waste	Children and adolescents in grades 7 and 8 (n=254)	Participant vs. nonparticipant	Ate NSLP lunch (vs. sack lunch or vending machine lunch) on observation day	Analysis of variance and Student-Newman-Keuls range test
Perry et al. (1984)	Nutrient intake at lunch	All students in selected classrooms in 3 schools in 1 district in Alabama	3-day food record	Children in grades 5 and 6 (n=233)	Participant vs. nonparticipant[7]	Ate NSLP lunch (vs. brown bag lunch) on food record days	Unmatched t-test

See notes at end of table. Continued—

Table 26—Studies that examined the impact of the National School Lunch Program on students' dietary intakes—Continued

Study	Outcome(s)	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Howe and Vaden (1980)	Nutrient intake at lunch and over 24 hours	Randomly selected students in 1 urban public high school in Kansas	Single 24-hour recall	Adolescents in grades 10 and 11 (n=104)	Participant vs. nonparticipant	Ate NSLP lunch on recall day	2-way analysis of variance
Yperman and Vermeersch (1979)	Food intake over 24 hours	All students in 2 classrooms per grade in 2 schools in California	Food frequency checklist	Children in grades 1-3 (n=307)	Participant vs. nonparticipant	Number of days ate school lunch on 5 days prior to data collection	Multivariate regression

[1] Data sources:
CSFII = Continuing Survey of Food Intakes by Individuals.
NHANES-I = First National Health and Nutrition Examination Survey.
NFCS = Nationwide Food Consumption Survey.
[2] Did not differentiate NLSP and other lunch programs.
[3] Included lunch skippers with nonparticipants.
[4] Accounted for lunch skippers.
[5] Study included a second district where both free lunch and free breakfast were offered. The two districts were considered separately in the analysis, but the analysis of the second district did not separate contributions of breakfast and lunch meals.
[6] Study compared intakes before and after introduction of a free lunch program. Results were reported for four different subgroups based on baseline characteristics: nutritionally adequate, nutritionally needy, low-income (eligible for free lunch), and not low-income.
[7] Unit of analysis was lunches rather than students; 60 percent of students ate NSLP daily.

State and local studies. Six of these studies (Group III-A) included relatively large numbers of children (more than 500) from multiple sites—schools, SFAs, or States. The remaining five studies (Group III-B) had substantially smaller samples and generally weaker designs. With the exception of studies by Rainville (2001) and Cullen et al. (2000), all of the Group III studies are based on data collected before the implementation of the SMI. Six are based on data from the mid-1980s or earlier.

The strongest evidence about the impact of the NSLP on the dietary intake of participating students comes from the SNDA-I study (Devaney et al., 1993) and an analysis by Gleason and Suitor (2003) that used data from the 1994-96 Continuing Survey of Food Intakes by Individuals (CSFII). SNDA-I is the most recent, comprehensive, and state-of-the-art study designed specifically to study the NSLP. It included nationally representative samples of public and private schools and of students attending those schools. Information, including a single 24-hour recall, was collected for 3,350 school-age children in 329 schools.

SNDA-I used a participant vs. nonparticipant design. However, Devaney and her colleagues used an instrumental variables approach to control for selection bias. The authors confirmed the robustness of their results using a variety of specifications. The model used to estimate impacts on dietary intake at lunch controlled for the price charged for a full-price lunch; student status with regard to free and reduced-price meal benefits; interaction terms for the price of a full-price lunch and benefit eligibility categories; availability of offer-vs.-serve;[84] ability to leave school for lunch; availability of low-, moderate-, and high-fat lunches;, and serving capacity of the lunch room. In estimating impacts on 24-hour nutrient intake, researchers adjusted for self-selection into the NSLP but not into the SBP because they concluded, based on exploratory analyses, that there was no selection bias in breakfast intakes.

Selection-bias adjustments are not without problems and frequently produce implausible results (see discussion in chapter 4). SNDA-I analysts, however, had access to many relevant variables and their findings, including differences between selection-adjusted and unadjusted results, make intuitive sense. Others reviewing the same literature (see, for example, Rossi, 1998 and Devaney et al., 1997) have reached the same conclusion.

A more recent study by Gleason and Suitor (2003) used data from the 1994-96 wave of the CSFII. This study improved upon the SNDA-I analysis by using a fixed-effects model to control for selection bias. The analysis included 1,614 children who (1) attended schools where the NSLP was offered and (2) had 2 days of intake data, at least one of which was a school day. The fixed-effects model was estimated in a paired-differences form, where differences between the 2 days of intake data were regressed on corresponding differences in student characteristics, including NSLP participation status. Thus, the estimation of NSLP impacts was based on variation in NSLP participation status of specific individual students rather than on variation in participation status of different groups of students. This ensured that the estimate was not influenced by unmeasured differences that may have existed between different groups of students.

The analysis included both students who reported intake for 2 school days and those who reported intake for 1 school day and 1 non-school day. To control for the possibility that students' intakes varied on school and non-school days for reasons other than the NSLP, the model included a dummy variable that indicated whether the intake day was a school day. The model also attempted to control for potential unobserved differences that may have had varying influences on children's consumption behaviors on the 2 days. For example, it included the day of the week, the number of hours of television watched on the intake day, two variables that indicated frequency of exercise, and variables that indicated whether reported intakes were heavier or lighter than usual.

An earlier study by Gleason and Suitor (2001) also used the 1994-96 CSFII. However, that study did not attempt to control for selection bias. The authors raised appropriate concerns about likely selection bias and cautioned that estimates of differences between NSLP participants and nonparticipants observed in that analysis should not be interpreted as valid estimates of NSLP impacts.

Although SNDA-I (Devaney et al., 1993) and the most recent study by Gleason and Suitor (2003) provide the strongest available data on NSLP impacts, both studies

[84] Offer vs. serve (OVS) is a NSLP policy that allows students to refuse some of the foods offered to them in reimbursable school lunches. At the time the SNDA-I data were collected, OVS was mandatory for secondary schools and was optional, at the discretion of local authorities, for elementary schools. Under OVS, students could refuse two of the three meal components in the traditional food-based meal pattern that was in effect at the time.

are based on data collected prior to the SMI. The literature search identified only two studies that compared dietary intakes of NSLP participants and nonparticipants using data collected sometime after the SMI was implemented (Rainville, 2001; Cullen et al., 2000).[85] Rainville looked at both food and nutrient consumption at lunch, comparing intakes of students who ate NSLP lunches and students who ate lunches from home. The study by Cullen et al. looked only at consumption of fruits and vegetables at lunch, comparing contributions of NSLP lunches and snack-bar lunches. Both of these studies were local in scope and both have substantial methodological limitations relative to SNDA-I and Gleason and Suitor (2003), particularly with regard to generalizability and selection bias. However, when viewed in concert with findings from SNDA-II, these more recent studies provide suggestive evidence of post-SMI impacts of the NSLP.

Impacts on Intake of Food Energy and Nutrients at Lunch

Nine studies examined the impact of NSLP participation on students' intake of food energy and nutrients at lunch. Results of these studies are summarized in table 27. The table is divided into four sections: food energy and macronutrients, vitamins, minerals, and other dietary components. The text follows this general organization, but combines findings for vitamins and minerals in one section.

In the interest of providing a comprehensive picture of the body of research, both significant and nonsignificant results are reported in table 27 and in all other "findings" tables. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned, however, to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies. In this case, emphasis is given to findings from SNDA-I (Devaney et al., 1993) and from the most recent Gleason and Suitor (2003) study, for the reasons discussed previously. All findings reported for SDNA-I are based on selection-bias-adjusted models, and all findings reported for Gleason and Suitor (2003) are based on fixed-effects models. For the most part, findings from the two studies are consistent. Where findings diverge, Gleason and Suitor's results are considered stronger because of the improved methods used to control for selection bias.

SNDA-I researchers stressed the importance of looking separately at NSLP effects by both age and gender. They pointed out, for example, that lunch options are usually more varied for older students and that these students typically make their own decisions about what to eat for lunch. Younger students, on the other hand, generally have fewer options and decisions about their lunches are often made by parents. Moreover, research has shown that adolescent females are more likely than males or younger children to consume diets low in nutrients relative to the RDAs.

In SNDA-I, selection-bias adjustments made little difference in conclusions about NSLP effects on younger children, but substantially affected the conclusions about older students, particularly females. SNDA-I conducted subgroup analysis by age and gender (6- to 10-year-olds, 11- to 18-year-old males, 11- to 18-year-old females) and by income (low-income and non-low-income (income greater than 185 percent of poverty)). In table 27, results of SNDA-I subgroup analyses are reported when estimates for one or more subgroups differed from results of the overall analysis *and* when the result of one of the analyses—the overall analysis or the subgroup analysis—revealed a statistically significant difference.

Food Energy and Macronutrients

Findings from SNDA-I (Devaney et al., 1993) and Gleason and Suitor (2003) suggest that, prior to the implementation of the SMI, NSLP participants and nonparticipants consumed roughly equivalent amounts of food energy at lunch. (Note that results are reported in table 27 using only the senior author's name and that SNDA-I results are reported as Devaney (1993).) Neither study found a significant difference in the energy intakes of NSLP participants and nonparticipants at lunch. Interestingly, however, both sets of researchers found that impact estimates that were *not adjusted for selection bias* showed that NSLP participants consumed significantly more food energy than nonparticipants (data not shown). Devaney and her colleagues attributed the difference between the two results to differences in unobserved characteristics that may affect participation, such as differences in appetite, food preferences, and food energy needs. These factors are controlled for in the selection-bias-adjusted

[85] Studies that examined the nutrient content of NSLP and non-NSLP meals as offered or served, but did not assess food and/or nutrient intakes of NSLP participants and nonparticipants, were not included in this review.

Table 27—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes at lunch

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Food energy and macronutrients				
Food energy	Gleason (2001) [national] Ho (1991) [1 school] Wellisch (1983) [national] Fraker (1987) [national] {females, 11-14}	Gleason (2003) [national] Howe (1980) [1 site] Fraker (1987) [national] {except subgroups noted}	Rainville (2001) [10 schools] Devaney (1993) [national] Fraker (1987) [national] {females, 5-10} Perry (1984) [3 schools]	Fraker (1987) [national] {males, 5-10}
Protein	Gleason (2003) [national] Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] {6-10; low-income} Ho (1991) [1 school] Fraker (1987) [national] {except subgroup noted} Wellisch (1983) [national] Howe (1980) [1 school]	Perry (1984) [1 site] Devaney (1993) [national] {except subgroups noted} Fraker (1987) [national] {females, 15-21}		
Carbohydrates		Ho (1991) [1 school][1]	Rainville (2001) [10 schools] Devaney (1993) [national] {11-18; females} Fraker (1987) [national] {males, 15-21}	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] {except subgroups noted} Fraker (1987) [national] {except subgroup noted}
Fat	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] {except subgroups noted} Ho (1991) [1 school]	Devaney (1993) [national] {11-18; low-income} Fraker (1987) [national] {except subgroup noted}	Rainville (2001) [10 schools] Fraker (1987) [national] {males, 15-21}	
Saturated fat	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] Ho (1991) [1 school][2]	Rainville (2001) [10 schools]		

See notes at end of table.

Continued—

Table 27—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes at lunch—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamins				
Vitamin A	Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] {except subgroup noted} Wellisch (1983) [national] Perry (1984) [3 schools] Howe (1980) [1 school]	Gleason (2003) [national] Devaney (1993) [national] {non-low-income} Ho (1991) [1 school][1]		
Vitamin B_6	Gleason (2001) [national] Rainville (2001) [10 schools] Wellisch (1983) [national]	Gleason (2003) [national] Devaney (1993) [national] {except subgroup noted}	Devaney (1993) [national] {non-low-income}	
Vitamin B_{12}	Gleason (2003) [national] Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] {except subgroups noted}	Devaney (1993) [national] {females, 11-18; non-low-income}		
Vitamin C	Howe (1980) [1 school]	Perry (1984) [3 schools]	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] {11-18}	Rainville (2001) [10 schools] Devaney (1993) [national] {except subgroup noted} Ho (1991) [1 school][2] Wellisch (1983) [national]
Vitamin D	Rainville (2001) [10 schools]			
Vitamin E		Gleason (2001) [national]	Gleason (2003) [national]	
Folate	Gleason (2001) [national] Rainville (2001) [10 schools]		Gleason (2003) [national] Devaney (1993) [national]	
Niacin	Rainville (2001) [10 schools] Wellisch (1983) [national]	Gleason (2001) [national] Howe (1980) [1 school]	Gleason (2003) [national] Devaney (1993) [national]	
Riboflavin	Gleason (2003) [national] Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] {except subgroups noted} Wellisch (1983) [national] Perry (1984) [3 schools] Howe (1980) [1 school]	Devaney (1993) [national] {females, 11-18; non-low-income}		

See notes at end of table.

Continued—

Table 27—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes at lunch—Continued

Outcome	Significant impact	No significant impact		Significant impact
	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Thiamin	Gleason (2001) [national] Rainville (2001) [10 schools] Wellisch (1983) [national] Howe (1980) [1 school]	Gleason (2003) [national] Perry (1984) [3 schools]	Devaney (1993) [national]	
Minerals				
Calcium	Gleason (2003) [national] Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] {except subgroup noted} Wellisch (1983) [national] Perry (1984) [3 schools] Howe (1980) [1 school]	Devaney (1993) [national] {females, 11-18}		
Iron	Gleason (2001) [national] Rainville (2001) [10 schools] Howe (1980) [1 school] Wellisch (1983) [national] {younger}	Gleason (2003) [national] Devaney (1993) [national] {except subgroup noted} Ho (1991) [1 school][1]	Devaney (1993) [national] {females, 11-18; non-low-income}	Wellisch (1983) [national] {older}
Magnesium	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] {except subgroups noted}[3] Wellisch (1983) [national]	Devaney (1993) [national] {6-10; 11-18; non-low-income}[3]		
Phosphorus	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] {except subgroups noted} Wellisch (1983) [national] Perry (1984) [3 schools]	Devaney (1993) [national] {11-18; non-low -income}		
Zinc	Gleason (2003) [national] Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] {except subgroups noted}	Devaney (1993) [national] {11-18; non-low -income}		

See notes at end of table.

Continued—

Table 27—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes at lunch—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Other dietary components				
Added sugars				Gleason (2003) [national] Gleason (2001) [national] Rainville (2001) [10 schools]
Cholesterol	Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] {6-10; low -income} Ho (1991) [1 school]	Gleason (2003) [national] Devaney (1993) [national] {except subgroups noted}	Devaney (1993) [national] {females, 11-18}	
Fiber	Gleason (2003) [national] Gleason (2001) [national] Rainville (2001) [10 schools]	Ho (1991) [1 school][1]		
Sodium	Gleason (2001) [national] Rainville (2001) [10 schools] Ho (1991) [1 school][2]	Gleason (2003) [national] Devaney (1993) [national] Fraker (1987) [national] {females, 15-21}	Fraker (1987) [national] {except subgroups noted}	Fraker (1987) [national] {females, 5-10}

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 3 schools). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

To maintain readability, results of SNDA-I (Devaney et al., 1993) subgroup analyses are presented only when results differed from results for overall sample *and* at least one of the analyses reported a statistically significant effect. All findings are from selection-bias-adjusted models.

[1] Results for NSLP vs. sack lunch. Difference between NSLP and vended lunch was in same direction and was statistically significant.

[2] Results for NSLP vs. sack lunch. Difference between NSLP and vended lunch was in same direction but was not statistically significant.

[3] In main analysis for overall sample, selection-bias-adjusted difference between participants and nonparticipants indicated that NSLP participants consumed significantly more magnesium than nonparticipants. This pattern was observed in all subgroup analyses; however, the differences were significant only among low-income students.

results. This may explain the significant differences in energy intake reported by other researchers whose analyses did not account for selection bias.

The available studies are largely consistent in finding that NSLP participants consumed significantly more protein at lunch than nonparticipants. SNDA-I did not find this effect in the overall analysis. However, subgroup analyses revealed a significant NSLP impact among 6- to 10-year-olds and among low-income children.

SNDA-I and Gleason and Suitor (2003) both found that lunches consumed by NSLP participants prior to the SMI were significantly lower in carbohydrates, as a percentage of total food energy, than lunches consumed by nonparticipants. SNDA-I subgroup analyses revealed that this pattern did not hold for females ages 11-18. Gleason and Suitor (2003) found that the difference in carbohydrate consumption was due to decreased consumption of added sugars among NSLP participants. Consumption of other forms of carbohydrates was essentially equivalent for the two groups.

Findings from SNDA-I and Gleason and Suitor (2003) are also consistent with regard to intakes of total fat and saturated fat. The data indicate that, prior to implementation of the SMI, lunches consumed by NSLP participants provided significantly more fat and saturated fat, as a percentage of total energy intake, than lunches consumed by nonparticipants. SNDA-I subgroup analyses revealed that the difference in intake of total fat was concentrated among 6- to 10-year-old and non-low-income students.

Findings from Rainville (2001), the only study in this group based on data collected after implementation of the SMI, paint a notably different picture. Rainville found no significant differences in the mean carbohydrates, fat, and saturated fat content of NSLP lunches and sack lunches consumed by elementary school students. These results suggest that the carbohydrate content of NSLP lunches has increased since the implementation of the SMI, while the fat and saturated fat content has decreased. This is consistent with the trend observed in SNDA-II (Fox et al., 2001).

However, Rainville's analysis did not adjust for selection bias, and factors other than selection bias may have contributed to their more positive findings. For example, Rainville included only 2^{nd}, 3^{rd}, and 4^{th} graders, while both SNDA-I and Gleason and Suitor (2003) included students in grades 1 through 12. SNDA-II found that lunches offered in elementary schools were lower in fat and saturated fat, on average, than lunches offered in secondary schools (the statistical significance of these differences was not tested). In addition, the schools included in Rainville's study, and the lunches they offered, may not be representative of lunches offered nationwide. The two volunteer school districts that participated in Rainville's study were relatively affluent—with 18 percent and 25 percent of students approved for free and reduced-price meals, respectively—and the reimbursable lunches offered in these districts provided even less fat (29.4 percent of total food energy) than required under the SMI (no more than 30 percent). By comparison, SNDA-II found that lunches offered in elementary schools provided an average of 33.5 percent of total food energy from fat.

Vitamins and Minerals

Data from SNDA-I (Devaney et al., 1993) and Gleason and Suitor (2003) suggest that prior to implementation of the SMI, NSLP participants had significantly greater lunch intakes of vitamin B_{12}, riboflavin, calcium, magnesium, phosphorus, and zinc than nonparticipants. Subgroup analyses conducted by Devaney and her colleagues revealed substantial variation in these results across subgroups. Most often, significant differences were concentrated among 6- to 10-year-olds and low-income students.

Findings from SNDA-I and Gleason and Suitor differ for vitamins A and C. SNDA-I found that NSLP participants consumed significantly more vitamin A and significantly less vitamin C at lunch than nonparticipants.[86] Gleason and Suitor observed comparable trends in intake, but found that differences between NSLP participants and nonparticipants were not statistically significant. As noted above, results from Gleason and Suitor are considered stronger because of the improved approach to selection-bias adjustment in their analysis.

For all other vitamins and minerals, neither SNDA-I nor Gleason and Suitor (2003) found significant differences between lunch intakes of NSLP participants and nonparticipants. It is likely that the significant effects reported in other studies are at least partially attributable to selection bias. Both SNDA-I researchers and Gleason and Suitor (2003) found significant effects for thiamin, vitamin B_6, folate, and iron in regression models that did not adjust for selection bias (data not

[86]Although participants consumed significantly less vitamin C at lunch than nonparticipants, intakes of both groups far exceeded the one-third RDA standard defined for NSLP meals.

shown); however, these effects disappeared in models that controlled for selection bias.

Every study that examined intakes of riboflavin, calcium, and phosphorus found that NSLP participants consumed significantly larger amounts of these nutrients at lunch than nonparticipants (although SNDA-I found that results varied for some subgroups of children). It is generally accepted that this pattern is due to increased consumption of milk, a concentrated source of all these nutrients, by NSLP students (Lin and Ralston; 2003, Devaney et al., 1993; Radzikowski and Gale, 1984). (Impacts on food consumption patterns are discussed in more detail in a subsequent section.)

Moreover, analyses completed by both SNDA-I and NESNP (Wellisch et al., 1983) researchers suggested that differences in the vitamin and mineral intakes of NSLP participants and nonparticipants at lunch are due to the *types* of food consumed, rather than the *quantities*. Both SNDA-I and NESNP examined the nutrient density of lunches and found it to be higher in lunches eaten by NSLP participants than those eaten by nonparticipants.[87] Although only the NESNP results were tested for statistical significance, both groups of investigators concluded that the NSLP increased intakes of selected nutrients by providing lunches that were more dense in those nutrients, rather than by providing more food.

Results of the SNDA-II study, which found that reductions in the fat and saturated fat content of NSLP meals were achieved without reducing vitamin and mineral content, suggest that impacts on intake of key vitamins and minerals are likely to persist in post-SMI meals. In fact, SNDA-II found that the average vitamin and mineral content of the lower fat lunches offered in the 1998-99 school year was significantly greater than the vitamin and mineral content of higher fat lunches offered in 1992-93 (SNDA-I) (Fox et al., 2001).[88]

Other Dietary Components

Both SNDA-I (Devaney et al., 1993) and Gleason and Suitor (2003) found that pre-SMI lunches consumed by NSLP participants and nonparticipants provided comparable amounts of cholesterol and sodium. However, the SNDA-I subgroup analysis revealed that among 6- to10-year-olds and low-income students, NSLP participants consumed significantly more cholesterol than nonparticipants.[89]

Gleason and Suitor (2003) also studied fiber intake. They found that lunches consumed by NSLP participants contributed significantly more fiber than those consumed by nonparticipants.

Impacts on Total Daily Intake of Food Energy and Nutrients

To have a meaningful influence on students' nutrition or health status, NSLP impacts on dietary intake must be sustained over the course of the day. It is possible for effects on lunch intakes to be offset by other meals and snacks consumed throughout the day. Therefore, for a more complete appreciation of how the NSLP affects students' dietary intake, it is important to examine the program's effect on the total diet. In the available literature, this was generally assessed as impacts on 24-hour intake.

Fourteen studies examined the impact of NSLP participation on 24-hour intake of food energy and nutrients. (Seven of these studies also assessed lunch intake and were included in the preceding section.) Findings are summarized in table 28 and discussed below. All the studies that assessed impacts on 24-hour intake were completed before implementation of the SMI. In addition, SNDA-I did not include subgroup analyses for the 24-hour intake data. Consequently, little is known about differential impacts on 24-hour intake for various subgroups of students. A few of the early NSLP studies did look at impacts among selected subgroups (Akin, 1983a and b; Hoagland, 1980; U.S. Department of Health Education and Welfare, 1972; Emmons et al., 1972). However, findings from these studies are quite dated, most of the studies used simple bivariate comparisons, and none attempted to control for selection bias.

Food Energy and Macronutrients

With emphasis given to findings from SNDA-I (Devaney et al., 1993) and Gleason and Suitor (2003), the available data indicate that before implementation of the SMI, NSLP participants and nonparticipants consumed similar amounts of food energy and protein over 24 hours.

SNDA-I and Gleason and Suitor (2003) both found that, in comparison with nonparticipants, 24-hour

[87]Nutrient density measures nutrient intake relative to energy intake: % RDA for nutrient 'X' % RDA for energy.

[88]SNDA-II looked only at vitamin A, vitamin C, calcium, and iron—the nutrients that are specifically addressed in SMI standards.

[89]Overall mean cholesterol intakes at lunch were less than one-third of the recommended daily maximum of 300 milligrams (NRC, 1989b).

Table 28—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes over 24 hours

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Food energy and macronutrients				
Food energy	Gleason (2001) [national] Wellisch (1983) [national] Akin (1983a, b) [national] Emmons (1972) [1 district] {except subgroup noted} HEW (1972) [10 States]	Gleason (2003) [national] Fraker (1987) [national] {except subgroup noted} Emmons (1972) [1 district] {nutritionally adequate}	Devaney (1993) [national] Hoagland (1980) [national] Howe (1980) [1 school] Price (1978) [1 State] {2-3 times per week}	Fraker (1987) [national] {females, 5-10}
Protein	Gleason (2003) [national] Gleason (2001) [national] Wellisch (1983) [national] Fraker (1987) [national] {except subgroups noted} Akin (1983a) [national] Price (1978) [1 State] Emmons (1972) [1 district] {except subgroup noted} HEW (1972) [10 States]	Devaney (1993) [national] Fraker (1987) [national] {females, 15-21} Hoagland (1980) [national] Howe (1980) [1 school] Emmons (1972) [1 district] {nutritionally adequate}	Fraker (1987) [national] {males, 15-21}	
Carbohydrate		Fraker (1987) [national] {males, 15-21}	Fraker (1987) [national] {females, 5-10, 15-21} Hoagland (1980) [national] [1]	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] Fraker (1987) [national] {except subgroups noted}
Fat	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national] Fraker (1987) [national] {females, 11-14}	Hoagland (1980) [national] [1] Fraker (1987) [national] {males, 5-14}	Fraker (1987) [national] {except subgroups noted}	
Saturated fat	Gleason (2003) [national] Gleason (2001) [national] Devaney (1993) [national]			

See notes at end of table.

Continued—

Table 28—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes over 24 hours—Continued

Outcome	Significant impact	No significant impact		Significant impact
	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamins				
Vitamin A	Devaney (1993) [national] Wellisch (1983) [national] Akin (1983a) [national] Akin (1983b) [national] {except subgroups noted} Price (1978) [1 State] HEW (1972) [10 States]	Gleason (2003) [national] Akin (1983b) [national] {6-11years; low income} Hoagland (1980) [national]	Gleason (2001) [national] Howe (1980) [1 school] Emmons (1972) [1 district] {except subgroup noted}	Emmons (1972) [1 district] {nutritionally adequate}
Vitamin B_6	Gleason (2001) [national] Wellisch (1983) [national] Akin (1983a, b) [national]	Gleason (2003) [national]	Devaney (1993) [national]	
Vitamin B_{12}	Gleason (2003) [national] Gleason (2001) [national] Akin (1983a) [national]	Devaney (1993) [national]		
Vitamin C	Akin (1983a) [national] {6-11 years} Akin (1983b) [national] {6-11 years; non-low income} Hoagland (1980) [national][1] Emmons (1972) [1 district] {nutritionally needy; not low-income} HEW (1972) [10 States]	Gleason (2003) [national] Akin (1983a) [national] {12-18 years} Akin (1983b) [national] {except subgroup noted} Emmons (1972) [1 district] {nutritionally adequate; low-income}	Gleason (2001) [national] Wellisch (1983) [national] Howe (1980) [1 school]	Devaney (1993) [national]
Vitamin E		Gleason (2003) [national] Gleason (2001) [national]		
Folate		Gleason (2003) [national] Gleason (2001) [national]	Devaney (1993) [national]	
Niacin	Wellisch (1983) [national] Akin (1983a) [national] {6-11 years} Emmons (1972) [1 district] {nutritionally needy} HEW (1972) [10 States]	Gleason (2003) [national] Gleason (2001) [national] Akin (1983a) [national] {12-18 years} Emmons (1972) [1 district] {except subgroups noted}	Devaney (1993) [national] Hoagland (1980) [national] Howe (1980) [1 school] Emmons (1972) [1 district] {nutritionally adequate}	

See notes at end of table.

Continued—

Table 28—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes over 24 hours—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Riboflavin	Gleason (2003) [national] Gleason (2001) [national] Wellisch (1983) [national] Price (1978) [1 State] Emmons (1972) [1 district] {nutritionally needy} HEW (1972) [10 States]	Devaney (1993) [national] Hoagland (1980) [national] Howe (1980) [1 school] Emmons (1972) [1 district] {except subgroups noted}	Emmons (1972) [1 district] {nutritionally adequate}	
Thiamin	Gleason (2001) [national] Akin (1983a) [national] {6-11 years} Emmons (1972) [1 district] {except subgroup noted} HEW (1972) [10 States]	Gleason (2003) [national] Wellisch (1983) [national] Akin (1983a) [national] {12-18 years} Howe (1980) [1 school] Emmons (1972) [1 district] {nutritionally adequate}	Devaney (1993) [national] Hoagland (1980) [national]	Price (1978) [1 State] {2-3 times per week}
Minerals				
Calcium	Gleason (2003) [national] Gleason (2001) [national] Wellisch (1983) [national] Akin (1983a) [national] Howe (1980) [1 school] Price (1978) [1 State] Emmons (1972) [1 district] {nutritionally needy} HEW (1972) [10 States]	Devaney (1993) [national] Hoagland (1980) [national] Emmons (1972) [1 district] {except subgroups noted}	Emmons (1972) [1 district] {nutritionally adequate}	
Iron	Akin (1983a, b) [national] Price (1978) [1 State] {0-1 time per week} Emmons (1972) [1 district] {nutritionally needy; not low-income} HEW (1972) [10 States]	Gleason (2003) [national] Gleason (2001) [national] Wellisch (1983) [national] Howe (1980) [1 school] Emmons (1972) [1 district] {low-income}	Devaney (1993) [national] Hoagland (1980) [national] Emmons (1972) [1 district] {nutritionally adequate}	
Magnesium	Gleason (2003) [national] Gleason (2001) [national] Wellisch (1983) [national] Akin (1983a) [national]	Devaney (1993) [national]		

See notes at end of table.

Continued—

Table 28—Findings from studies that examined the impact of the National School Lunch Program on students' dietary intakes over 24 hours—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Phosphorus	Gleason (2003) [national] Gleason (2001) [national] Wellisch (1983) [national] Akin (1983a) [national] Price (1978) [1 State]	Devaney (1993) [national] Hoagland (1980) [national]		
Zinc	Gleason (2003) [national] Gleason (2001) [national]	Devaney (1993) [national]		
Other dietary components				
Added sugars				Gleason (2003) [national] Gleason (2001) [national]
Cholesterol	Gleason (2001) [national]	Gleason (2003) [national] Devaney (1993) [national]		
Fiber	Gleason (2003) [national]	Gleason (2001) [national]		
Sodium	Gleason (2001) [national	Gleason (2003) [national] Devaney (1993) [national] Fraker (1987) [national] {males, 5-10; females, 15-21} Hoagland (1980) [national][1]	Fraker (1987) [national] {except subgroups noted}	

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 3 schools). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in Chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Findings for SNDA-I (Devaney et al., 1993) are based on selection-bias-adjusted model.

Findings for Akin et al. (1983b) were reported as significant at $p \leq 0.10$.

Unless otherwise noted, results for Price et al. (1978) are for children who usually participated in NSLP 4-5 times per week.

Findings for Emmons et al. (1972) are based on comparison of intakes before and after introduction of free lunch program. Authors looked at differences in at-home intakes and 24-hour intakes. Differences reported as significant are those where 24-hour intakes were different but at-home intakes were either not different or smaller than 24-hour differences. Study assessed impacts in four subgroups (see table 26).

[1] Significance of differences not tested/not reported.

intakes of NSLP participants were lower in carbohydrates and higher in total fat and saturated fat as a percentage of total energy intake. Gleason and Suitor (2003) also found that 24-hour intakes of NSLP participants were significantly lower in added sugars than the intakes of nonparticipants. All these findings are consistent with findings from the analysis of lunch intakes, indicating that pre-SMI impacts on intakes of carbohydrate, fat, and saturated fat persisted over the course of the day.

Vitamins and Minerals

Findings from SNDA-I (Devaney et al., 1993) and Gleason and Suitor (2003) are divergent for 24-hour intakes of most vitamins and minerals. SNDA-I found that most of the increases in vitamin and mineral intakes observed at lunch diminished over the course of the day. In SNDA-I, the only significant NSLP impacts that persisted over 24 hours were an increase in vitamin A intake and a decrease in vitamin C intake. (Overall mean vitamin C intakes of both groups were more than 250 percent of the RDA.) NSLP participants' 24-hour intakes of vitamin B_{12}, calcium, phosphorus, magnesium, and zinc continued to be greater than those of nonparticipants, but the differences were not statistically significant.

In contrast, Gleason and Suitor (2003) found that all of the impacts on vitamin and mineral intakes observed at lunch persisted over 24 hours. Specifically, they found that, relative to nonparticipants, NSLP participants had significantly greater 24-hour intakes of vitamin B_{12}, riboflavin, calcium, magnesium, phosphorus, and zinc. In keeping with findings from their analysis of lunch intakes, Gleason and Suitor (2003) found no significant impact on 24-hour intakes of vitamins A or C.

As noted, findings from Gleason and Suitor (2003) are considered stronger than findings from SNDA-I. Indeed, Devaney and colleagues cautioned that SNDA-I's estimates of NSLP impacts over 24 hours were less precise than their estimates of NSLP impacts at lunch. This is true because estimates of 24-hour impacts are influenced by differences in unmeasured characteristics and measurement error associated with other eating occasions, in addition to differences in unmeasured characteristics and measurement error associated with lunch. The same is true of Gleason and Suitor's (2003) estimates of NSLP impacts over 24 hours, of course; however, the fixed-effects model estimated by Gleason and Suitor (2003) did a better job than the SNDA-I model of controlling for unmeasured characteristics that may have affected consumption at eating occasions other than lunch. Gleason and Suitor's model was based on differences within individual students rather than between groups of students, and the model included covariates that controlled for several potentially relevant variables, including Body Mass Index (BMI),[90] frequency of exercise, hours of television watched, and whether reported intakes were heavier or lighter than usual.

Other Dietary Components

Both SNDA-I (Devaney et al., 1993) and Gleason and Suitor (2003) found that NSLP participation did not affect students' 24-hour intakes of cholesterol or sodium. This is consistent with findings from their respective analyses of lunch intakes (using results for the overall SNDA-I sample).

In addition, Gleason and Suitor (2003) found that NSLP participants consumed significantly more fiber over 24 hours than nonparticipants. This is consistent with the finding from the analysis of lunch intakes and suggests that the NSLP's impact on fiber intake persists over the course of the day.

Impacts on Food Consumption Patterns

Examining the food consumption patterns of NSLP participants and nonparticipants can prove helpful in understanding the effects the NSLP has on students' nutrient intake. Several researchers looked at food consumption patterns, using a number of approaches. SNDA-I researchers examined the percentage of students consuming specific foods and food groups at lunch (Devaney et al., 1993). Simple weighted tabulations were reported, without adjustment for observed differences in characteristics of the two groups or for selection bias. In their first analysis of the CSFII data, Gleason and Suitor (2001) computed the number of Food Guide Pyramid servings consumed by each child and compared regression-adjusted means. Their analysis looked at both lunch and 24-hour consumption.

Cullen et al. (2000) also looked at Food Guide Pyramid servings, but their analysis was limited to lunch and to fruits and vegetables. Rainville (2001) and Wolfe and Campbell (1993) compared cumulative counts of food items within Food Guide Pyramid groups (expressed as categorical variables). Finally, Melnick et al. (1998) and Yperman and Vermeersh (1979) used index scores to reflect 24-hour food consumption. Melnick and his colleagues computed a Food Guide Pyramid score and

[90]BMI is the accepted standard for defining overweight and obesity. BMI is equal to [weight in kilograms] ÷ [height in meters]^2.

a 5-A-Day score for each student and also tabulated the number of servings of fats, oils, and sweets consumed. Yperman and Vermeersch constructed a measure similar to the Food Guide Pyramid score, using data from a food frequency checklist.

Because none of the studies that examined impacts on food consumption controlled for selection bias, conclusions about impacts on these outcomes are more tentative than those about impacts on intake of energy and nutrients. Results of the available studies, summarized in table 29, are largely consistent. Only the studies by Rainville and Cullen are based on data that were collected sometime after the implementation of the SMI.

Food Consumption at Lunch

The available data suggest that NSLP participants consumed *more* milk and vegetables at lunch and *fewer* sweets and snack foods than nonparticipants. Findings for other food groups are equivocal. SNDA-I found that a significantly greater proportion of NSLP participants than nonparticipants consumed grain products at lunch. In contrast, Gleason and Suitor (2001) found that, on average, NSLP participants consumed significantly *fewer servings* of grains at lunch than nonparticipants. In both cases, between-group differences were relatively small.

The Gleason and Suitor (2001) finding deserves more weight than the SNDA-I finding because the former analysis looked at the actual number of servings consumed (rather than the percentage of children eating at least one item within the food group) and adjusted for differences in observed characteristics of students. Rainville (2001) reported results similar to Gleason and Suitor and found that the increase in the number of grain items consumed by nonparticipants was attributable to a high prevalence of sandwiches in lunches from home.

Gleason and Suitor (2001) found no difference between NSLP participants and nonparticipants in consumption of fruits and juices at lunch. However, most of the other studies reported that NSLP participants consumed more fruit and juices than nonparticipants.

Food Consumption Over 24 Hours

Data on food consumption patterns of NSLP participants and nonparticipants over 24 hours are more limited. (SNDA-I (Devaney et al., 1993), Rainville (2001), and Cullen (2000) did not evaluate 24-hour consumption.) The available data suggest that some NSLP impacts on food consumption at lunch maintained over 24 hours, while others faded.

Gleason and Suitor (2001) reported that NSLP impacts on consumption of milk, vegetables, and meat were maintained over 24 hours. However, the decreased consumption of grain products at lunch noted among NSLP participants did not persist over 24 hours, nor did the decreased consumption of sweetened beverages.

Melnick et al. (1998) found that NSLP participants consumed fewer servings of fats, sweets, and oils over 24 hours than nonparticipants. These researchers also found that NSLP participants scored higher than nonparticipants on a composite measure that evaluated servings from the Food Guide Pyramid (5th graders), as well as on a 5-A-Day score that looked specifically at fruit and vegetable consumption.

The study by Yperman and Vermeersch (1979), which found that the NSLP had a significant negative impact on students' "dietary complexity," stands in stark contrast to the positive or neutral findings of other studies. This result can be heavily discounted, however, because the study is dated and methods used to collect and analyze food group data do not meet current standards.

Impacts on Other Nutrition- and Health-Related Outcomes

The literature search identified 10 studies that looked at impacts of the NSLP beyond food and nutrient intake. Table 30 describes these studies, three of which are also included in the preceding section on dietary intake (Wellisch et al., 1983; Hoagland et al., 1980; and Emmons et al., 1972). Six studies looked at children's weight and/or height. Four studies looked at biochemical measures, specifically iron status or serum cholesterol levels, and three looked at impacts on household food expenditures. Findings from these studies are summarized in table 31. One study (Gretzen and Vermeersch, 1980) also looked at school attendance and cognitive performance. That study, which is dated and has serious limitations as a test of the NSLP, found no effects of participation in a free school lunch program on any of these measures.[91]

Weight and Height

Few studies have looked at the relationship between NSLP participation and children's weight status or linear growth, and none of them offers definitive evidence. The NESNP (Wellisch et al., 1983) measured students' height, weight, and triceps skinfold (a measure of body

[91]The study analyzed 8 years of school records in an attempt to determine the impact of free school lunch, alone and in combination with Head Start.

Table 29—Findings from studies that examined the impact of the National School Lunch Program on students' food consumption patterns

	Significant impact	No significant impact		Significant impact
Measure	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Lunch consumption				
Fruits and fruit juices	Rainville (2001) [10 schools] Devaney (1993) [national][1] Wolfe (1993) [51 schools][1]	Gleason (2001) [national] Cullen (2000) [5 schools]		
Grain products	Devaney (1993) [national]			Gleason (2001) [national] Rainville (2001) [10 schools]
Meat, poultry, fish, and meat substitutes	Gleason (2001) [national] Devaney (1993) [national]			Rainville (2001) [10 schools] Wolfe (1993) [51 schools]
Milk and milk products	Gleason (2001) [national] Rainville (2001) [10 schools] Devaney (1993) [national] Wolfe (1993) [51 schools]			
Vegetables	Gleason (2001) [national] Rainville (2001) [10 schools] Cullen (2000) [5 schools] Devaney (1993) [national][1] Wolfe (1993) [51 schools][1]			
Fats, oils, and salad dressings			Devaney (1993) [national]	
Snack foods				Rainville (2001) [10 schools] Wolfe (1993) [51 schools]
Sugar, sweets, and sweetened beverages[2]				Gleason (2001) [national] Devaney (1993) [national]

See notes at end of table.

Continued—

Table 29—Findings from studies that examined the impact of the National School Lunch Program on students' food consumption patterns—Continued

	Significant impact	No significant impact		Significant impact
Measure	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Food diversity/ total number of food items	Rainville (2001) [10 schools] Wolfe (1993) [51 schools]			
24 hours				
Fruit and fruit juices	Wolfe (1993) [51 schools]		Gleason (2001) [national]	
Grain products		Gleason (2001) [national]		
Meat, poultry, fish, and meat substitutes	Gleason (2001) [national]			
Milk and milk products	Gleason (2001) [national] Wolfe (1993) [51 schools]			
Vegetables	Gleason (2001) [national] Wolfe (1993) [51 schools]			
Fats, sweets, and oils				Melnick (1998) [25 schools]
Snack foods				Wolfe (1993) [51 schools]
Sweetened beverages			Gleason (2001) [national]	

See notes at end of table.

Continued—

Table 29—Findings from studies that examined the impact of the National School Lunch Program on students' food consumption patterns—Continued

Measure	Significant impact	No significant impact		Significant impact
	Participants scored higher	Participants scored higher/same	Participants scored lower	Participants scored lower
Summary measures				
5-A-Day Index Score	Melnick (1998) [25 schools]			
Food Guide Pyramid Index Score	Melnick (1998) [25 schools] {5th grade}	Melnick (1998) [25 schools] {2nd grade}		
Dietary Complexity Score				Yperman (1979) [2 schools]

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 3 schools). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Food group results for Gleason et al. (2001) and for Cullen (2000) are based on mean number of servings consumed. Results for Devaney et al. (1993) are based on the percentage of children consuming food group. Results for Rainville (2001) are based on cumulative counts of lunch items in each group, and results for Wolfe and Campbell (1993) are based on categorical scores based on number of items reported.

Findings for SNDA-I (Devaney et al., 1993) are not adjusted for selection bias.

Wolfe and Campbell (1993) did not present data for 24-hour consumption but reported that, with the exception of differences in meat consumption and food diversity, all differences observed in lunch consumption persisted over 24 hours.

[1] Study looked at fruits and vegetables as one group (recorded here under "fruits") and vegetables other than potatoes or tomato sauce as another group (recorded here under vegetables).

[2] Devaney et al. (1993) looked at sugar, sweets, and sweetened beverages as a group. Gleason et al. looked only at sweetened beverages and included separate measures for soda and fruit drinks/flavored drinks.

Table 30—Studies that examined the impact of the National School Lunch Program on other nutrition and health outcomes

Study	Data source[1]	Population (sample size)	Design	Measure of participation	Analysis method
Weight and/or height					
Jones et al. (2003)	1997 PSID, Child Development Supplement	Children ages 5-12 with household incomes ≤185% of poverty (n=772)	Participant vs. nonparticipant	Parent report that child "participates"	Multivariate regression
Wolfe et al. (1994)	Students in 51 schools in New York State, excluding New York City (1987-88)	Children in grades 2 and 5 (n=1,797)	Participant vs. nonparticipant	Parent report that "child eats school lunch"	Multivariate regression
Wellisch et al. (1983) (NESNP)	Nationally representative sample of students from 276 public schools (1980-81)	Children and adolescents in grades 1-12 (n=6,556)	Participant vs. nonparticipant	Average long-term weekly participation	Multivariate regression
Gretzen and Vermeersch (1980)[2]	All students in 2 intervention programs and 2 comparison programs in 1 SFA in California	Children and adolescents in grades 1-8 (n=332)	Participant vs. nonparticipant	Began receiving free school lunch in grade 1 and regularly through grade 8	Analysis of variance; bivariate t-tests
Emmons et al. (1972)	All students in selected grades in 1 district in rural New York State (1970-71)[3]	Children in grades 1-4 (n=844)	Participants, before vs. after[4]	Took 70% or more of school meals offered during study period	Comparison of means (type of statistical test not reported)
Paige (1972)	Students in 4 schools in Baltimore, MD	Children in grades 1, 2, and 6 (n=742)	Participant vs. nonparticipant, before and after	Not reported	Comparison of means (type of statistical test not reported)
Nutritional biochemistries					
Kandiah and Peterson (2001)	Students in 1 school in Indiana	Children/adolescents ages 11-15 (n=3,155)	Participants, before vs. after (cholesterol)	Ate school lunch at least 3 times per week	Multivariate regression
Hoagland (1980)	1971-74 NHANES-I	Children and adolescents ages 6-21 (n=3,155)	Participant vs. nonparticipant[5] (iron, cholesterol, protein)	Ate school lunch on recall day	Linear regression

See notes at end of table.

Continued—

Table 30—Studies that examined the impact of the National School Lunch Program on other nutrition and health outcomes—Continued

Study	Data source[1]	Population (sample size)	Design	Measure of participation	Analysis method
Emmons et al. (1972)	All students in 2 selected grades in 1 district in rural New York State (1970-71)[3]	Children in grades 1-4 (n=844)	Participants, before vs. after (iron)	Took 70% or more school meals offered during study period[4]	Comparison of means (type of statistical test not reported)
Paige (1972)	Students in 4 schools in Baltimore, MD	Children in grades 1, 2, and 6 (n=742)	Participants vs. nonparticipants, before and after (iron)	Not reported	Comparison of means (type of statistical test not reported)
Household food expenditures					
Long (1991)	1980-81 NESNP	Children and adolescents in grades 1-12 (n=5,778)	Participant vs. nonparticipant	Any household member participates in NSLP at least once during a typical week	Multivariate regression with selection-bias adjustment[6]
Wellisch et al. (1983) (NESNP)	Nationally representative sample of students in 276 public schools (1980-81)	Children and adolescents in grades 1-12 (n=6,556)	Participant vs. nonparticipant	Current weekly NSLP participation	Multivariate regression
West and Price (1976)	Students in schools/ districts in 8 regions in Washington State; Blacks and Mexican-Americans were oversampled (1972-73)	Children ages 8-12 (n=992)	Participant vs. nonparticipant	Value of free school lunches (dollars per month)	Multivariate regression. Separate models for Blacks, Whites, Mexican-Americans.

[1]Data sources:

NESNP = National Evaluation of School Nutrition Programs.

NHANES-I = First National Health and Nutrition Examination Survey.

PSID = Panel Study of Income Dynamics, Child Development Supplement.

[2]Study also examined physical fitness, school attendance, and academic performance.

[3]Study included a second district where both free lunch and free breakfast were offered. The two districts were considered separately in the analysis, but the analysis of the second district did not separate contributions of breakfast and lunch meals.

[4]Study compared intakes before and after introduction of a free lunch program. Results reported for four different subgroups based on baseline characteristics: nutritionally adequate, nutritionally needy, low-income (eligible for free lunch), and not low-income.

[5]Did not differentiate NLSP and other lunch programs.

[6]Participation measure not same week as expenditure measure; included NSLP and SBP in expenditures.

Table 31—Findings from studies that examined the impact of the National School Lunch Program on other nutrition and health outcomes

	Significant impact	No significant impact		Significant impact
Outcome	Participants higher	Participants higher/same	Participants lower	Participants lower
Weight and height				
Height		Wellisch (1983) [national] Emmons (1972) [1 district] Paige (1972) [4 schools] {grade 1} Gretzen (1980) [4 schools] {females}	Paige (1972) [4 schools] {grade 2, grade 6} Gretzen (1980) [4 schools] {males}	
Probability of underweight				Wellisch (1983) [national][1,2] Wolfe (1994) [1 State][3]
Weight	Wellisch (1983) [national] {older}	Gretzen (1980) [4 schools] Emmons (1972) [2 districts] {nutritionally needy; low income} Paige (1972) [4 schools] {grade 2}	Paige (1972) [4 schools] {grade 1, grade 6}	
Weight for height		Wellisch (1983) [national][1] Gretzen (1980) [4 schools]		Paige (1972) [4 schools] {grade 1}
Body Mass Index	Wolfe (1994) [1 State]			
Percent body fat[4]	Wellisch (1983) [national] {older} Wolfe (1994) [1 State]			
Probability of overweight/ overfatness	Wellisch (1983) [national] {older}[5]	Jones (2003) [national] {food-secure males}[6] Wolfe (1994) [1 State][7]	Jones (2003) [national] {food-insecure males and food-secure females}[6]	Jones (2003) [national] {food-insecure females}[6]

See notes at end of table.

Continued—

Table 31—Findings from studies that examined the impact of the National School Lunch Program on other nutrition and health outcomes—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants higher	Participants higher/same	Participants lower	Participants lower
Nutritional biochemistries				
Hemoglobin		Emmons (1972) [2 districts][8]	Hoagland (1980) [national]	
Hematocrit		Emmons (1972) [2 districts][8] Paige (1972) [4 schools]	Hoagland (1980) [national]	
Composite growth and iron status variable			Paige (1972) [4 schools]	
Serum cholesterol		Hoagland (1980) [national]		Kandiah (2001)
LDL cholesterol				Kandiah (2001)

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 6 schools). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Gretzen and Vermeersch (1980) also examined physical fitness, school attendance, and academic performance. No significant differences were found.

[1] Included only males age 11 and younger and females age 10 and younger.
[2] Based on weight for height <25th NCHS percentile.
[3] Based on arm fat area <10th percentile; results not significant using BMI.
[4] Based on measurements of triceps skinfold (Wellisch et al., 1983) or arm fat area (Wolfe et al., 1994).
[5] Based on weight for age and triceps fatfold > 75th NCHS percentile.
[6] Based on BMI >=85th percentile on CDC growth charts. CDS's definition for "at risk of overweight."
[7] Assessed using two measures: BMI >90th percentile and arm fat area > 90th percentile in NHANES I and II.
[8] Significance of differences not tested/not reported (samples too small).

fat). Participation was defined on the basis of children's average weekly participation from first grade through the current school year. The analysis, which did not control for selection bias, found that older participants weighed more and had greater mean triceps skinfold measurements than comparably aged nonparticipants. These findings are difficult to interpret, however, because the authors did not provide information on whether program participants were closer to age-standardized norms than nonparticipants (Rush, 1984). Thus, it is not clear whether the findings suggest a health benefit or risk. If, for example, children tended to be underweight for their age or stature, greater weight and fatness among participants could be considered a benefit. On the other hand, if children tended to be overweight for age or stature, these findings would not be considered beneficial.

A more recent study by Wolfe et al. (1994) obtained similar results and reported them in a more easily interpreted manner. The authors assessed the prevalence of overweight in elementary school children in New York State, using BMI and measures of triceps skinfold and arm fat area. Data were compared with national reference data for 1974 and 1980. NSLP participants were defined on the basis of a parent report that the child "eats school lunch." The authors concluded that overweight was a problem among elementary school students in New York State, and that students who ate the school lunch tended to be fatter than those who did not.

Wolfe and her colleagues made no attempt to assess or control for selection bias, a critical consideration in estimating the impact of a feeding program on weight status. Thus, these results indicate that NSLP participants in New York State were more overweight than nonparticipants. They do not, however, indicate that these differences are the result of NSLP participation. It is possible, for example, that overweight children chose to participate in the NSLP more often than nonoverweight children.

A recent study by Jones et al. (2003) looked at the relationship between food security, participation in FANPs, including the NSLP, and the risk of overweight. The authors used data from the 1997 Panel Study of Income Dynamics (PSID) Child Development Supplement to examine the risk of overweight among children ages 5-12 in low-income households (income $\leq$185 percent of poverty). Risk of overweight was defined as BMI at or above the 85^{th} percentile. This cutoff is routinely used to identify children who are considered to be "at risk" of becoming overweight (CDC, 2003). As used by Jones et al. (2003), it also includes children who are considered to be overweight (BMI at or above the 95th percentile) (CDC, 2003). Weights were reported by primary caregivers and heights were measured by field interviewers. The authors indicate that approximately 86 percent of the children had been weighed within the preceding month and that 16 percent of caregivers had to estimate weight because they had no recent reference point.

The analysis assessed the likelihood of being at risk of becoming overweight among children living in food-secure and food-insecure households, while controlling for participation in a number of FANPs and other relevant characteristics. Results showed that NSLP participation did not affect the likelihood that males would be at risk of becoming overweight, regardless of whether they lived in food-secure or food-insecure households. The likelihood of being at risk of becoming overweight was also unaffected by NSLP participation status among females in food-secure households. Among females in food-insecure households, however, those who participated in the NSLP had 71-percent reduced odds of being at risk of becoming overweight, compared with those who did not participate. The authors offer no explanation for the apparent protective effect of NSLP participation among food-insecure females and suggest that more research is needed to understand the relationship between income, food security, FANP participation, and weight status.

It is doubtful that cross-sectional studies can adequately address questions about program impacts on children's weight and height. Indeed, researchers who attempted to assess the impact of the WIC program on these outcomes concluded that a longitudinal study with serial measurements was essential (Puma et al., 1991).

Nutritional Biochemistries

Four of the studies identified in the literature search examined impacts of the NSLP on nutritional biochemistries. Researchers examined measures of iron status (hemoglobin and/or hematocrit) and/or cholesterol levels. Only Hoagland (1980) used a national dataset in assessing these outcomes. The three smaller, local studies used the "participants, before vs. after" design (essentially a longitudinal design with a single followup measurement), which is preferable to the "participant vs. nonparticipant" design for assessing impacts on biological variables. With the exception of the recent study by Kandiah and Peterson (2001), studies are based on data from the 1970s. None of the studies reported a significant NSLP effect.

Iron

Analyzing children's hemoglobin and hematocrit values from NHANES-I, Hoagland (1980) found no significant differences between NSLP participants and nonparticipants. Working with a sample of children from four schools in Baltimore, Paige (1972) found no effects on hematocrit levels or on a composite variable that reflected both growth status and iron status. Emmons and colleagues (1972) found so few students with low iron status that they did not test the significance of differences between groups.

Cholesterol

Hoagland (1980) also used NHANES-I data to assess children's cholesterol levels. He found no significant difference between NSLP participants and nonparticipants. In the same analysis, Hoagland attempted to look for differences in biochemical indicators of protein-calorie malnutrition. Finding no abnormal levels of serum albumin in the sample, however, he concluded that these measures were not useful for assessing NSLP health impacts.

Kandiah and Peterson (2001) examined total cholesterol and LDL cholesterol levels in a group of 30 children and adolescents ages 11-15. The sample was limited to students who ate NSLP meals at least three times per week. Baseline levels were compared with followup levels measured 4 months later. Results showed that both total cholesterol and LDL cholesterol levels decreased significantly over the 4-month period. This was true for students who ate NSLP lunches as well as those who ate both breakfast and lunch. Rather than attribute these changes to a positive impact of the NSLP, the authors concluded that changes in cholesterol levels over time were due to hormonal fluctuations associated with puberty. No information was provided on the protocol used in collecting students' blood samples (for example, whether students were fasting when bloods were drawn and for what period of time) or on the reliability of the measures obtained.

Household Food Expenditures

Assessment of household food expenditures can provide information on the extent to which receipt of NSLP meal benefits increases the value of food available to families.[92] Potentially, the NSLP meal benefit will be an addition to total household food expenditures. However, its value may be partly offset if the household reduces some food expenditures because of the availability of the subsidized lunch—if, for example, money that would have been spent to purchase lunch for the student is applied to nonfood uses.

Two of the three studies that examined impacts on household food expenditures were based on data collected for the NESNP. Wellisch and her colleagues (1983) reported a dollar-for-dollar increase in the value of food available to participating households as a result of participation in the NSLP. Long (1991) reanalyzed the NESNP data, using only the sample of students who attended schools that offered the NSLP and adjusting for selection bias, and obtained somewhat different results. She found that the overall impact of NSLP participation was to increase household food expenditures, but she estimated that each additional NSLP benefit dollar reduced other food expenditures by about $0.61, for a net addition of $0.39 to the value of food expenditures on behalf of the household. Long's results were comparable to those of West and Price (1976), who evaluated the impact of free school lunches on household food expenditures for a sample of children ages 8-12 in Washington State.

West and Price (1976) and Wellisch et al. (1983) both found somewhat larger supplementation effects for Black than White households. Supplementation was also somewhat greater for Hispanic households, but the effect was not statistically significant. Long's reanalysis of the NESNP data did not include subgroup analyses.

Summary

The body of research reviewed in this chapter indicates that, prior to implementation of the SMI, the NSLP had a significant impact on the dietary intake of school-age children and adolescents. There is strong evidence that the program increased children's intakes of selected vitamins and minerals at lunch (vitamin B_{12}, riboflavin, calcium, phosphorus, magnesium, and zinc), and the strongest available evidence suggests that these effects persisted over 24 hours. Because of limitations in the dietary assessment methodologies used, it is not possible to determine whether NSLP participants were more likely than nonparticipants to have adequate intakes of these vitamins and minerals.

There is also convincing evidence that, prior to the SMI, NSLP participants consumed less carbohydrate and more fat and saturated fat (as a percentage of total

[92]Food expenditure data have also been used to evaluate the success of the NSLP in meeting its second objective: encouraging the consumption of domestic agricultural products. The program is considered efficient in meeting its agricultural support goals if most of the NSLP subsidy is spent on food and little is diverted to nonfood expenditures (Radzikowski and Gale, 1984).

food energy) than nonparticipants, both at lunch and over 24 hours. The strongest available evidence suggests that the difference in carbohydrate intake was due to decreased consumption of added sugars among NSLP participants.

Finally, the available evidence indicates that, prior to the SMI, NSLP participation had no significant effect on intake of sodium or cholesterol. NSLP participation was associated, however, with a significantly greater intake of dietary fiber, both at lunch and over 24 hours.

Evidence from the SNDA-II study demonstrates that, even in the early stages of the SMI, schools had made significant progress in decreasing the fat and saturated fat content of school lunches and in increasing the carbohydrate content. Since SNDA-II data were collected (the 1998-99 school year), efforts have continued at the Federal, State, and local levels to make school lunches consistent with SMI standards for these nutrients. In addition, there has been increased emphasis on nutrition education for school-age children to promote acceptance and consumption of healthier school meals.

Consequently, the current impact of the NSLP on students' intakes of total fat, saturated fat, and carbohydrates is unknown and can only be answered with new research. The same can be said of the program's impacts on vitamin and mineral intakes. However, evidence from SNDA-II suggests that changes in the macronutrient profile of school lunches has been achieved without compromising overall vitamin and mineral content. In fact, lunches offered in 1998-99 provided significantly greater amounts of vitamin A, vitamin C, calcium, and iron than lunches offered in 1991-92.

With the exception of impacts on household food expenditures, the existing evidence is too limited to support conclusions about whether NSLP participation affects other nutrition- and health-related outcomes. There is limited, but reasonably strong, evidence that NSLP participation increases total household food expenditures. However, the available data are quite dated (the most recent were collected in the early 1980s).

Clearly, there is a critical need for an updated study of the NSLP and its impacts on children. To provide a comprehensive picture of how the NSLP influences children's food and nutrient intakes, future studies will need to differentiate between the multiple sources of foods and beverages available at school (reimbursable meals, a la carte purchases, vending machines, snack bars, etc.).

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Chapter 6

School Breakfast Program

The School Breakfast Program (SBP) began as a pilot program in 1966. The intent was to provide breakfast at school to children from poor areas who may not have eaten breakfast at home, and to children in rural areas who ate an early breakfast, did chores, and then arrived at school hungry after traveling long distances (Devaney and Stuart, 1998). The program was modeled after the National School Lunch Program (NSLP), which had been in existence for some 20 years when the SBP was established. The combination of the NSLP and SBP was intended to provide "a coordinated and comprehensive child food service [program] in schools" (P.L. 89-842).

Schools that participate in the SBP provide breakfasts to children, regardless of household income. Schools receive Federal reimbursements for each meal served, with higher reimbursements for meals served free of charge or at a reduced price to children from low-income families. In FY 2002, more than 8 million children participated in the SBP on an average school day. Approximately 1.4 billion meals were served, at a total Federal cost of $1.6 billion (U.S. Department of Agriculture (USDA), Food and Nutrition Service (FNS), 2003a).

Program Overview

The SBP was authorized by the Child Nutrition Act of 1966 (P.L. 89-842).[93] Greater Federal subsidies were offered for schools identified as having a "severe need" as a way of encouraging participation by schools in low-income areas (which tended to have higher operating costs). Congress authorized the SBP as a permanent program in 1975. While the program continued to provide greater subsidies to schools in areas of severe need, the authorizing legislation declared that the SBP was targeted to "all schools where it is needed to provide adequate nutrition for all children in attendance" (P.L. 94-105).

In 1989, the Child Nutrition Act was amended with the specific intention of expanding the availability of the SBP in the Nation's schools. The Secretary of Agriculture was required to award startup grants, administered through State agencies, to "a substantial number of States" on a competitive basis. The grants, which were targeted toward school districts that served large proportions of low-income children, were funded at a level of $3 million in 1990. The funds were to be used to help cover nonrecurring costs associated with initiating the SBP.[94] Since 1989, the size of the SBP has more than doubled, increasing from 3.8 million breakfasts per day in FY 1989 to 8.1 million breakfasts per day in FY 2002 (USDA/FNS, 2003a).

The SBP operates in essentially the same manner as the NSLP (see chapter 5). The program is administered by FNS at the Federal level and by school food authorities (SFAs) at the local level. SFAs receive cash reimbursements for each meal served (commodities are tied to the NSLP). For the 2002-03 school year, the basic subsidies were $1.17 for free breakfasts, $0.87 for reduced-price breakfasts, and $0.22 for breakfasts served to children who purchased meals at the full price (referred to as "paid meals").[95] Children eligible for reduced-price breakfasts cannot be charged more than $0.30 per breakfast. SFAs set their own prices for full-price/paid breakfasts, but must operate their school meal service program on a nonprofit basis (USDA/FNS, 2003b). Of the 1.4 billion breakfasts served in FY 2002, 83 percent were served to children who received their meals free or at a reduced price (USDA/FNS, 2003a).

Program Use

In comparison with the NSLP, the SBP is available to fewer children and student participation rates are lower. The SBP is offered in approximately 78 percent of the schools and institutions that offer the NSLP (USDA/FNS, 2003b and USDA/FNS, 2003c). Using data from the first School Nutrition Dietary Assessment Study (SNDA-I), Rossi (1998) found that in schools where the SBP was available, only 78 percent of children who were eligible for free or reduced-price breakfasts were certified to receive meal subsidies. And of those certified, only 37 percent participated in the breakfast program. The combined effect was that at the time the

[93] Much of the text in the program overview section also appears in another report prepared by Abt Associates Inc. (McLaughlin et al., 2002). A preliminary draft of this chapter was used in preparing that report.

[94] Changes made by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA; Public Law 104-193) eliminated this grant program.

[95] Reimbursement rates are higher for Hawaii and Alaska. In addition, schools that operate in high-poverty areas may qualify for "severe-need" reimbursement. In the 2002-03 school year, severe-need schools could receive up to an additional $0.23 for free and reduced-price breakfasts.

SNDA-I data were collected (1991-92 school year), only 29 percent of children eligible for free and reduced-price breakfast were eating school breakfasts.

Findings from more recent studies are similar. The second School Nutrition Dietary Assessment Study (SNDA-II), completed in the 1998-99 school year, found that 22 percent of children in SBP schools participated in the program on an average day (compared with 60 percent for the NSLP) (Fox et al., 2001). Students approved for free meals participated in the SBP at a higher rate (39 percent) than students approved for reduced-price meals (20 percent) or students who purchased full-price meals (8 percent). Participation was greatest in elementary schools (26 percent) and lowest in high schools (11 percent).

A USDA-sponsored study found that a major factor affecting application and participation decisions related to the NSLP and SBP was the perceived stigma of receiving free or reduced-price school meals (Glantz et al., 1994). This was found to be more of an issue for the SBP and for secondary school students than for the NSLP and elementary school students. Study findings suggested that parents and older students believed that receiving free or reduced-price meals labeled students as poor and set them apart. While program regulations require school districts to ensure that children approved for free and reduced-price meals are not overtly identified, the perception was that simply eating a school breakfast carries a stigma, regardless of one's income status.

Several other factors have been identified as potential barriers to SBP participation. These include scheduling (when breakfast is served relative to the official start of the school day), meal prices, competing a la carte offerings, bus/transportation issues, lack of time to eat, lack of space, and student preferences for other foods (Reddan et al., 2002; Rosales and Jankowski, 2002; Project Bread, 2000).

Offering a free breakfast to all school children, regardless of family income, is viewed as a promising vehicle for increasing participation in the SBP. Under existing Federal regulations, schools may eliminate the burden of determining eligibility for meal benefits and provide all meals free of charge. Under Provisions 2 and 3 of the National School Lunch Act (which govern both the NSLP and the SBP), schools are reimbursed at established rates for a 4-year period.[96] During this period, breakfasts and lunches are offered to all students free of charge and schools do not have to conduct free and reduced-price certifications. State agencies may grant subsequent 4-year extensions if there has been no substantial change in the income level of a school's target population (USDA/FNS, 2003d). School districts are responsible for costs in excess of Federal reimbursements. In the 1999-2000 school year, an estimated 3,154 schools (3.8 percent of all schools) used either Provision 2 or 3 (Abraham et al., 2002).

Some States require that all schools, or schools with a specific proportion of low-income students, participate in the SBP. According to the Food Research and Action Center (FRAC), 37 of the 50 States had their own legislative requirements related to the SBP in the 2002-03 school year, and/or provided funding for school breakfasts (Food Research and Action Center, 2003). Twenty-five States had laws mandating that specific schools participate in the SBP, and 22 States provided some type of funding.[97] Three States (Illinois, Maryland, and Massachusetts) provided State funding for so-called "universal-free" school breakfast in certain schools. In these schools, breakfasts are provided free to all children regardless of household income. In addition, North Carolina provided funding for universal-free school breakfasts for kindergarten students.[98]

The idea of providing universal-free school breakfasts became increasingly popular in the 1990s. Several States and school districts implemented demonstrations to test the feasibility and impact of such programs. Early results indicated that universal-free breakfasts substantially increased participation. Program evaluators also reported positive effects on tardiness, absentee rates, academic achievement, and related outcomes. However, because most of the demonstrations were small, used nonexperimental designs, and had other design and/or data limitations, these findings were considered tentative (McLaughlin et al., 2002).

To obtain a more scientifically sound assessment of the potential impacts of providing universal-free school breakfasts, Congress authorized the School Breakfast Program Pilot Project (SBPP) in 1998 (P. L. 105-336). This 3-year demonstration project, administered by FNS, includes a comprehensive evaluation of both the implementation and impact of a universal-free

[96]Schools may operate under Provisions 2 or 3 for one or both meal programs. Currently, more schools operate under these provisions for the SBP than for the NSLP (USDA, FNS, Office of Analysis and Evaluation, 2004).

[97]Counts are not mutually exclusive. Some States provide no funding and/or have no mandates.

[98]Minnesota also provided universal-free breakfast funding from 1999 to 2002. However, the statute that granted the funding was repealed by the State legislature in 2003.

school breakfast program. The project began in the 2000-01 school year and ended at the end of the 2002-03 school year. Results from the first year of implementation, including information on impacts on a variety of student outcomes, were published in late 2002 (McLaughlin et al., 2002). A final report covering all 3 years of the pilot is expected in 2004.

Nutrition Standards

To be eligible for Federal subsidies, SBP meals must meet defined nutrition standards. As described in chapter 5, USDA launched the School Meals Initiative (SMI) in 1995 to improve the nutritional quality of school meals. Prior to the SMI, schools that participated in the SBP were required to follow a meal pattern that specified the types and amounts of foods and beverages to be offered to students of different ages. The basic meal pattern, which was modeled after the NSLP meal pattern, includes:

- Milk: 1 serving per meal
- Fruit, juice, or vegetables: 1 serving per meal
- Meat or meat alternate and bread or bread alternate: 2 servings total per meal.[99]

Under the SMI, new nutrient-based standards were established for SBP meals. SMI nutrition standards specify that breakfasts must provide, over the course of a week, an average of 25 percent of students' daily requirements for energy (calories) and key nutrients (calcium, iron, protein, and vitamins A and C). Breakfasts must also be consistent with the *Dietary Guidelines for Americans* recommendations for intake of fat and saturated fat.[100]

The Healthy Meals for Healthy Americans Act (P.L. 103-448) formally required that school meals be consistent with the *Dietary Guidelines* and that schools begin complying with SMI nutrition standards in the 1996-97 school year unless a waiver was granted by the relevant State agency. The regulatory requirement that school meals be consistent with the *Dietary Guidelines* has been incorporated into the FNS strategic plan. The current goal is that all schools will satisfy these standards by 2005 (USDA/FNS, 2000).

The SNDA-I study demonstrated that, prior to the SMI, breakfasts offered in the SBP were consistent with SMI nutrition standards for key nutrients, but were low in energy relative to defined Recommended Energy Allowances (REAs), high in fat and saturated fat, relative to *Dietary Guidelines* recommendations, and high in sodium relative to the National Research Council's (NRC) recommendation (Burghardt et al., 1993). Data from SNDA-II, collected in the 1998-99 school year (early in SMI implementation), showed improvement in the nutritional profile of SBP meals. Breakfasts offered in 1998-99 continued to meet standards for key nutrients, but were significantly lower in total fat, saturated fat, and sodium than pre-SMI breakfasts (Fox et al., 2001). Indeed, the average nutrient profile of breakfasts offered in the1998-99 school year was consistent with SMI nutrition standards for both total fat and saturated fat. Breakfasts offered in elementary schools were also consistent with the NRC's recommendation for sodium, and breakfasts offered in secondary schools all but met this standard (601 milligrams (mg) sodium, on average, compared with a standard of 600 mg). On average, breakfasts continued to fall short of the benchmark for energy content.[101]

In the years since the SNDA-II data were collected, efforts to implement the SMI nutrition standards have continued at the Federal, State, and local levels. Consequently, even this relatively recent data may not provide an accurate picture of the nutrient content of meals currently offered in the SBP.[102]

The existing literature on SBP impacts needs to be considered cautiously because program operations changed substantially after most of the available research was completed. The SMI and related initiatives may have affected the meals offered to students and students' consumption of those meals. In addition, the concerted efforts made in recent years to increase participation in the SBP may have led to changes in the characteristics of the children being served by the program. This, in turn, may lead to changes in program

[99]One serving from each category or two servings from one of the two categories.

[100]Goals for sodium and cholesterol content are not included in SMI nutrition standards; however, schools are encouraged to monitor levels of these dietary components.

[101]For secondary school breakfasts, the difference between the mean energy content of pre-SMI and post-SMI breakfasts was statistically significant.

[102]The more recent Evaluation of the SBPP (McLaughlin et al., 2002) assessed the nutrient content of SBP meals in elementary schools participating in the SBPP demonstration in the 2000-01 school year. However, data from that study are not directly comparable to data from SNDA-I. The SBPP analysis was based on the meals actually selected by students (weighted nutrient analysis), while the SNDA-I and SNDA-II results discussed above were based on meals offered to students (unweighted nutrient analysis). SNDA-II included both weighted and unweighted analyses. A comparison of weighted analysis results from SNDA-II and the Evaluation of the SBPP suggests that the fat and saturated fat content of SBP meals in elementary schools has continued to decline.

impacts. For these reasons, new research is essential to understanding the nutrition- and health-related impacts of the SBP as it operates today (Guthrie, 2003).

Research Overview

This review, like the other reviews in this report, focuses on research that examined the impact of a federally sponsored food and nutrition assistance program—in this case the SBP—on health- and nutrition-related outcomes. A related body of research focuses on the general impacts of eating breakfast rather than the specific impacts of participating in the SBP (eating an SBP breakfast). Much of this research was conducted in controlled environments or in developing countries, and is not reviewed here. The interested reader can find summaries of these and related studies in two other reports (Jacobson et al., 2001; Briefel et al., 1999).

Relevant SBP research can be divided into two categories: (1) studies that looked at impacts on students' dietary intakes and (2) studies that looked at impacts on academic performance and related outcomes such as attendance, tardiness, and behavior: A few studies (see table 32) also examined impacts on height and/or weight or nutritional biochemistries. (None found significant effects.) The evaluation of the SBPP is the only study to look at all of these outcomes concurrently. The following sections describe each body of research and summarize key findings.

Impacts on Students' Dietary Intakes

The literature search identified 14 studies that attempted to estimate SBP impacts on children's dietary intake (table 32). This includes two national evaluations that included student-level measures—SNDA-I and NESNP (the National Evaluation of School Nutrition Programs)—as well as a reanalysis of the SNDA-I data (Group I). (The third national evaluation conducted by USDA—SNDA-II—did not collect student-level data). Also included are four studies based on secondary analysis of data from national surveys (Group II), five State and local studies (Group III), and two studies of universal-free breakfast demonstrations (Group IV).

The strongest available data on SBP impacts in this area come from the SNDA-I study (Gordon et al., 1995 and Devaney and Stuart, 1998) and from the first-year report of the evaluation of the SBPP (McLaughlin, 2002). SNDA-I is the most recent, comprehensive, and state-of-the-art study designed specifically to study the SBP. It included a nationally representative sample of public and private elementary and secondary schools and a nationally representative sample of students attending those schools. SDNA-I researchers included statistical controls for selection bias in their analysis. SNDA-I data are dated, however, because they were collected before the SMI and before recent initiatives to increase SBP participation.

Data from the SBPP are more recent—collected in spring 2001—but they are not nationally representative and are based on data from six school districts that volunteered to participate in a universal-free breakfast demonstration. The SBPP used a randomized experimental design; however, the evaluation was designed to assess the impact of a universal-free breakfast program rather than the impact of the SBP, per se. The main analyses completed for the first-year SBPP report compared the *entire* treatment group (students in schools where universal-free breakfast was available) with the *entire* control group (students in schools where the standard SBP was available). Results of these analyses provide no information on the question that is central to understanding the impact of the SBP: Do the dietary (or other) outcomes of students who participate in the SBP differ from those of students who do not participate in the program?

However, SBPP researchers completed a separate analysis that does provide some insight on this issue. A statistical procedure was used to estimate impacts on students who *actually participated* in the universal-free school breakfast program. In the analysis of dietary intakes, universal-free breakfast participation was defined as consumption of a school breakfast on the day dietary intake data were collected. The Bloom correction (Bloom, 1984) was used to adjust the estimate of the average impact on the entire treatment group, based on the difference between the proportion of students in treatment and control schools who ate breakfast on a typical school day. Results provide unbiased estimates of the impact of participating in universal-free school breakfast.[103] These findings are suggestive of the impact of participating in the regular SBP some 6 years after the SMI was launched.[104]

A recent study by Gleason and Suitor (2001) also deserves comment. This study used data from the

[103]For more information, see McLaughlin et al. (2002), chapter 4 and appendices C and F.

[104]The characteristics of meals provided in universal-free breakfast programs are likely to be comparable to those provided in the regular SBP (see McLaughlin et al., 2002). However, the characteristics and consumption behaviors of students who choose to participate in universal-free school breakfast and students who choose to participate in the regular SBP may not be comparable.

Table 32—Studies that examined the impact of the School Breakfast Program on students' dietary intakes

Study	Outcome(s)	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group I: National evaluations							
Devaney and Stuart (1998) (SNDA-I)	Likelihood of eating breakfast	Nationally representative sample of students from 329 public and private schools	Single 24-hour recall	Children and adolescents in grades 1-12 (n=2,966)	Participant vs. nonparticipant	Ate SBP breakfast on recall day	Multivariate regression with selection-bias adjustment
Gordon et al. (1995) (SNDA-I)	Nutrient intake at breakfast and over 24 hours Food intake at breakfast	Nationally representative sample of students from 329 public and private schools	Single 24-hour recall	Children and adolescents in grades 1-12 (n=2,966)	Participant vs. nonparticipant	Ate SBP breakfast on recall day	Multivariate regression with selection-bias adjustment (nutrients) Bivariate t-tests (foods)
Wellisch et al. (1983) (NESNP)	Nutrient intake at breakfast and over 24 hours[2]	Nationally representative sample of students from 276 public schools	Single 24-hour recall	Children and adolescents in grades 1-12 (n=2,180)	Participant vs. nonparticipant	Ate SBP breakfast and NSLP lunch on recall day (nonparticipants ate NSLP lunch only)	Multivariate regression
Group II: Secondary analysis of national surveys							
Gleason and Suitor (2001)	Nutrient intake at breakfast and over 24 hours Food intake at breakfast and over 24 hours	1994-96 CSFII	2 nonconsecutive 24-hour recalls	Children and adolescents in SBP schools ages 6-18 (n=2,693)	Participant vs. nonparticipant	Ate SBP breakfast on recall day	Comparison of regression-adjusted means
Basiotis et al. (1999)	Nutrient intake over 24 hours Food intake over 24 hours	1994-96 CSFII	2 nonconsecutive 24-hour recalls	Low-income children ages 6-18 (sample size not reported)	Participant vs. nonparticipant	Ate SBP breakfast on recall day	Multivariate regression
Devaney and Fraker (1989)	Nutrient intake at breakfast and over 24 hours	1980-81 NESNP	Single 24-hour recall	Children ages 5-10 (n=2,118) and 11-21 (n=2,809)	Participant vs. nonparticipant	Ate SBP breakfast on recall day	Multivariate regression

See notes at end of table.

Continued—

Table 32—Studies that examined the impact of the School Breakfast Program on students' dietary intakes—Continued

Study	Outcome(s)	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Hoagland (1980)	Nutrient intake over 24 hours[2]	1971-74 HANES-I	Single 24-hour recall	Children and adolescents ages 6-21 (n=412)[3]	Participant vs. nonparticipant	Ate school breakfast on recall day	Analysis of variance
Group III: State and local studies							
Nicklas et al. (1993a)	Nutrient intake at breakfast	Bogalusa Heart Study (1984-85 and 1987-88)	Single 24-hour recall	Children age 10 (n=393)	Participant vs. nonparticipant	Ate school breakfast on recall day	Analysis of variance
Nicklas et al. (1993b)	Nutrient intake over 24 hours	Bogalusa Heart Study (1984-85 and 1987-88)	Single 24-hour recall	Children age 10 (n=393)	Participant vs. nonparticipant	Ate school breakfast on recall day	Analysis of variance
Emmons et al. (1972)	Nutrient intake at breakfast and over 24 hours[2]	All students in 2 school districts in rural New York State (1970-71)	Single 24-hour recall	Children in grades 1-4 (n=844)	Participants, before vs. after[4]	Took 70% or more of school meals offered during study period	Comparison of means (type of statistical test not reported)
Hunt et al. (1979)	Nutrient intake over 24 hours	2 schools in Compton, CA (1970-71)	Single 24-hour recall	Children in grades 3-6 (n=555)	Participant vs. nonparticipant[5]	60% participation in SBP on days in school during experimental period	Analysis of variance
Price et al. (1978)	Nutrient intake over 24 hours	Students in schools/districts in 8 regions in Washington State; Blacks and Mexican-Americans were oversampled (1971-73)	3 nonconsecutive 24-hour recalls, including 1 weekend day	Children ages 8-12 (n=728)[6]	Participant vs. nonparticipant	Usually ate school breakfast 4-5 times/week	Multivariate regression

See notes at end of table.

Continued—

Table 32—Studies that examined the impact of the School Breakfast Program on students' dietary intakes—Continued

Study	Outcome(s)	Data source[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group IV: Studies of universal-free breakfast							
McLaughlin et al. (2002)	Nutrient intake at breakfast and over 24 hours Food intake at breakfast and over 24 hours[2,7]	70 matched pairs of school units in 6 school districts[8]	24-hour recall, with second recall for subsample (usual intake)	Children in grades 2-6 (n=4,290)	Randomized experiment	Ate universal-free breakfast on recall day[9]	Multivariate regression with Bloom correction to assess impact on universal-free breakfast participants (subgroup analyses)
Cook et al. (1996)	Nutrient intake at breakfast	Elementary schools in Central Falls, RI, matched with schools in Providence, RI	Single breakfast recall	Children in grades 3-6 (n=225)	Participant vs. nonparticipant	Ate SBP breakfast on recall day	Not well described.

[1]Data sources:
CSFII = Continuing Survey of Food Intake of Individuals.
NHANES-I = First National Health and Nutrition Examination Survey.
NESNP = National Evaluation of School Nutrition Programs.
[2]Also examined impacts on height and/or weight, but reported no significant findings.
[3]The study compared SBP participants with students who did not have access to the SBP. Only three SBP participants were included in the sample.
[4]Study compared intakes before and after introduction of free lunch (one district) and free lunch and breakfast (one district). Results reported for four different subgroups based on baseline characteristics: nutritionally adequate, nutritionally needy, low-income (eligible for free lunch), not low income.
[5]Study examined the effect of introducing a free breakfast program, comparing students in experimental school to control school that had no breakfast program.
[6]School breakfast was not the main focus of the study. Only 20 children in the sample consumed a school breakfast.
[7]The study also examined impacts on BMI and food security and found no significant effects.
[8]The study focused on students in grades 2-6. For sampling/matching purposes, schools with different grade configurations (e.g., K-2 and 3-5) were considered one unit. There were a total of 73 treatment schools and 70 control schools.
[9]The study's main analysis compared outcomes for the entire treatment group with outcomes for the entire control group. Findings discussed in this report, however, are from a separate analysis that estimated impacts on students who actually participated in universal-free breakfast on the day of the recall.

1994-96 wave of the Continuing Survey of Food Intakes by Individuals (CSFII) to study food and nutrient intakes of SBP participants and nonparticipants. Although well done and based on more recent data than SNDA-I, this study is not as strong as either SNDA-I or the evaluation of the SBPP, for at least two reasons. First, the CSFII data are generalizable to the U.S. population as a whole, but not to schoolchildren specifically. Second, Gleason and Suitor did not attempt to control for selection bias, presumably because of the lack of relevant variables in the CSFII dataset. Indeed, the authors caution that, because of likely selection bias, the estimates presented in their report should *not* be interpreted as estimates of SBP impacts.

Impacts on the Likelihood That Students Will Eat Breakfast

The overarching goal of the SBP is to provide breakfast to children who might otherwise not eat before starting the school day. The original analysis of the SNDA-I data (Gordon et al., 1995) found that the likelihood that a child would eat breakfast before school began was not significantly different for children in schools that did and did not offer the SBP. About 12 percent of the children in each type of school ate no breakfast. This analysis was flawed, however, because it defined children who ate breakfast as those who consumed at least 50 calories between the time of waking and 45 minutes after the start of school, a threshold that could include extremely small snacks. As an example, an average-size sandwich cookie provides approximately 50 calories.

A reanalysis of the SNDA-I data, completed by Devaney and Stuart (1998), considered three different definitions of "breakfast." Each definition was based on foods consumed between waking and 45 minutes after the start of school and included foods consumed at home and at school. The three definitions were:

(1) Consumption of any food or beverage (except water).

(2) Consumption of food or beverages that contributed more than 10 percent of the REA.

(3) Consumption of food or beverages from at least two of five major food groups PLUS more than 10 percent of the REA.

Results of this analysis indicated that, for the student population as a whole, the availability of the SBP had no significant impact on the likelihood of breakfast consumption, regardless of the definition used. Among students from low-income households, however, availability of the SBP was associated with a significantly greater likelihood that students would eat a more substantial breakfast, (a breakfast that satisfied definition 2 or 3). At the same time, availability of the SBP made it significantly less likely that low-income students would consume a nominal breakfast (a breakfast that provided 10 percent or less of the REA).[105] These results, summarized in table 33, suggest that, at the time the SNDA-I data were collected, the primary objective of the SBP was being met. That is, low-income students were more likely to eat breakfast if the SBP was available in their school.[106]

Impacts on Dietary Intake

Table 34 summarizes results of studies that compared dietary intakes of SBP participants and nonparticipants at breakfast. (As noted previously, the evaluation of the SBPP (McLaughlin et al., 2002) actually compared intakes of participants and nonparticipants in schools where universal-free breakfasts were available). Table 35 provides comparable data for intakes over 24 hours. Both tables are divided into five sections: food energy

[105]The results differed slightly for elementary and secondary school students. For those in secondary school, a significantly greater likelihood of breakfast consumption was observed only for the most stringent breakfast definition (2 food groups and more than 10 percent of the REA).

[106]The Evaluation of the SBPP (McLaughlin et al., 2002) assessed the impact of a universal-free breakfast program on the likelihood that students would eat breakfast. These data are not included in this review because they have limited applicability to the regular SBP, where free breakfasts are available only to students who are certified to receive that benefit.

Table 33—Low-income students' breakfast consumption patterns by SBP availability

	Type of breakfast consumed			
		Any food or beverage:		Food from two food groups *plus*
SBP availablility	None	≤10% REA	>10% REA	>10% REA
	Percent			
SBP available	12.5	13.0*	6.1*	67.4*
SBP not available	13.3	22.8	8.6	54.8

Notes: *SBP vs. non-SBP difference is statistically significant ($p<0.01$).
REA = Recommended Energy Allowance.
Results reported are for elementary students. For secondary students, a significantly greater likelihood of breakfast consumption was observed only for the most stringent definition (two food groups and >10 percent of REA).
Source: Devaney and Stuart (1998), reanalysis of data from SNDA-I (Gordon et al., 1995).

Table 34—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes at breakfast

Outcome	Significant impact: Participants consumed more	No significant impact: Participants consumed more/same	No significant impact: Participants consumed less	Significant impact: Participants consumed less
Food energy and macronutrients				
Food energy	Gleason (2001) [national] Cook (1996) [1 city] Gordon (1995) [national] Nicklas (1993a) [1 city] Devaney (1989) [national] {5 to 10}	McLaughlin (2002) [6 districts] Devaney (1989) [national] {11 to 21}		
Protein	Cook (1996) [1 city] Gordon (1995) [national] Nicklas (1993a) [1 city] Wellisch (1983) [national]	McLaughlin (2002) [6 districts] Gleason (2001) [national]		
Carbohydrates	Nicklas (1993a) [1 city]		McLaughlin (2002) [6 districts] Gleason (2001) [national]	Gordon (1995) [national]
Fat	Cook (1996) [1 city] Nicklas (1993a) [1 city]	Gleason (2001) [national] Gordon (1995) [national]	McLaughlin (2002) [6 districts]	
Saturated fat	Gleason (2001) [national]	Gordon (1995) [national]	McLaughlin (2002) [6 districts] Nicklas (1993a) [1 city]	
Vitamins				
Vitamin A	Cook (1996) [1 city] Emmons (1972) [2 districts] {nutritionally needy}	McLaughlin (2002) [6 districts] Gleason (2001) [national] Gordon (1995) [national]	Devaney (1989) [national] {5 to 10}	Wellisch (1983) [national] Devaney (1989) [national] {11 to 21}
Vitamin B_6	Cook (1996) [1 city]	McLaughlin (2002) [6 districts] Gordon (1995) [national]	Gleason (2001) [national] Devaney (1989) [national]	Wellisch (1983) [national]
Vitamin B_{12}		Gleason (2001) [national] Gordon (1995) [national]	McLaughlin (2002) [6 districts]	
Vitamin C	Gleason (2001) [national] Emmons (1972) [2 districts] {low-income}	McLaughlin (2002) [6 districts] Gordon (1995) [national]		

See notes at end of table.

Continued—

Table 34—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes at breakfast—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamin E		Gleason (2001) [national]		
Folate		McLaughlin (2002) [6 districts]	Gleason (2001) [national] Gordon (1995) [national]	
Niacin	Hunt (1979) [2 schools]	McLaughlin (2002) [6 districts] Gordon (1995) [national]	Gleason (2001) [national]	Wellisch et al. (1983) [national]
Riboflavin	Gleason (2001) [national] Gordon (1995) [national] Emmons (1972) [2 districts] {low-income}	McLaughlin (2002) [6 districts]		
Thiamin		McLaughlin (2002) [6 districts] Gleason (2001) [national] Gordon (1995) [national]		Wellisch et al. (1983) [national]
Minerals				
Calcium	McLaughlin (2002) [6 districts] Gleason (2001) [national] Cook (1996) [1 city] Gordon (1995) [national] Devaney (1989) [national] Wellisch (1983) [national] Emmons (1972) [2 districts] {all incomes}			
Iron	Cook (1996) [1 city]	McLaughlin (2002) [6 districts]	Gleason (2001) [national] Gordon (1995) [national]	Devaney (1989) [national] Wellisch (1983) [national]
Magnesium	Gleason (2001) [national] Gordon (1995) [national] Devaney (1989) [national] Wellisch (1983) [national]	McLaughlin (2002) [6 districts]		

See notes at end of table.

Continued—

Table 34—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes at breakfast—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Phosphorus	McLaughlin (2002) [6 districts] Gleason (2001) [national][1] Gordon (1995) [national] Wellisch (1983) [national]			
Zinc		Gleason (2001) [national] McLaughlin (2002) [6 districts] Gordon (1995) [national]		
Other dietary components				
Cholesterol		Gleason (2001) [national] Gordon (1995) [national] Nicklas (1993a) [1 city]		McLaughlin (2002) [6 districts] Devaney (1989) [national]
Fiber	Gleason (2001) [national]	McLaughlin (2002) [6 districts]		
Sodium	Gleason (2001) [national] Nicklas (1993a) [1 city]	Gordon (1995) [national]	McLaughlin (2002) [6 districts]	
Added sugars	Nicklas (1993a) [1 city]		McLaughlin (2002) [6 districts]	Gleason (2001) [national]
Food group servings				
Dairy	McLaughlin (2002) [6 districts] Cook (1996) [1 city] Gordon (1995) [national]	Gleason (2001) [national]		
Fruits	McLaughlin (2002) [6 districts] Gleason (2001) [national] Cook (1996) [1 city] Gordon (1995) [national]			
Grains	Gordon (1995) [national] Cook (1996) [1 city] Gleason (2001) [national] {non-whole grains}	McLaughlin (2002) [6 districts] Gleason (2001) [national] {total grains}	Gleason (2001) [national] {whole grains}	

See notes at end of table.

Continued—

Table 34—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes at breakfast—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Meat	Gordon (1995) [national]	Gleason (2001) [national]		McLaughlin (2002) [6 districts]
Vegetables		McLaughlin (2002) [6 districts] Gleason (2001) [national] Gordon (1995) [national]		

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 1 city or 1 State). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Findings for Gordon et al. (1995) are based on selection-bias-adjusted models. Authors note that results were essentially identical to results of standard regression model.

Findings for Cook et al. (1996) are based on comparisons between universal-free breakfast participants and nonparticipants in matched control schools (nutrients) and home-breakfast consumers in universal-free schools (foods).

Wellisch et al. (1983) also assessed intakes of calcium and vitamin C. They found no difference between SBP participants and nonparticipants, but did not report point estimates.

Findings for Emmons (1972) are based on comparison of intakes before and after introducing free lunch and free lunch and breakfast programs. Differences reported as significant are those where pre/post difference was significant for district in which both breakfast and lunch were introduced, but not in the district where only lunch was introduced. The study assessed impacts in four subgroups (see table 33). Only significant findings are reported here.

Table 35—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes over 24 hours

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Food energy and macronutrients				
Food energy	Gleason (2001) [national] Gordon (1995) [national] Nicklas (1993b) [1 city] Hoagland (1980) [national]	Hunt (1979) [2 schools]	McLaughlin (2002) [6 districts] Devaney (1989) [national]	
Protein	Gordon (1995) [national] Nicklas (1993b) [1 city] Hoagland (1980) [national]		McLaughlin (2002) [6 districts] Gleason (2001) [national] Hunt (1979) [2 schools]	
Carbohydrates		McLaughlin (2002) [6 districts] Hunt (1979) [2 schools]	Gleason (2001) [national] Gordon (1995) [national]	Nicklas (1993b) [1 city]
Fat	Nicklas (1993b) [1 city]	Gleason (2001) [national] Basiotis (1999) [national] Gordon (1995) [national] Hunt (1979) [2 schools]	McLaughlin (2002) [6 districts]	
Saturated fat	Basiotis (1999) [national]	Gleason (2001) [national] Gordon (1995) [national]	McLaughlin (2002) [6 districts]	Nicklas (1993b) [1 city]
Vitamins				
Vitamin A		McLaughlin (2002) [6 districts] Devaney (1989) [national] {11-21} Hoagland (1980) [national]	Gleason (2001) [national] Gordon (1995) [national] Hunt (1979) [2 schools]	Devaney (1989) [national] {5-10}
Vitamin B_6		Gordon (1995) [national]	McLaughlin (2002) [6 districts] Gleason (2001) [national] Devaney (1989) [national] {5-10}	Devaney (1989) [national] {11-21}
Vitamin B_{12}		Gordon (1995) [national]	McLaughlin (2002) [6 districts] Gleason (2001) [national]	
Vitamin C	Gleason (2001) [national] Hoagland (1980) [national] Price (1975) [1 State]	Gordon (1995) [national]	McLaughlin (2002) [6 districts]	Hunt (1979) [2 schools]

See notes at end of table.

Continued—

Table 35—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes over 24 hours—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamin E			Gleason (2001) [national]	
Folate		McLaughlin (2002) [6 districts] Gordon (1995) [national]	Gleason (2001) [national]	
Niacin	Hoagland (1980) [national]	Gordon (1995) [national]	McLaughlin (2002) [6 districts] Gleason (2001) [national] Hunt (1979) [2 schools]	
Riboflavin	Price (1975) [1 State]	McLaughlin (2002) [6 districts] Gleason (2001) [national] Gordon (1995) [national]	Hoagland (1980) [national] Hunt (1979) [2 schools]	
Thiamin	Gordon (1995) [national] Price (1975) [1 State]	Gleason (2001) [national] Hoagland (1980) [national]	McLaughlin (2002) [6 districts] Hunt (1979) [2 schools]	
Minerals				
Calcium	Gleason (2001) [national] Gordon (1995) [national] Devaney (1989) [national] Wellisch (1983) [national] Price (1975) [1 State]	Hoagland (1980) [national] Hunt (1979) [2 schools]	McLaughlin (2002) [6 districts]	
Iron	Hoagland (1980) [national]	Gordon (1995) [national]	McLaughlin (2002) [6 districts] Gleason (2001) [national] Devaney (1989) [national] {11-21} Hunt (1979) [2 schools]	Devaney (1989) [national] {5-10}
Magnesium	Gordon (1995) [national] Wellisch (1983) [national]	Gleason (2001) [national] Devaney (1989) [national]	McLaughlin (2002) [6 districts]	
Phosphorus	Gleason (2001) [national] Gordon (1995) [national] Wellisch (1983) [national] Hoagland (1980) [national] Price (1975) [1 State]		McLaughlin (2002) [6 districts]	

See notes at end of table.

Continued—

Table 35—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes over 24 hours—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Potassium	Nicklas (1993b) [1 city]			
Zinc		Gleason (2001) [national] Gordon (1995) [national]	McLaughlin (2002) [6 districts]	
Other dietary components				
Cholesterol		Gleason (2001) [national] Gordon (1995) [national] Nicklas (1993b) [1 city]	Basiotis (1999) [national]	McLaughlin (2002) [6 districts] Devaney (1989) [national]
Fiber		Gleason (2001) [national]	McLaughlin (2002) [6 districts]	
Sodium	Nicklas (1993b) [1 city]	Gleason (2001) [national] Gordon (1995) [national]	McLaughlin (2002) [6 districts] Basiotis (1999) [national]	
Added sugars	Nicklas (1993b) [1 city]	McLaughlin (2002) [6 districts]	Gleason (2001) [national]	
Food group servings				
Dairy	Gleason (2001) [national] Basiotis (1999) [national]	McLaughlin (2002) [6 districts] Hunt (1979) [2 schools]		
Fruits	Gleason (2001) [national] Basiotis (1999) [national]	McLaughlin (2002) [6 districts]	Hunt (1979) [1 school]	
Grains		Gleason (2001) [national]	McLaughlin (2002) [6 districts] Hunt (1979) [2 schools]	Basiotis (1999) [national]
Meat		Gleason (2001) [national] Basiotis (1999) [national]	McLaughlin (2002) [6 districts]	Hunt (1979) [2 schools]
Vegetables		Basiotis (1999) [national]	McLaughlin (2002) [6 districts] Gleason (2001) [national] Hunt (1979) [2 schools]	
Soda				Gleason (2001) [national]

See notes at end of table.

Continued—

Table 35—Findings from studies that examined the impact of the School Breakfast Program on students' dietary intakes over 24 hours—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants scored higher	Participants scored higher/same	Participants scored lower	Participants scored lower
Summary measures				
Total HEI	Basiotis (1999) [national]			

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 3 schools). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Wellisch et al. (1983) also assessed intakes of energy, protein, magnesium, vitamin A, vitamin B_6, niacin, thiamin, iron, and vitamin C. They found no significant effects but did not report point estimates.

Price et al. (1978) also assessed intakes of energy, protein, calcium, phoshorus, iron, vitamin A, thiamin, riboflavin, and niacin. They reported no significant effects, but did not provide point estimates.

and macronutrients, vitamins, minerals, other dietary components, and food group servings. The text follows this general organization, but discusses findings for vitamins and minerals in one section. As in all such tables included in this report, results for each study are reported using the primary author's name. In the interest of providing a comprehensive picture of the body of research, both significant and nonsignificant results are reported in tables 35 and 36 and in all other "findings" tables. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned, however, to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

In this case, emphasis is given to findings from SNDA-I (Gordon et al., 1995) for the reasons cited previously. Findings from the evaluation of the SBPP (McLaughlin et al., 2002) are considered to provide some insight into potential changes in program impact over time. To provide additional context for these observations, data from the SBPP evaluation are considered in light of data from the SNDA-II study (Fox et al., 2001).

Findings reported for SNDA-I are based on results of regression models that controlled for selection bias using an instrumental variables approach. The models used are analogous to those used in assessing NSLP impacts (see chapter 5). However, Gordon and her colleagues found few substantive differences between results of models that did and did not attempt to control for selection into the SBP and noted that statistical tests rejected the presence of selection bias. They appropriately caveat this comment with the observation that the available identifying variables were not strong predictors of SBP participation. In estimating impacts on 24-hour nutrient intake, models adjusted for self-selection into the NSLP but not the SBP.

Energy and Macronutrients

Most studies completed prior to the implementation of the SMI, including the SNDA-I study (Gordon et al., 1995), found that SBP participants consumed more food energy and protein at breakfast than nonparticipants (table 34) and that this boost persisted over the course of the day (table 35). Of the studies that examined both breakfast and 24-hour intakes, only the Devaney and Fraker (1989) reanalysis of NESNP data found that the SBP increment in food energy was not maintained over 24 hours.

With regard to other macronutrients, SNDA-I found that SBP participants consumed significantly less carbohydrates at breakfast than nonparticipants and, although the differences were not significant, tended to consume more fat and saturated fat, both at breakfast and over 24 hours.

The evaluation of the SBPP (McLaughlin et al., 2002), the only post-SMI study identified, found no significant differences in energy and macronutrient intakes of universal-free breakfast participants and nonparticipants, either at breakfast or over 24 hours. Moreover, the general trend was the reverse of the trend observed in SNDA-I. That is, on average, point estimates for the percentage of calories from fat and saturated fat were lower for universal-free breakfast participants than nonparticipants.

These results imply a change in the nutrient profile of SBP meals over time. The suggested trend—that SBP meals are lower in energy and protein and lower in fat and saturated fat (as a percentage of total energy) than they were at the time the SNDA-I data were collected—is consistent with findings from SNDA-II (Fox et al., 2001). SNDA-II compared the nutrient content of SBP breakfasts offered in 1998-99 with SBP breakfasts offered in 1991-92 (SNDA-I).

Vitamins and Minerals

Among studies completed prior to the SMI, there is a virtual consensus that the SBP increased students' intakes of three minerals—calcium, phosphorus, and magnesium—both at breakfast and, when assessed, over the full day. There is also a consistent finding that the SBP increased riboflavin intake at breakfast, but this effect generally did not persist over the full day. All of these nutrients (calcium, phosphorus, magnesium, and riboflavin) occur in concentrated amounts in milk.

Findings from the Evaluation of the SBPP (McLaughlin et al., 2002) are somewhat consistent with this picture, but suggest that the current impact of school breakfast on mineral intake is smaller than previously estimated and that none of the impacts persist over 24 hours. In the SBPP evaluation, universal-free breakfast participants were found to consume significantly more calcium and phosphorus at breakfast than nonparticipants, but neither of these differences persisted over 24 hours. Differences for magnesium and riboflavin were not statistically significant for either time point. In addition, the SBPP evaluation estimated *usual* daily (24-hour) intakes and assessed the impact of universal-free breakfast on the likelihood that students had *adequate* intakes. No significant differences were found between students who participated in universal-free breakfast and those who did not.

Data from SNDA-II provide a potential explanation for the apparent change in impact over time. SNDA-II found that SBP breakfasts offered in 1998-99 provided 5-6 percent less calcium than breakfasts offered at the time SNDA-I data were collected, although breakfasts offered at both points in time more than satisfied the program standard of providing one-fourth of children's daily calcium needs (Fox et al., 2001). This pattern was observed for both elementary and secondary schools. SNDA-II did not assess magnesium, phosphorus, or riboflavin content.

Other Dietary Components

SNDA-I (Gordon et al., 1995) found that SBP participants consumed more cholesterol and sodium than nonparticipants (negative trends), both at breakfast and over 24 hours. However, none of the differences were statistically significant.

The SBPP evaluation (McLaughlin et al., 2002) found that universal-free breakfast participants consumed significantly *less* cholesterol than nonparticipants, both at breakfast and over 24 hours. In addition, mean sodium intakes were lower for universal-free breakfast participants; however the difference was not statistically significant. The SBPP evaluation also assessed fiber intake and intake of added sugars. There was no significant difference between universal-free breakfast participants and nonparticipants for either measure.

The apparent shift in program impacts over time implied by the SBPP data is consistent with data from SNDA-II. SNDA-II found that SBP breakfasts offered in 1998-99 were significantly lower in cholesterol and sodium than breakfasts offered in 1991-92 (Fox et al., 2001).

Food Intake

A few researchers have examined SBP impacts on food consumption patterns. SNDA-I researchers (Gordon et al., 1995) examined the percentage of students that consumed one or more foods from specific food groups at breakfast. Simple weighted tabulations were reported and the statistical significance of differences between groups was assessed using bivariate t-tests. McLaughlin et al. (2002) and Gleason and Suitor (2001) computed the number of Food Guide Pyramid servings consumed by each child and assessed differences between groups using multivariate regressions. Both analyses looked at breakfast consumption as well as consumption over 24 hours.

Basiotis and his associates (1999) used data from the 1994-96 CSFII to compare scores for food-based components of the Healthy Eating Index (HEI). These scores are based on comparisons of Food Guide Pyramid servings to age-specific recommendations. The paper presents results of bivariate t-tests but reports that results of multivariate analyses were consistent.

Findings from McLaughlin et al. (2002) provide the strongest suggestive evidence of current SBP impacts. These data indicate that universal-free breakfast participants consumed significantly more servings of fruit and dairy products at breakfast than nonparticipants, and significantly fewer servings of meats and meat substitutes. However, data on 24-hour intakes indicate that all of these effects dissipated over the course of the day.

Impacts on School Performance and Cognitive/Behavioral Outcomes

The most recent (and expanding) focus of the relevant SBP literature considers impacts of eating school breakfast on a variety of cognitive and behavioral outcomes related to school performance. Characteristics of eight studies identified through the literature review are summarized in table 36. (As noted previously, research conducted outside the United States or in controlled environments has not been included in this review.) With one exception (Meyers, 1989), these studies were done to evaluate universal-free breakfast programs rather than the actual SBP. Consequently, findings from these studies provide, at best, suggestive evidence of potential SBP impacts. Because the SBP does not offer breakfasts free of charge to all students, impacts observed in demonstrations of universal-free breakfast cannot be assumed to apply to the regular SBP.

Studies in this group used one of two approaches to defining a comparison group. The approach used most often was to compare schools that offered universal-free breakfast (treatment schools) with matched schools that offered the regular SBP (control schools). The criteria used to match schools and the relative comparability of the schools ultimately selected varied across studies. Some studies used a pre/post design,

Table 36—Studies that examined the impact of universal-free breakfast programs on school performance and behavioral/cognitive outcomes

Study	Outcomes	Data source	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Peterson et al. (2003)	Attendance, academic achievement, health, and discipline	455 schools in Minnesota (1998-2002)	School records and standardized test scores	All children for attendance measures; children in grades 3 and 5 for academic measures (n=43,067)	Participant vs. nonparticipant	Enrolled in universal-free SBP school	Logistic regression
McLaughlin et al. (2002)	Cognitive functioning, attendance, tardiness, behavior academic achievement, student health status[1]	70 matched pairs of school units in 6 school districts (1999-2001)[2]	School records and standardized test scores	Children in grades 2-6 (n=4,290)	Randomized experiment	Ate universal-free breakfast on day of measurement (short-term cognitive functioning)[3] Cumulative participation in universal-free breakfast over the year (all other measures)[3]	Multivariate regression with Bloom correction to asses impact on universal-free breakfast participants (subgroup analysis)
Murphy et al. (2001a)	Attendance and academic achievement	48 schools in Baltimore (1995-2000)	School records and standardized test scores	All children in sample schools (n=not stated)	Participants, before vs. after, separate groups, plus participants vs. nonparticipants, before and after	Enrolled in universal-free SBP school	Analysis of variance

See notes at end of table.

Continued—

Table 36—Studies that examined the impact of universal-free breakfast programs on school performance and behavioral/cognitive outcomes—Continued

Study	Outcomes	Data source	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Murphy et al. (2001b)	Attendance, tardiness, academic achievement	55 schools in Maryland (1997-2000)	School records and standardized test scores	Varied by outcome for both schools and students	Participants, before vs. after, separate groups, plus participants vs. nonparticipants, before and after	Enrolled in universal-free SBP school	Analysis of variance; bivariate t-tests
Murphy et al. (2000)	Attendance, tardiness, academic achievement, emotional functioning	30 schools in Boston, MA (1998-2000)	School records, standardized test scores, parent and student interviews	All children in sample schools (n=not stated)	Participants, before vs. after	Frequency of eating breakfast during 1 index week	Analysis of variance
Murphy et al. (1998)	Attendance, psychological measures, academic achievement	1 school in Baltimore; 2 schools in Philadelphia (dates not reported)	School records and parent, teacher, and student interviews	Children in grades 3-8 (n=133)[4]	Participants, before vs. after	Frequency of eating breakfast during 1 index week	Logistic regression
Cook et al. (1996)	Attendance, tardiness	All elementary schools in Central Falls, RI, matched with schools in Providence, RI (1994)	School records	Children in grades Pre-K-6 (n=not reported)	Participant vs. nonparticipant	Enrolled in universal-free SBP school	Not well described
Meyers et al. (1989)[5]	Attendance, tardiness, academic achievement	16 schools in Lawrence, MA (1985-87)	School records and standardized test scores	Children in grades 3-6 (n=1,023)	Participant vs. nonparticipant	Ate SBP on 3 of 5 days during 1 selected week during school year	Multivariate regression

[1] The study also examined impacts of BMI and food security and found no effects.

[2] The study focused on students in grades 2-6. For sampling/matching purposes, schools with different grade configurations (e.g., K-2 and 3-5) were considered as one school unit. There were a total of 73 treatment schools and 70 control schools.

[3] The study's main analysis compared outcomes for the entire treatment group with outcomes from the entire control group. Findings discussed in this report, however, are from a separate analysis that estimated impacts based on students' actual participation in universal-free breakfast. Impacts on short-term outcomes were estimated on the basis of participation on the day of measurement and impacts on longer term outcomes were estimated on the basis of cumulative participation over the year.

[4] For school-recorded data (maximum sample). Sample sizes varied for interview data (n=85) and teacher ratings (n=76).

[5] The Meyers et al. study (1989) was not a study of universal-free breakfast. The study compared outcomes in schools that did and did not implement the SBP.

where data collected before the implementation of universal-free breakfast was compared with data collected after implementation.

In this research, impacts were generally measured on the basis of group membership rather than on individual behavior. As discussed in the preceding description of the SBPP evaluation, impact analyses generally compared the *entire* treatment group (students in schools where universal-free breakfast was available) with the *entire* control group (students in schools where the standard SBP was available). This is a much less precise definition of participants and nonparticipants than is used in the research that examined SBP impacts on students' dietary intake and limits the confidence one can place in the findings, relative to potential impacts of the regular SBP.

As noted previously, however, the evaluation of the SBPP included a separate analysis that compared universal-free breakfast participants and nonparticipants on the basis of actual participation in the school breakfast program. For analyses that focused on school-performance outcomes, participation was defined on the basis of either same-day or cumulative participation over the implementation year, depending on the outcome. This more precise definition of universal-free breakfast participation, combined with the randomized design, dictates that considerably more credence be given to results of the SBPP study than to the other studies.

In interpreting these findings, however, it is important to note that (1) breakfast skipping was low in SBP schools; most children ate something for breakfast either at school, home, or elsewhere and (2) findings are based on data from the first year of a 3-year demonstration and may not hold across all 3 years.

Key findings for all studies are summarized in table 37 and are discussed below, by outcome.

Attendance and Tardiness

Attendance and/or tardiness are important school performance outcomes because they may serve as mediators of any effect breakfast consumption may have on learning. If the presence of a breakfast program encourages attendance and/or discourages tardiness, the program may have a positive influence on school performance simply by increasing the amount of time students spend at school.

Five of the seven studies that looked at the effect of universal-free school breakfast on attendance rates reported a significant positive effect. Similarly, all five of the studies that assessed impacts on tardiness found significant reductions in tardiness at universal-free schools. The stronger evaluation of the SBPP, which used a randomized design and estimated impacts based on cumulative program participation over the course of the intervention year, found no significant differences in attendance or tardiness.[107]

Academic Achievement

All of the studies in this group considered the impact of offering universal-free breakfasts on academic achievement. Most studies used standardized test scores to assess impacts, although a few used student grades.

As table 37 clearly illustrates, results of the SBPP evaluation stand in stark contrast to results of most of the other studies. As noted earlier in this chapter, USDA sponsored the evaluation of the SBPP to provide a scientifically sound study of this issue. Virtually all of the other studies in this group are limited to one geographic area (one city or State), most had small sample sizes, and there was no consistency across studies in the measures used to assess achievement. Moreover, all of these studies are subject to problems of selection bias because they used nonexperimental designs. Finally, as Ponza and his colleagues (1999) point out, the analyses used in many of these studies are open to question because they did not adequately control for clustering.

The SBPP evaluation does not suffer from the design and measurement weakness that limit the other studies in this group. As such, it provides definitive data on the impact of universal-free breakfast participation. The SBPP study compared gains in standardized test scores for reading and math for universal-free breakfast participants and nonparticipants (defined on the basis of cumulative annual participation rates), and found no significant differences.

Cognitive Functioning

The SBPP evaluation also examined the impact of same-day participation in universal-free breakfast on three different measures of cognitive functioning: stimulus discrimination, digit span, and verbal fluency. These measures assess students' memory and retrieval skills as well as attentional abilities, and all three were expected to be sensitive to the immediate effects, if

[107]Data on tardiness were not consistently available at the student and/or school level.

Table 37—Findings from studies that examined the impact of universal-free breakfast programs on school performance and behavioral/cognitive outcomes

	Significant impact	No significant impact		Significant impact
Outcome	Participants better	Participants better/same	Participants worse	Participants worse
Attendance	Murphy (2001a) [1 city] Murphy (2000) [1 city] Murphy (1998) [3 schools] Cook (1996) [1 city] Meyers (1989) [1 city]	Murphy (2001b) [1 State]	McLaughlin (2002) [6 districts] Peterson (2003) [1 State]	
Tardiness	Murphy (2001b) [1 State] Murphy (2000) [1 city] Murphy (1998) [3 schools] Cook (1996) [1 city] Meyers (1989) [1 city]	McLaughlin (2002) [6 districts]		
Academic achievement	Murphy (2000) 1 city Murphy (2001a) [1 city] Murphy (2001b) [1 State] {school-wide scores} Murphy (1998) [3 schools] Meyers (1989) [1 city]	Peterson (2003) [1 State] {3rd grade math; 5th grade math, reading, writing} Murphy (2001b) [1 State] {individual data}	McLaughlin (2002) [6 districts]	Peterson (2003) [1 State] {3rd grade reading}
Behavior/ emotional functionality	Murphy (2001b) [1 State] {suspensions} Murphy (2000) [1 city] Murphy (1998) [3 schools]	Murphy (2001b) [1 State] {office referrals}	McLaughlin (2002) [6 districts] {other scales} McLaughlin (2002) [6 districts] {ability to focus and follow instructions}	McLaughlin (2002) [6 districts] {teacher-rated oppositional scale}
Nurse referrals/reported health status		McLaughlin (2002) [6 districts] Murphy (2001b) [1 State]		

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 3 schools). Where findings pertain only to a specific subgroup rather than the entire study population, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in Chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

McLaughlin et al. (2002) also assessed impacts on short-term cognitive functioning and food security and found no significant effects.

any, of breakfast consumption. The analysis revealed only very minor differences between groups, and none of the differences was statistically significant.

Other Outcomes

Studies of universal-free school breakfast have also examined measures of student behavior and health. The evaluation of the SBPP found a significant and negative effect of universal-free breakfast participation (defined on the basis of cumulative participation rates over the demonstration year) on teacher-rated behavioral opposition, but no effects on a variety of other behavioral measures.

The evaluation of the SBPP also examined impacts of universal-free breakfast participation on student health status, Body Mass Index (BMI) (a measure of weight status), and food security status. No significant effects were reported.

Summary

The available research suggests that low-income students are more likely to consume a substantial breakfast when the SBP is available to them. Pre-SMI research indicated that SBP participants had significantly higher intakes of nutrients provided by milk (calcium, phosphorus, magnesium, and riboflavin) at breakfast and/or over 24 hours. There was also strong evidence that SBP participants consumed significantly more food energy and protein at breakfast than nonparticipants, as well as less carbohydrates. In addition, although differences were not statistically significant, mean intakes of fat and saturated fat, as a percentage of total energy intake, as well as intakes of cholesterol and sodium, were greater for SBP participants than nonparticipants. Data from the post-SMI SBPP evaluation suggest that, currently, there are few significant differences in the nutritional quality of breakfasts consumed by SBP participants and those consumed by nonparticipants, and that differences that are observed dissipate over the course of the day. While not definitive, the patterns observed in the SBPP data are consistent with the most recent national study of the nutritional characteristics of SBP meals (SNDA-II).

Although data from the SBPP and SNDA-II studies are useful, the true impact of the post-SMI SBP on students' dietary intakes is unknown. As discussed in detail in chapter 5, there is a critical need for an updated study of both the NSLP and the SBP and the programs' impacts on children.

Data from several State and local studies of universal-free school breakfast demonstrations reported that the availability of a universal-free breakfast program had a positive impact on attendance, tardiness, academic achievement, and/or related outcomes. However, the methodologically superior evaluation of the SBPP found no such effects. The only significant impact reported in the first-year report of the SBPP evaluation was an increase in oppositional behavior among long-term participants in unversal free breakfast. The project's final report, expected in 2004, will confirm whether these results held over all 3 years of the demonstration.

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Chapter 7

Child and Adult Care Food Program

The Child and Adult Care Food Program (CACFP) provides Federal funds for meals and snacks served to children and adults in licensed, nonresidential day care facilities, including family and group child care homes, some child care centers, Head Start programs, after-school care programs, and adult day care centers.[108] Since July 1999, the program has also served preschool children who reside in homeless shelters. Federal assistance reimburses care providers at fixed rates for each meal and snack served.

The limited amount of research on the CACFP is almost entirely descriptive, focusing on the characteristics of participating institutions and the children and adults they serve. Several studies, including four nationally representative studies sponsored by the U.S. Department of Agriculture (USDA), have documented the nutrient content of meals and snacks offered to participants, but only one of these studies examined the nutrient content of meals and snacks offered in nonparticipating institutions.

Some studies have assessed the nutrient contribution of CACFP meals and snacks to participants' overall diets. However, there has been no research on the impact of the program on participants' nutrition and health status, relative to nonparticipants.

Program Overview

The CACFP is, in reality, two separate programs. One component serves children in child care centers, family child care homes, after-school care programs, and homeless shelters, and the other component serves adults attending adult day care centers. Program regulations allow these components to be administered by two separate State agencies.

The child care component of the program is substantially larger than the adult care component. In December 2001, the program served an average of 2.6 million children and 74,000 adults per day (USDA, Food and Nutrition Service (FNS), 2002a). In FY 2002, the $1.9 billion reimbursed to participating institutions supported the provision of 1.7 billion meals and snacks to children and 44.6 million meals and snacks to adults (USDA/FNS, 2003).

[108]Program regulations refer to snacks as "supplements."

To be eligible for Federal reimbursement, providers must serve meals and snacks that meet established meal pattern requirements. These requirements are modeled on the food-based menu planning guidelines used in the National School Lunch Program (NSLP) and the School Breakfast Program (SBP) (see chapters 5 and 6). The meal patterns specify foods (meal components) to be offered at each meal and snack as well as minimum portion sizes. For children, minimum portion sizes vary by age. Currently, CACFP meals and snacks are not required to meet specific nutrient-based standards such as those implemented in the mid-1990s for the NSLP and SBP. Child care centers and homes may receive reimbursement for two meals and one snack or two snacks and one meal per child per day. Homeless shelters may receive reimbursement for three meals per child per day.

Child Care Component

The CACFP began as a pilot program in 1968, known as the Special Food Service Program for Children (SFSPFC). The SFSPFC was established under Section 17 of the National School Lunch Act (42 U.S.C. 1766). Participation was initially limited to center-based child care in areas with poor economic conditions. Beginning in 1976, family child care homes were also eligible to participate, provided that they met State licensing requirements, where these were imposed, or obtained approval from a State or local agency. Homes had to be sponsored by a nonprofit organization that assumed responsibility for ensuring compliance with Federal and State regulations and that acted as a conduit for meal reimbursements. These rules govern participation of family child care homes to this day.

The CACFP became a permanent program in 1978. At the time, the program was focused exclusively on children and was called the Child Care Food Program (CCFP). The program was not renamed the Child *and Adult* Care Food Program (CACFP) until 1987, when the adult day care component of the program was added. (The adult care component of the program is discussed in a subsequent section of this chapter.)

Initially, the system used in the CCFP to reimburse both centers and homes was modeled after the system used in the NSLP. Three categories of reimbursement were established, based on family income, and a means test was used to determine the family incomes of individual children. The largest reimbursement was provided for meals served to children with family incomes of 125 percent or less of the Federal poverty level ("free" meals); a lesser reimbursement was provided for meals served to children whose family incomes ranged from 125 to 195 percent of poverty ("reduced-price" meals); and the lowest reimbursement was provided for meals served to children whose family incomes exceeded 195 percent of the poverty guideline ("full price" meals).[109]

Applying the means test in family child care settings was perceived to limit participation. Providers complained that the means test was overly burdensome and too invasive for their relationship with the few families for whom they provided child care. In addition, sponsors claimed that meal reimbursements were insufficient to cover their administrative costs and allow for adequate reimbursement to the homes.[110] As a consequence, very few homes participated in the program—fewer than 12,000 by December 1978, approximately 2 years after homes were eligible to participate.

The 1978 Child Nutrition Amendments (P.L. 95-627) incorporated wide-ranging changes to the CCFP with the purpose of expanding participation, particularly among family child care homes. Most significantly, the 1978 Amendments eliminated the means test for homes. The three-level reimbursement structure was replaced with a single reimbursement rate for all participants, at a level slightly below the free-meal reimbursement rate for child care centers. In addition, the amendments separated the reimbursement of sponsors' administrative costs from the meal reimbursement for family child care homes. Other changes included alternative procedures for approving homes and startup and expansion funds for family child care sponsors.

The 1978 Amendments provided financial incentives for homes serving middle-income children to participate in the CCFP and for sponsoring agencies to recruit such homes into the program. Following the implementation of these amendments in May 1980, the family child care component of the program began a period of tremendous growth. Between June 1980 and March 1981, the number of participating homes increased by 40 percent—from 17,000 to 43,000.

This growth brought with it a change in the profile of children being served by the CCFP. In early 1980, program administrative data showed that most of the children served in participating homes were from low-income families—only 32 percent of these children were from families with incomes above 195 percent of the poverty level. By January 1982, however, most of the children served in participating homes were from middle-income families—62 percent of children in participating homes were from families with incomes above 195 percent of the poverty level (Glantz et al., 1983). By 1995, with over 190,000 homes participating in the program, more than 75 percent of the children in participating homes were from families with incomes above 185 percent of the poverty level (the revised threshold for eligibility for reduced-price meals established in 1982) (Glantz et al., 1997).

Program Changes To Improve Benefit Targeting

Since the mid- to late 1990s, several changes have been implemented to better target the benefits provided through the child care component of the CACFP and to expand program coverage to meet the needs of low-income children receiving care in other settings. The most dramatic change was implemented in 1996 as part of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) (P.L. 104-193). PRWORA changed the reimbursement structure for family child care homes to target benefits more specifically to homes serving low-income children. The new rate structure for family child care homes took effect July 1, 1997.

Under the new reimbursement structure, family child care homes located in low-income areas have reimbursement rates that are similar to the rates that existed for all family child care homes before PRWORA. A low-income area is defined as either an area where at least half of the children live in families with incomes below 185 percent of the poverty level or an area served by an elementary school in which at least half of the enrolled children are eligible for free or reduced-price school meals. Homes where the provider's own income is below 185 percent of the poverty guideline have the same reimbursement structure as homes located in low-income areas. Homes meeting one of these criteria are referred to as tier I homes.

All other homes are reimbursed at substantially lower rates. This latter group of homes, referred to as tier II homes, includes those that are neither located in a low-income area nor operated by a low-income provider.

[109]Effective January 1982, the income eligibility for free meals was increased from 125 to 130 percent of poverty and the threshold for reduced-price meals was reduced from 195 to 185 percent of poverty.

[110]Meal reimbursements generated by participating homes were paid directly to the sponsoring agency. The sponsor was permitted to deduct administrative costs before passing reimbursement on to providers.

Tier II homes can receive the higher tier I reimbursement rates for meals and snacks served to children from families with incomes below 185 percent of poverty, if family income is documented. FY 2003 reimbursement rates are shown in table 38.

As noted previously, family child care homes can participate in the CACFP only if they are sponsored by a recognized sponsoring agency. Sponsors are responsible for determining that homes meet the CACFP eligibility criteria, for providing training and other support to family child care providers, and for monitoring homes to ensure that they comply with applicable Federal and State regulations. Sponsors receive and verify the homes' claims for CACFP reimbursement, forward the claims to USDA for payment, receive the reimbursements from USDA, and distribute the meal reimbursements to the homes. Sponsors receive Federal reimbursement for the costs of providing these administrative services that are the lesser of (1) actual costs, (2) the budget amount approved by their State CACFP office, (3) 30 percent of total program funds (funds and administrative reimbursements), or (4) the sum of the number of homes sponsored times the administrative cost reimbursement rates shown in table 39.

The legislative changes enacted under PRWORA do not affect sponsors' administrative payment levels, but do add new responsibilities. Sponsors have been given primary responsibility for classifying providers as tier I or tier II. In addition, for tier II homes seeking reimbursement at the tier I level for individual children, sponsors are responsible for administering the income test. Parents send income verification forms directly to sponsors, who then determine whether the household income is below 185 percent of the poverty guideline. Providers are notified of the number of children approved for the higher reimbursement rates but not the names of the children approved.

A congressionally mandated study of the effect of tiering found that the legislative change achieved desired objectives: The number of low-income children served in CACFP homes grew by 80 percent between 1995 and 1999, and the number of meal reimbursements for low-income children doubled (Hamilton et al., 2001). Moreover, tiering had no adverse effect on either the number or nutritional characteristics of meals and snacks offered by tier II providers (Crepinsek et al., 2002).

Other Recent Program Changes

In 1998, the Child Nutrition Reauthorization Act (P.L. 105-336) expanded institutional eligibility for the child component of the CACFP to include after-school care programs and homeless shelters. To be eligible for participation, after-school programs must be located in geographic areas where 50 percent or more of the children enrolled in school are eligible for free or reduced-price meals in the NSLP. They must also provide regular, structured activities for children, including educational and enrichment activities (USDA/FNS, 2002b). Snacks are served free of charge, and providers are reimbursed at the free snack rate for all snacks provided. Reimbursement is limited to one snack per child per day on school days, weekends, or holidays during the school year.

P.L. 105-336 also added homeless shelters to the list of institutions eligible to participate in the CACFP. The participation of homeless shelters grew out of a demonstration project (the Child Nutrition Homeless Demonstration) that was authorized by P.L. 101-147 (the Child Nutrition and WIC Reauthorization Act of 1989). The purpose of the demonstration was to determine the best means of providing year-round food assistance to homeless preschool children residing in emergency shelters (Macro International, 1991). Sites selected for the demonstration provided free meals and snacks to children, following CACFP meal pattern guidelines, and received standard CACFP reimbursements.

Table 38—Meal reimbursement rates for homes and centers, July 1, 2002-June 30, 2003[1]

	Family child care homes		Child or adult care centers		
Meal	Tier I rate	Tier II rate	Paid	Reduced	Free
	Dollars				
Breakfast	0.98	0.37	0.22	0.87	1.17
Lunch/supper	1.80	1.09	.20	1.74	2.14
Supplement (snack)	.53	.14	.05	.29	.58

[1]Meal reimbursement rates are higher in Alaska and Hawaii.

Table 39—Monthly administrative cost reimbursement rates for sponsors of family child care homes, July 1, 2002-June 30, 2003[1]

Number of homes	Rate per home
	Dollars
Initial 50 (homes 1-50)	84
Next 150 (homes 51-200)	64
Next 800 (homes 201-1,000)	50
All additional (homes 1,001 and over)	44

[1]Administrative cost reimbursement rates are higher in Alaska and Hawaii.

The demonstration ran for 4 years (FY 1990 through FY 1994) and increased from one sponsor and four shelters serving approximately 22,000 meals (Macro International, 1991) to 59 sponsors and 81 shelters serving more than 700,000 meals and snacks (Fox and Cutler, 1996). Because the demonstration showed the feasibility of providing USDA-reimbursed meals and snacks in a variety of homeless shelters, the Homeless Child Nutrition Program was established as a permanent program in 1994 (P.L. 103-448). The program was incorporated into the CACFP in July 1999.

In 2000, the Agricultural Risk Protection Act (P.L. 106-224) expanded institutional eligibility for the CACFP to include some for-profit child centers. Eligibility was extended to centers where 25 percent or more of enrolled children (or 25 percent of licensed capacity, whichever is less) are eligible for free and reduced-price meals.[111] Initially, the timeframe for this temporary provision was December 21, 2000, to September 30, 2001. In 2001, under P.L. 107-76, the timeframe was extended through September 2002. In 2003, the FY 2003 appropriations bill extended the timeframe through September 2003 (Garnett, 2003).

Finally, in 2000 and 2001, after-school care programs in seven States (Delaware, Michigan, Missouri, New York, Oregon, Pennsylvania, and Illinois) were authorized to provide supper to participating children (USDA/FNS, 2002c).

Adult Day Care Component

In 1987, as a means of increasing support for elderly feeding programs, P.L. 100-175 amended the Older Americans Act to mandate that the CCFP be expanded to allow eligible adult day care centers to participate. Centers that provide day care services to persons age 60 or older or to functionally impaired persons 18 and older are eligible to participate in the program. Eligible centers have the option of participating in the CACFP or in the Nutrition Services Incentive Program (discussed in chapter 10), but cannot receive reimbursement under both programs for the same meal. Participation in the adult component of the CACFP has increased steadily over time, after a period of rapid growth in the early years of operation. Since 1988, the number of meals served to adults has increased from 2 million to approximately 45 million (USDA/FNS, 2003).

The adult component of the CACFP operates in essentially the same manner as the child care *center* component of the program. Adult day care centers are reimbursed for meals and snacks served to participating adults, using the same income-eligibility criteria and reimbursement rates as participating child care centers. Moreover, meals and snacks served in adult day care centers must meet CACFP meal pattern requirements to qualify for Federal reimbursement.

Review of Research on the Child Care Component of the CACFP

To date, no research has examined the impact of the child care component of the CACFP on participants' nutrient intake or other nutrition- and health-related outcomes. Ten descriptive studies of the child care component of the CACFP were identified. Characteristics of these studies are summarized in table 40.

Seven studies, four of which were national in scope, examined the nutrient content of meals and snacks offered in child care centers and/or homes participating in the CACFP. Six studies assessed children's nutrient intake from CACFP meals. Four of these studies looked at nutrient intake both in and out of care, so were able to describe the contribution of CACFP meals and snacks to children's overall diets.

Only one study, the first national study of the program, examined meals and snacks offered to nonparticipating children—that is, to children receiving care in child care centers that did not participate in the CACFP (Glantz and O'Neill-Fox, 1982). This study, which was completed when the program was still focused exclusively on children, is described next.

Evaluation of the Child Care Food Program

The 1978 Child Nutrition Amendments (P.L. 95-627) directed USDA to study meal quality in day care centers and homes that participated in the CCFP. The study examined the nutrient content and nutrient density of meals and snacks offered in participating and nonparticipating child care centers as well as the quality and variety of foods offered (Glantz and O'Neill-Fox, 1982). The study also described meals and snacks offered in participating homes, but did not include a sample of nonparticipant homes.[112]

[111] For-profit centers that either have tax-exempt status or receive Title XX funding for 25 percent or more of enrolled children have long been eligible for CACFP participation. The provision in P.L. 106-224 did not affect eligibility of so-called "Title XX centers."

[112] The study was unable to identify a sample of eligible nonparticipating homes.

Table 40—Studies that examined the nutrient content of meals and snacks offered in the Child and Adult Care Food Program and/or the nutrient contribution of meals and snacks consumed by program participants

Study	Measure(s)	Sample	Data collection method
Studies of the child care component of the CACFP: Nutrient content of meals and snacks offered			
Crepinsek et al. (2002)	Nutrient content of menus relative to RDAs Compliance with the *Dietary Guidelines for Americans*	Nationally representative sample of 542 tier II homes	Self-administered menu forms for a 5-day period
Fox et al. (1997)	Nutrient content of menus relative to RDAs Compliance with the *Dietary Guidelines for Americans*	Nationally representative sample of 1,962 centers and homes	Self-administered menu forms for a 5-day period
Briley et al. (1993)	Nutrient content of menus relative to RDAs Compliance with the *Dietary Guidelines for Americans*	Nationally representative sample of 171 centers	Self-administered menu forms for a 10-day period
Drake (1992)	Nutrient content of menus relative to RDAs	46 randomly selected centers in Kansas City area	Self-administered menu forms for a 10-day period
Briley et al. (1989)	Nutrient content of menus relative to RDAs Frequency of foods served	Convenience sample of 40 centers in Texas	Self-administered menu forms for 3 10-day periods
Domer (1983)	Nutrient content of menus relative to RDAs	Convenience sample of 1 center in North Carolina	Self-administered menu forms for a 20-day period
Glantz and O'Neill-Fox (1982)	Nutrient content of menus relative to RDAs Nutrient density of menus Food quality and variety	Nationally representative samples: ▪ 100 participating centers ▪ 64 nonparticipating centers ▪ 60 participating homes	Self-administered menu forms for a 3-day period
Studies of the child care component of the CACFP: Children's nutrient intake from CACFP meals and snacks			
Crepinsek and Burstein (2003)	In-care nutrient intake relative to RDAs, and *Dietary Guidelines for Americans* Out-of-care nutrient intake relative to RDAs and *Dietary Guideline for Americans*	Nationally representative sample of 336 homes and centers	In-care observations for 2 nonconsecutive days 24-hour recall with parent for 2 nonconsecutive days
Fox et al. (1997)	In-care nutrient intake relative to RDAs and *Dietary Guidelines for Americans*	Nationally representative sample of 372 centers and homes	In-care observations for 2 nonconsecutive days

See notes at end of table.

Continued—

Table 40—Studies that examined the nutrient content of meals and snacks offered in the Child and Adult Care Food Program and/or the nutrient contribution of meals and snacks consumed by program participants—Continued

Study	Measure(s)	Sample	Data collection method
Briley et al. (1993)	In-care nutrient intake relative to RDAs, *Dietary Guidelines*, and Food Guide Pyramid Out-of-care nutrient intake relative to RDAs, *Dietary Guidelines*, and Food Guide Pyramid	Convenience sample of 12 centers in central Texas, 6 centers with "strong" menus, and 6 centers with weaker menus	In-care observations for 3 consecutive days Parent-maintained food records for 3 consecutive days
Drake (1992)	In-care nutrient intake relative to RDAs	Convenience sample of 4 centers in Kansas City	In-care observations for 10 consecutive days
Drake (1991)	In-care nutrient intake relative to RDAs Out-of-care nutrient intake relative to RDAs	Convenience sample of 1 center in Kansas City	In-care observations for 5 consecutive days 24-hour recall with parent for 5 consecutive days
Glantz et al. (1983)	In-care nutrient intake relative to RDAs Out-of-care nutrient intake relative to RDAs	Convenience sample of 20 centers in Boston area	In-care observations for 5 consecutive days 24-hour recall with parent for 5 consecutive days
Studies of the adult care component of CACFP			
Ponza et al. (1993)	Frequency of foods served In-care nutrient intake relative to RDAs Out-of-care nutrient intake relative to RDAs	Nationally representative sample of 85 adult day care centers and 942 participating adults	Self-administered menu forms for a 5-day period In-care meal observations and 24-hour recall

Design

The study used a quasi-experimental design to evaluate the effects of CCFP participation on the quality of meals offered to children in child care. Data were collected from a nationally representative sample of 100 participating child care centers, 60 eligible nonparticipating centers, and 62 participating family child care homes. A one-day site visit was completed in each child care center and home. The preparation and service of one lunch and one snack were observed. Data on the portions of food served to children were obtained using a "plate game," a technique that obtained actual gram weights of the portions served to or taken by a sample of six children. These data were used to impute portion sizes for an analysis of three daily menus from a randomly selected week.

The main limitation of this design is the potential for selection bias; child care centers and homes that were more concerned with nutrition (and therefore more likely to offer nutritious meals and snacks) may have been more likely to choose to participate in the CCFP than other child care providers. The analysis did not attempt to control for selection bias or for measured differences that may have existed between participating and nonparticipating providers. Results of the study are based on simple comparisons of the nutritional characteristics of meals and snacks offered by participating and nonparticipating providers. Data were tabulated separately for participating homes and participating centers.

Findings

Three measures were used to assess the nutritional characteristics of meals and snacks offered by participating and nonparticipating providers: nutrient content; nutrient density; and food quality and variety. On each of these measures, participating centers and homes scored significantly higher than nonparticipating centers.

Nutrient Content. Compared with nonparticipating centers, both participating centers and homes offered meals and snacks that provided a significantly greater proportion of the Recommended Dietary Allowances (RDAs) for food energy and for all nutrients examined except vitamins A and C.[113] At least part of this difference was due t CCFP centers and homes serving breakfast much more frequently than nonparticipating centers. Nonparticipating centers tended to offer a morning snack rather than a complete breakfast.

Nutrient Density. Nutrient density scores were used to assess the overall quality of the meals and snacks offered.[114] With the exception of the score for vitamin C, nutrient density scores for meals and snacks offered in participating centers and homes were significantly higher than scores for meals and snacks offered in nonparticipating centers. That is, participating centers and homes served meals and snacks that provided significantly greater amounts of nutrients, relative to food energy.

Food Quality and Variety. Three daily menus were scored using an index that reflected the quality of foods served (for example, fresh fruits or vegetables vs. canned fruits or vegetables), as well as the variety of foods served within major food groups. Mean scores on this index were significantly greater for participating centers and homes than for nonparticipating centers. Differences were noted for both the quality and variety components of the index.

Limitations

Findings reported by Glantz and O'Neill-Fox (1982) should be treated with some caution because the data are now considerably out of date. Many market and legislative changes may have affected characteristics of foodservice programs in both participating and nonparticipating centers and homes. Moreover, as just noted, the study did not attempt to deal with potential selection bias or adjust for any differences in measured characteristics that may have existed between the participant and nonparticipant groups. This allows for the possibility that the differences observed between participating centers and homes and nonparticipating centers were a reflection of a greater interest in or focus on nutrition among CCFP providers than among other providers.

Other Studies of the Child Care Component of the CACFP

All of the other studies identified for this review examined the nutrient content of meals and snacks offered to or consumed by participating children but did not assess program impact—that is, the studies did not include comparisons to nonparticipating institutions or children.

[113]Meals and snacks offered in participating centers and homes were also higher in vitamins A and C. These differences, however, were not statistically significant.

[114]Nutrient density scores were calculated for each nutrient examined using the following equation: percent RDA for nutrient/percent RDA for food energy. A score of less than 1 indicated a suboptimal ratio of nutrients to calories.

CACFP regulations and guidance materials provide only broad standards for meals and snacks offered under the program. In the absence of specific nutrient-based standards, most of these descriptive studies used the recommendations of the American Dietetic Association (ADA) (1994, 1999) as a benchmark for assessing the nutrient content of CACFP meals and snacks. The ADA recommends that children in care for 8 or more hours per day receive food that provides at least one-half to two-thirds of their daily needs for energy and nutrients (based on age-appropriate RDAs). In addition, the ADA recommends that meals and snacks be consistent with the *Dietary Guidelines for Americans*.

Findings from the identified studies must be interpreted in light of shifting program policies regarding the maximum number of meals and snacks eligible for reimbursement. Before 1981, participating centers and homes could be reimbursed for up to two meals and two snacks per day for each child in care. The previously described evaluation of the CCFP (Glantz and O'Neill-Fox, 1982) collected data when this policy was in effect.

The 1981 Omnibus Budget Reconciliation Act (P.L. 97-35) limited reimbursements to a maximum of two meals and one snack per child per day. Following this change, there was a marked reduction in the number of child care centers that offered a morning snack (Glantz et al., 1988). Three of the identified studies are based on data collected while this policy was in place (Briley et al., 1989; Domer, 1983; and Glantz et al., 1983).

The policy governing maximum reimbursements was changed again, in 1988, when Congress allowed child care providers to be reimbursed for an additional meal or snack for children in care 8 or more hours per day (P.L. 100-435). Thus, findings from five of the most recent studies (Crepinsek and Burstein, 2004 (which used data collected as part of the study reported on by Fox et al., 1997); Fox et al., 1997; Briley et al., 1993; and Drake, 1991 and 1992) reflect a program that allowed providers to be reimbursed for up to two meals and two snacks per child per day.

Under the 1996 PRWORA reforms, the so-called "fourth-meal provision" was eliminated again. Today, CACFP providers can be reimbursed for a maximum of one meal and two snacks or two meals and one snack per child per day, regardless of how long the child is in care. The most recent study of CACFP meals and snacks (Crepinsek et al., 2002) collected data in 1999, after the PRWORA change had been implemented.

Nutrient Content of CACFP Meals and Snacks Offered to Participating Children

Seven studies described the nutrient content of meals and snacks offered to participating children (table 40).[115] During periods when USDA allowed providers to be reimbursed for up to two meals and two snacks per day for each child in care, most studies found that, on average, the combinations of meals and snacks commonly offered to children provided them with the opportunity to obtain at least 50 percent of their daily energy and nutrient needs (Fox et al., 1997; Drake, 1992; Glantz and O'Neill-Fox, 1982).[116] Briley et al. (1993) reported similar results but found that meals and snacks offered to children in full-time care fell short of the ADA's recommendation for food energy, iron, and niacin, a finding that may have been an artifact of the way the data were analyzed.[117]

Two studies conducted during a time when CACFP reimbursements were limited to two meals and one snack per child per day (Briley et al., 1989; Domer, 1983) found that the full complement of meals and snacks offered in participating centers provided less than 50 percent of the RDAs for food energy and several nutrients.

The most recent study of CACFP meals, based on data collected in 1999 and limited to tier II family child care homes, found that, on average, the mean nutrient content of the most common combinations of meals and snacks offered (breakfast, lunch, and one snack and breakfast, lunch, and two snacks) satisfied the ADA guidelines for full-time care (Crepinsek et al., 2002). Indeed,

[115]Most of the available studies provide separate results for different meals (breakfast and lunch) as well as for snacks. This discussion is limited to findings related to the full complement of meals and snacks offered, relative to the ADA recommendations.

[116]Glantz and O'Neill-Fox (1982) reported that, on average, the total diets (all meals and snacks combined) offered in participating child care centers contributed only 48 percent of the RDA for iron (which is just below the ADA recommendation of 50 percent for children in care 8 or more hours per day). However, methodology used in this study was different from that of all other studies examined here. In computing the mean percentage of the RDAs, levels exceeding 100 percent for any individual provider were truncated to 100 percent. While this approach had the intended effect of minimizing the effects of excessively high levels on the mean values, it also understated the true mean values. One can probably safely assume that, in the absence of this truncation, the true mean percentage of the RDA for iron would have exceeded the 50 percent benchmark established by the ADA.

[117]The study analyzed the nutrient content of the average menu offered in each center but did not directly measure quantities of food offered or served. Rather, the analysis assumed that centers served the amounts specified in program regulations, which probably resulted in an underestimation of the energy and nutrient content of the meals offered. Fox et al. (1997) found that portion sizes actually served to or taken by children are generally equivalent to or greater than the minimum portion sizes specified in the CACFP regulations.

researchers demonstrated that the mean nutrient content of meals and snacks offered in 1999 did not differ significantly from the mean nutrient content of meals and snacks offered by similar providers in 1995 (based on data from Fox et al., 1997).[118] This was true despite the elimination of the "fourth meal" provision and reduction in reimbursement rates for tier II providers by PRWORA. Findings may differ for other types of providers (who were affected by the change in the maximum number of meals and snacks eligible for reimbursement but not the change in reimbursement rate).

As noted, the ADA recommends that meals and snacks offered in the CACFP be consistent with the *Dietary Guidelines*. Under current program regulations, CACFP providers are not required to meet these standards. Indeed, the applicability of the *Dietary Guidelines* to the diets of children between the ages of 2 and 5 has been somewhat controversial over the years.

The 1995 edition of the *Dietary Guidelines* stated that recommendations for total fat and saturated fat did not apply specifically to children between the ages of 2 and 5. Rather, the recommendation was that "after [2 years], children should gradually adopt a diet that, by about 5 years of age, contains no more than 30 percent of calories from fat" (USDA and U.S. Department of Health and Human Services (HHS), 1995).

The most recent edition of the *Dietary Guidelines*, released in 2000, takes a firmer stand on this issue and states specifically that advice about intake of total fat, saturated fat, and cholesterol "applies to children who are 2 years of age and older" (USDA/HHS, 2000).

The two largest and most recent studies of CACFP meals and snacks compared the nutrient content of the combinations of meals and snacks most commonly offered by CACFP providers with *Dietary Guidelines* and associated recommendations for total fat, saturated fat, cholesterol, and sodium. Fox and colleagues (1997) limited their analysis to meals and snacks offered to children age 5 and older; at the time, the 1995 edition of the *Dietary Guidelines* was in effect. Results showed that the two most common combinations of meals and snacks offered to children in this age group met or approximated the *Dietary Guidelines* recommendation for fat but exceeded the recommendation for saturated fat. In addition, meals and snacks were high in sodium, relative to energy contributions. While the two most common combinations of meals and snacks provided 61-71 percent of children's daily energy needs, on average, they provided 68-75 percent of the recommended daily maximum of sodium.

The most recent study of CACFP meals and snacks, completed in 2002, examined meals and snacks offered to children age 2 and older (Crepinsek et al., 2002). Results were similar to those reported by Fox et al. in 1997. For the two most common combinations of meals and snacks offered (the same combinations assessed by Fox et al., 1997), mean saturated fat content exceeded the *Dietary Guidelines* recommendation and mean sodium content was high, relative to energy content. As noted, however, this study included only tier II homes, so findings may be different for other types of providers.

Nutrient Content of CACFP Meals and Snacks Consumed by Participating Children

The nutrient profile of meals and snacks actually consumed by participating children may differ from the nutrient profile of meals and snacks offered by providers. For example, children may decline one or more of the foods offered, children may select portions that differ from that of the average portion, or children may waste (not consume) some of the food they take. Thus, to gain a full understanding of the contributions of CACFP meals and snacks to children's energy and nutrient needs, one must examine the nutrient content of CACFP meals and snacks actually consumed by children.

As summarized in table 40, six studies examined the nutrient content of the meals and snacks *consumed* by children while in care. Four of these studies have limited generalizability because they used small convenience samples (Briley et al., 1993; Drake, 1992 and 1991; and Glantz et al., 1983).

The most recent comprehensive study of the CACFP, the Early Childhood and Child Care Study (Fox et al., 1997), included meal observations in a nationally representative sample of 372 participating child care centers and homes. Observations were completed for 1,347 children between the ages of 1 and 10. Children generally selected portions of food that were equivalent to or greater than the minimum portion sizes specified in the CACFP meal pattern requirements and generally consumed between 70 and 80 percent of the portions taken.

Among children in care 8 or more hours per day, CACFP meals and snacks provided about 50 percent

[118] One significant difference was detected for the combination of breakfast, lunch, and two snacks. The mean energy content of this combination of meals and snacks was greater in 1999 than 1995.

of daily needs for energy and iron.[119] Intake of calcium from CACFP meals and snacks approximated, on average, three-quarters of the RDA. Average intakes of protein, vitamin A, and vitamin C exceeded 100 percent of the RDA.

Comparisons of intakes to *Dietary Guidelines* recommendations were limited to 5-year-olds because of the 1995 *Dietary Guidelines* restriction and limited samples of older children in full-time care. Findings showed that CACFP meals and snacks consumed by these children provided more than the recommended amounts of both total fat and saturated fat. In addition, intake of sodium from CACFP meals and snacks was high, relative to energy intake.

The Early Childhood and Child Care Study also attempted to describe the relative contribution of CACFP meals and snacks to children's 24-hour intakes. The analysis was ultimately abandoned, however, because of low response rates. The study methodology called for two 24-hour recalls on nonconsecutive days made up of in-care observations for foods consumed in care, as well as telephone interviews with parents for foods consumed at home. Because of difficulties in reaching parents to complete the interview about at-home consumption within 48 hours of the in-care observations, complete data were obtained for a relatively small percentage of the sample (roughly 40 percent).

Crepinsek and Burstein (2004) recently analyzed the 24-hour recall data from the Early Childhood and Child Care Study. A nonresponse analysis revealed that, although children with complete and incomplete dietary recall information were similar on a number of key variables, children with complete information were more likely than those with incomplete information to be from households with incomes equal to or greater than 185 percent of poverty. At the same time, children with complete and incomplete information were quite similar with regard to the meals and snacks they were observed to eat in care. Moreover, mean intakes of food energy and key nutrients at each eating occasion differed little between the two groups. The authors concluded that, with the use of sampling weights to correct for discrepancies between the two groups to the extent possible, the description of CACFP contributions to total nutrient intakes based solely on respondents with complete information is not greatly distorted. Key findings from this analysis, which was not limited to children in full-time care, are summarized here.

For toddlers and preschoolers, CACFP meals and snacks contributed 36-47 percent of daily energy needs and 45 to more than 100 percent of the RDA for key nutrients. CACFP meals and snacks made smaller contributions to the intakes of school-age children (ages 6-10) because they spend fewer hours in care (typically 3 hours per day). For food energy and iron, the CACFP contribution for 6-10-year-olds was less for children who received care in centers than for children who received care in family child care homes. The authors attribute this difference to the fact that children receiving care in homes are likely to be offered breakfast and lunch, whereas, children receiving care in centers are likely to be offered snacks (as reported by Fox et al., 1997).

CACFP meals and snacks did not contribute disproportionately to children's daily intake of fat, saturated fat, or sodium, although total daily intakes of children ages 3-10 exceeded recommendations for intake of all these nutrients. CACFP meals and snacks provided more than the recommended level of saturated fat, as a percentage of total food energy.

Children's 24-hour intakes of food energy, protein, vitamins A and C, calcium, iron, and zinc met or exceeded the RDAs. The one exception was energy intake among children ages 6-10 in child care centers. For this group of children, mean daily intake of food energy was equivalent to 87 percent of the RDA.

Review of Research on the Adult Care Component of the CACFP

To date, only one study has examined the adult day care component of the CACFP (Ponza et al., 1993). Although this descriptive study of the meals and snacks served by participating adult day care centers compared the characteristics of participating and nonparticipating centers, it did not collect menu or dietary intake information from nonparticipating centers. The study collected menu information for a 1-week period from a nationally representative sample of 85 adult day care centers participating in the CACFP as well as information on foods consumed over a single 24-hour period by a random sample of 942 adults attending these centers.

The study did not analyze the nutrient content of the meals and snacks served in participating centers. The

[119]This analysis was limited to children under the age of 6 because older children were seldom in care 8 or more hours per day.

analysis of menus was limited to tabulations of the types and frequencies of foods offered. The study described dietary intakes of CACFP participants and assessed the contribution of program meals to total daily intake. The authors examined the percentage of the RDA for food energy and key nutrients consumed during the day as part of CACFP reimbursable meals and snacks, as well as the percentage of total daily intake supplied by CACFP meals and snacks.[120] The study also compared the composition of CACFP meals and snacks consumed by participants with the *Dietary Guidelines for American*s.

On average, total nutrient intake from all CACFP meals and snacks supplied 42 percent of the RDA for food energy and between 52 percent (iron) and 83 percent (vitamin C) of the RDAs for key nutrients. Taken together, CACFP reimbursable meals and snacks consumed by participating adults contributed about one-half of their total daily intake.

The study also found that CACFP meals and snacks consumed by participants were not consistent with the *Dietary Guidelines for Americans*. On average, the percentage of food energy derived from fat (33 percent) and saturated fat (11 percent) exceeded the recommended levels of no more than 30 percent and less than 10 percent, respectively.

Summary

Very little solid information exists concerning the impact of the CACFP on the nutrition and health outcomes of participating children and adults. Only one study has attempted to compare CACFP conditions with the conditions that would exist in the absence of the program. That study (Glantz and O'Neill-Fox, 1982) provides some evidence that the program improves the quality of meals and snacks served to children in participating child care centers and homes, but the evidence has substantial limitations. First, it is quite dated. Since the time that this study was completed, program regulations, program participation, and national patterns of child care use have changed significantly. Second, the study looked at only the characteristics of meals offered and did not examine children's actual intakes from CACFP meals and snacks. Finally, the study design is potentially vulnerable to selection bias.

Other available research is less outdated but is even more limited than the 1982 study in the sense that it includes no representation of the conditions that would exist in the absence of CACFP. Moreover, the most recent comprehensive descriptive study of the child care component of the program (Fox et al., 1997) is based on data that were collected when providers were eligible to receive reimbursement for an additional meal or snack for children in care 8 or more hours per day. Subsequent research has indicated that this change had no impact on the number or nutritional quality of meals and snacks served in tier II homes (Crepinsek et al., 2002), but information is lacking for other types of providers.

Similarly, the one study that has been completed on the adult component of the program was entirely descriptive in nature and offers no comparison of participants' nutrient intakes to those that would exist in the absence of the CACFP.

Thus, the existing literature leaves unanswered the fundamental question of how the CACFP affects the nutrient intake and other nutrition- and health-related outcomes of participating children and adults. The need for an answer to this question will become increasingly important as child care use patterns continue to evolve and the population continues to age.

Addressing the question of program impact will almost certainly require new, special-purpose research. The national surveys that measure nutrient intake in detail, such as the National Health and Nutrition Examination Survey (NHANES) and the Continuing Survey of Food Intake of Individuals (CSFII), do not provide reliable indicators of institutional participation in the CACFP and do not even ask about meals that might be consumed in family child care homes. Thus, identifying a valid sample of CACFP and non-CACFP participants from these existing databases is unlikely.

In addition to collecting primary data, future studies of the CACFP should do the following:

- Use dietary assessment methods that measure usual energy and nutrient intake of participating children and/or adults, children and adults attending nonparticipating day care programs, and, perhaps, children and adults who are not in care.
- Look at meals and snacks offered by participating and nonparticipating programs and the foods actually consumed by attending children and adults.

[120]Program regulations limit CACFP reimbursements to a maximum of two meals and one snack per day. Some adults in participating centers consume an additional snack while in care that is not reimbursable.

- Examine the impact of the CACFP on the total dietary intake of participants over the full day, including food consumed in care and food consumed outside of care.

Given the problems encountered in the Early Childhood and Child Care Study—low response rates because of difficulties in obtaining information from parents about food consumed at home to couple with information obtained during in-care observations—special attention will need to be paid to the methodology used to collect complete dietary recall information for children.[121]

[121]This problem has been encountered in other studies that used a comparable methodology. For example, the first School Nutrition Dietary Assessment Study (SNDA-I) (Burghardt et al., 1993, chapter five) achieved response rates of 30-35 percent for students in grades 1 and 2 where parent interviews were used to collect information on out-of-school food consumption for a single 24-hour period.

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Chapter 8

Summer Food Service Program

The Summer Food Service Program (SFSP) provides funds to eligible organizations to serve nutritious meals and snacks, free of charge, to children at approved feeding sites. The program operates mainly during the summer when schools are not in session and the National School Lunch Program (NSLP) and School Breakfast Program (SBP) are not available. Organizations eligible to sponsor feeding sites include public or private nonprofit schools; local government agencies; nonprofit community organizations, such as YMCAs, Boys and Girls Clubs, churches, National Youth Sports Programs (NYSP), and residential camps.[122] Because the SFSP is an entitlement program, no eligible sponsor may be denied funding.

Research on the SFSP has been entirely descriptive in nature. Much of it has focused on describing program operations and characteristics of sponsoring organizations. Two national studies of the program (Gordon and Briefel, 2003; Ohls et al., 1988) assessed the nutrient content of meals served in the SFSP but did not examine the contribution of SFSP meals to students' daily nutrient intakes or make any comparison to nutrient intakes of nonparticipants.

Program Overview

The SFSP was created to ensure that low-income children would have access to nutritionally balanced meals when school is not in session. The program was created in 1968 as a 3-year pilot project and was permanently authorized as an entitlement program in FY 1975.

In most States, the SFSP is administered by State education agencies, the entities that oversee the NSLP and SBP. Locally, the SFSP is operated by approved sponsoring organizations, which include school districts, State or local government agencies, churches, private nonprofit residential camps, and community organizations. Sponsors provide free meals at one or more feeding sites.

Feeding sites may be either "open sites," "enrolled sites," or camps (U.S. Department of Agriculture (USDA), Food and Nutrition Service (FNS), 2002a). Open sites are those located in areas where at least 50 percent of the children are from households with incomes at or below 185 percent of poverty (making them eligible for free or reduced-price meals in the NSLP or SBP). Open sites are required to be open to provide food to all children in the neighborhood, regardless of their enrollment in site-sponsored activities. Enrolled sites are those in which 50 percent of the children enrolled in a program or activity offered at the site are eligible to receive free or reduced-price meals, based on individual applications. Camp sites are residential summer camps. Camp sites receive reimbursement only for meals served to children whose documented household income makes them eligible for free or reduced-price meals.

Children up to age 18 or older, if they participate in a program for mentally or physically handicapped individuals, are eligible to receive meals. Lunch is, by far, the most frequently served meal in the SFSP. However, sponsors may also offer breakfast, supper, and/or snacks. Most children receive one or two meals per day. Residential camps and sites that serve migrant children may serve (and be reimbursed for) up to three meals. To receive Federal reimbursement, SFSP sites must serve meals and snacks that meet defined meal patterns, similar to those used in the NSLP (see chapter 5) and the SBP (see chapter 6).

Sponsors receive two types of reimbursement for each meal served, and reimbursement rates vary by type of meal. The largest reimbursement is for operating or foodservice costs ($2.35 per lunch or supper served in the summer of 2003). Sponsors also receive an additional per meal reimbursement to cover administrative costs ($0.2475 per lunch or supper in self-preparation or rural sites and $0.2050 per lunch or supper in all other sites). Funds received through these two reimbursement streams are not fungible and sponsors must monitor their costs very closely to ensure that reimbursements fully cover their costs (USDA/FNS, 2002b).

In FY 2002, the SFSP operated in approximately 30,000 feeding sites nationwide and served about 122 million meals (USDA/FNS, 2003). During peak operation in July 2002, the program served

[122]NYSPs are Federally funded sports camps for low-income children. Programs are administered by colleges and universities.

approximately 1.9 million children per day.[123] The total FY 2002 Federal cost for the SFSP was $263 million.

Program History

During its first year of operation (FY 1975), the SFSP served meals to an average of 1.79 million children each day. Over the next 2 years, the program grew to serve more than 2.8 million children per day. Program growth was sharply curtailed in 1981, however, when the 1981 Omnibus Reconciliation Act (OBRA) eliminated private nonprofit sponsors other than schools and residential camps. This action was taken because a 1977 report issued by the U.S. General Accounting Office (GAO) described extensive program abuses by these sponsors (Ohls et al., 1988). In addition, the OBRA legislation restricted use of foodservice management companies and other vendors, expanded program monitoring and administration, and tightened eligibility requirements. Prior to OBRA, the criteria used to define area eligibility for feeding sites were 30 percent of children from low-income households. OBRA increased this threshold to the 50-percent standard that is currently in use.

After implementation of the OBRA reforms, SFSP participation decreased substantially. In 1985, the program served 1.5 million children per day, roughly half as many as had been served in 1977. The precipitous decrease in participation led to renewed concerns, particularly among child welfare advocacy groups, that low-income children were going without needed nourishment during the summer. Advocates concerned about rural hunger raised concerns that rural areas had particularly low participation rates and greater barriers to participation by sponsor organizations (Shotland and Loonin, 1988).

In the mid- to late 1980s, several pieces of legislation were passed with the aim of increasing children's access to and participation in the SFSP. In 1989, the OBRA restriction on private nonprofit organizations was reversed and these organizations were again eligible to serve as program sponsors. This change resulted in an increase in the number of feeding sites available and, consequently, the number of children served. Between 1989—when the change went into effect—and 1993, the number of children served by the SFSP increased by about 30 percent (Food Research and Action Center (FRAC), 1993).[124]

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) included many amendments to SFSP operations. Although most amendments were designed to increase the efficiency of program administration, PRWORA also included language that removed program expansion as a stated goal, reduced per meal cash subsidies by roughly 10 percent, and eliminated the subsidy for a fourth meal that had previously been provided to some sponsors. The GAO studied the effects of these changes and reported no deleterious effect on the number of participating sponsors and children (Robinson, 1998). State-level administrators did report, however, that the program may have been affected in other ways—for example, a reduction in the number of feeding sites, meaning children may have had to travel further to get to a site, and/or in the number of food items provided to participating children (Robinson, 1998).

In the years since PRWORA, concerns about the number of low-income children who go without Federal meal benefits during the summer have continued to escalate. In describing the problem, Under Secretary of Agriculture Eric M. Bost pointed out that the 2 million SFSP meals served per day in FY 2000 represented only about 12 percent of the free and reduced-price meals served each day during the regular school year through the NSLP (Bost, 2000). Bost deemed this level of SFSP participation, which reached "only a fraction of eligible children," to be "unreasonably low."[125]

In keeping with this overarching concern, the most recent legislative changes in the SFSP have focused on increasing program penetration by attracting more program sponsors, particularly school districts. In 2000, USDA implemented several changes designed to eliminate and streamline paperwork requirements for sponsors. In addition, in late 2000, P.L. 106-554 (The Consolidated Appropriations Act) authorized a special

[123]An additional 1.6 million children per day received summer meals through the NSLP as part of summer school programs or year-round schools (based on reported NSLP participation for July 2002 (USDA/FNS, 2003)).

[124]The Food and Nutrition Service (FNS) studied private, nonprofit sponsors after this change was implemented and concluded that the change was successful in leading to an overall increase in program participation as well as to increased coverage of rural areas. The study also found that private, nonprofit sponsors administered the program as effectively as public sponsors (Decker et al., 1990).

[125]There are several reasons that SFSP participation is lower than NSLP participation. One is that open SFSP sites must be located in low-income neighborhoods, whereas the NSLP is available everywhere. Another is that attendance at SFSP sites is voluntary, while children must attend school during the year (Gordon and Briefel, 2003). In addition, systems that transport students to schools during the normal school year are generally not operational during the summer.

pilot project to increase the number of children participating in the SFSP in Puerto Rico and 13 States with low SFSP participation rates (Garnett, 2001; FRAC, 2001).[126] The pilot project was initially authorized to operate from FY 2001 through FY 2003 and was extended by Congress through March 31, 2004. It simplified recordkeeping and reporting requirements and provided sites with the maximum per-meal reimbursement for both operating (foodservice) and administrative cost reimbursements. Moreover, pilot sites were allowed to use funds from each reimbursement stream to cover excess costs associated with the other reimbursement stream. (As described previously, under current program regulations, reimbursements for operating and administrative costs are strictly separate.)

Analysis by FRAC (FRAC, 2003) and USDA's Food and Nutrition Service (Singh and Endahl, 2004) indicates that States participating in the pilot successfully increased SFSP participation. Singh and Endahl found that, in all 14 States combined, the number of SFSP sponsors increased by 18 percent between July 2000 and July 2003, and average daily participation increased by 43 percent. Impacts varied substantially across States, however, and based on July 2003 data, many pilot States continued to have low SFSP participation relative to other States. Evaluation of the pilot impacts was complicated by other SFSP initiatives (described below) that were implemented during the same period.

Before the start of SFSP activities for summer 2002, USDA implemented several regulatory changes designed to facilitate program participation at the sponsor level, thereby increasing the number of children reached through the SFSP. The most significant change was the nationwide implementation of "seamless summer waivers" for school districts that operate the NSLP (USDA/FNS, 2002c). The waivers, which will run through FY 2004, allow school districts to offer the SFSP without having to deal with previously required paperwork and administrative requirements. School districts operate the SFSP in essentially the same manner as they operate the NSLP. All meals served at waiver sites are claimed as NSLP meals and are reimbursed at the NSLP free meal rate, which is slightly lower than the SFSP rate. However, program administrators do not have to deal with the administrative burden associated with operating two different programs. To receive a waiver, school districts must be approved by their State agencies and must qualify as either an open site or an enrolled site under SFSP regulations (see previous discussion).

Tasse and Ohls (2003) studied early reaction to and effects of seamless waivers. They reported that about 540 school districts, or about 14 percent of all SFSP sponsors, operated the program with a seamless waiver in summer 2002. Although school district response to the waiver was generally positive, early evidence indicated that the waiver had a limited impact on the number of children receiving summer meals. In summer 2002, only about 21 percent of the sponsors using the waiver were new to the SFSP, and not all of these new sponsors entered the program because of the waiver. Moreover, average daily participation rates were substantially lower for new sponsors than for seamless waiver sponsors as a whole (531 children per day vs. 972 children per day). Tasse and Ohls (2003) estimated that on a typical day in summer 2002, about 50,000 children received meals who would not have done so without the seamless waiver. A decision about the ultimate success of the waiver will require information about impacts during summer 2003 and summer 2004.

Other actions taken by USDA to increase SFSP sponsorship include providing State agencies with the flexibility to approve deviations in the length of time between meal services and/or the duration of meal service, when existing requirements pose a barrier to participation, and to consider closed, enrolled sites that provide services exclusively to the "Upward Bound" program as categorically eligible for the SFSP. (Income-eligibility thresholds used for "Upward Bound" are identical to those used in the SFSP.) Finally, USDA developed a Web-based geographic information tool to help State agencies and other interested organizations identify areas that are underserved by the SFSP (Gordon and Briefel, 2003).[127]

Research Review

Research on the SFSP has focused on issues related to program participation and operations rather than on impacts. The most recent study of the SFSP, which was based on data collected in summer 2001, was completed in 2003 (Gordon and Briefel, 2003). The objectives of the study were to provide information about the characteristics of the SFSP and its operations at the State, sponsor, and site levels, to assess factors that affect participation of both sponsors and children, and to assess

[126] The 13 States are Alaska, Arkansas, Idaho, Indiana, Iowa, Kansas, Kentucky, Nebraska, New Hampshire, North Dakota, Oklahoma, Texas, and Wyoming.

[127] Available at www.ers.usda.gov/data/SFSP/.

nutritional quality and plate waste of SFSP meals. The study did not look at the contribution of SFSP meals to children's overall nutrient intake, or make any comparisons to eligible, nonparticipating children.

Data were collected from all SFSP State administrators as well as from nationally representative samples of sponsors and sites; 162 feeding sites were visited in person. In addition to in-person interviews, site visits included structured observations of site characteristics and operations, the types and amounts of food served on 5 or 10 randomly selected plates, and the types and amounts of food wasted on 10 randomly selected plates. Lunch was always observed. If breakfast or supper were offered, one of these meals was observed as well (snacks were not observed).

The study found that school districts made up 48 percent of all SFSP sponsors in summer 2001 and served 51 percent of all SFSP meals. School districts were found to be well-suited to serve as SFSP sponsors because they have experience preparing and serving meals to children and have available buildings and staff.

Of all SFSP sponsors, 14 percent were government agencies, generally municipal recreation or social service departments. Although fewer in number, overall, government agencies were the largest sponsors, operating 36 percent of all feeding sites and serving 31 percent of all SFSP meals in summer 2001. Government agencies frequently used vendors to provide meals because they lacked the facilities and/or expertise to prepare meals themselves.

More than 8 out of 10 (83 percent) SFSP feeding sites were open sites, 14 percent were enrolled sites, and 3 percent were residential camps. All sites served lunch, 49 percent also served breakfast, 19 percent served a snack, and only 5 percent served supper. Almost all sites (93 percent) offered some type of activities for children, including educational activities, supervised free play, organized games or sports, arts and crafts, field trips, and swimming.

More than half (58 percent) of all SFSP participants in summer 2001 were elementary school children, 20 percent were middle school age, 17 percent were preschool age, and 5 percent were high school age. Children were racially and ethnically diverse: 39 percent were African American, 29 percent were non-Hispanic White, 27 percent were Hispanic, and 5 percent were Asian, American Indian, or members of another racial/ethnic group.

SFSP sites served meals to children in a variety of settings. Most (76 percent) served meals indoors, and most (70 percent) used a serving line or food pickup line. Nutrient analysis of randomly observed plates indicated that SFSP breakfasts, as served, provided more than one-quarter of the 1989 Recommended Dietary Allowances (RDA) for protein, vitamin A, iron, calcium, and vitamin C and about 21 percent of the Recommended Energy Allowance (REA). SFSP lunches provided, on average, more than one-third of the RDA for these key nutrients, as well as approximately one-third of the REA.

SFSP breakfasts exceeded the *Dietary Guidelines* recommendation for saturated fat content and SFSP lunches exceeded *Dietary Guidelines* recommendations for both total fat and saturated fat. Study authors reported that, overall, nutrient profiles of SFSP breakfasts and lunches were similar to those reported for breakfasts served in the SBP and lunches served in the NSLP (as reported in the second School Nutrition Dietary Assessment Study, Fox et al., 2001).

Observations of plate waste indicated that children wasted about one-third of the calories and nutrients served at both breakfast and lunch.[128] Waste varied across sites and for different foods. Vitamin A at lunch was found to have the highest level of waste (53 percent) because of a high rate of waste for vegetables. Findings from the plate waste analysis were similar to those reported in the previous national study of the SFSP (Ohls et al., 1988), as well as in a study of plate waste in the NSLP (Reger et al., 1996).

Other Studies of the SFSP

Other SFSP studies have been undertaken mainly by advocacy organizations. FRAC publishes annual status reports that consolidate data on SFSP program participation by State. The reports also highlight best practices, summarize new regulations, and provide other information of use to current and prospective sponsor organizations. The 11th report in the series was published in June 2003 and summarizes data for summer 2002 (FRAC, 2003).

In 1995, the FRAC report also included results of a survey of 5,282 heads of households designed "to provide reliable information on the extent of childhood hunger in

[129]An earlier version of the same survey was completed by Wehler et al. (1991).

[130]A two-stage probability sample design was used to select census block groups. All households were enumerated and screened to determine income and presence of children under the age of 12.

the U.S." (FRAC, 1995).[129] The survey, the Community Childhood Hunger Identification Project (CCHIP), documented food insufficiency among low-income families with children, examined families' attempts to cope with food insufficiency and hunger, and described consequences of hunger for children. The survey also examined the role FANPs play in helping low-income households deal with food insufficiency and hunger. Low-income households with at least one child under the age of 12 were randomly sampled in 11 different geographic locations nationwide.[130] Interviews were conducted face-to-face with the person responsible for care and feeding of the children.

A major finding of the study was that 71 percent of low-income families with at least one child under the age of 12 had never heard of the SFSP. The authors argued that low participation in the SFSP was probably due to lack of awareness among target families. In addition, participation in the SFSP and in other child-oriented FANPs was found to vary by food security status. About 15 percent of "hungry" families participated in the SFSP compared with10 percent of "at risk" families and 9 percent of "not hungry" families.

Summary

The impact of the SFSP on participants' nutrition and health status has not been studied. Ongoing efforts to expand SFSP availability are continuing and, at least in the short term, research related to the SFSP is likely to focus on the effectiveness of these initiatives.

The recent descriptive study of the SFSP provides a solid understanding of the operations and characteristics of the SFSP at the State, sponsor, and site levels (Gordon and Briefel, 2003). The next step in evaluating the SFSP is to examine how eligible children who do not participate in the SFSP fare during the summer. USDA's Food and Nutrition Service is currently undertaking a qualitative study to examine this issue.

Ultimately, an impact study must include detailed assessment of both SFSP participants and nonparticipants. Such a study will face several implementation challenges, including the short timeframe during which the SFSP operates (6-8 weeks), as well as analytical challenges related to selection bias. However, questions about the nutrition and health impacts of the SFSP can be addressed only with a study that looks at both participants and income-eligible nonparticipants.

[128]Estimates of plate waste included only foods that were served to or selected by children but not eaten (that is, some portion of the food remained on the plate after children were through with their meal). Estimates did not include food that might have been left in or taken from "share boxes," designated places where children could leave food they did not want to eat or take food left by other children. Share boxes were available at 44 percent of sites.

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Chapter 9

The Emergency Food Assistance Program

The Emergency Food Assistance Program (TEFAP) provides commodity foods to emergency kitchens (often referred to as soup kitchens), homeless shelters, and similar organizations that serve meals to the homeless and other needy individuals. Through food banks and food pantries, the program also provides basic commodities to low-income households for preparation and consumption at home. The U.S. Department of Agriculture (USDA) purchases commodity foods and processes, packages, and distributes them to designated State agencies, which, in turn, distribute them to approved local charitable organizations.

To date, there has been no direct evaluation of TEFAP's effects on nutrition- and health-related outcomes. A small number of studies have examined the characteristics of people who are likely to receive TEFAP foods.

Program Overview

TEFAP evolved from the Federal Surplus Relief Corporation, which was established under the Agricultural Adjustment Act of 1933. It was reauthorized as the Federal Surplus Commodities Corporation (FSCC) under Section 32 of The Potato Control Act of 1935. From its inception, the program pursued parallel goals of reducing Federal food inventories and storage costs (associated with farm price supports) and assisting needy households.

The current program was first authorized as the Temporary Emergency Food Assistance Program in 1981. In 1988, when Federal stocks of some surplus foods were depleted, the Hunger Prevention Act of 1988 authorized the purchase of commodities specifically for TEFAP. These commodities are in addition to any surplus commodities donated to TEFAP by USDA. The name associated with the acronym TEFAP was changed to The Emergency Food Assistance Program under the 1990 Farm Act.

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), the initial welfare reform law, made several important changes in TEFAP. First, TEFAP was combined with the previously separate Commodity Distribution Programs for Charitable Institutions, Soup Kitchens, and Food Banks. Second, PRWORA defined a formula for allocating available commodities to States, based on poverty rates, unemployment rates, and related factors. Finally, PRWORA established a requirement that $100 million from annual Food Stamp Act appropriations be used to purchase commodities for TEFAP.

TEFAP foods are distributed free of charge. However, individuals who receive TEFAP foods for home use must meet eligibility criteria defined by each State. The types of commodities available through TEFAP vary from year to year, depending on agricultural conditions as well as State preferences. In FY 2001, more than 40 products were available, including canned and dried fruits, canned vegetables, fruit juice, canned meat, poultry, and fish, dried egg mix, peanut butter, nonfat dry milk, rice, pasta, and cereal (USDA, Food and Nutrition Service (FNS), 2003a). In FY 2002, 611 million pounds of food were distributed through TEFAP, at a Federal cost of $435 million (USDA/FNS, 2003b).

A recently completed study of providers in the Emergency Food Assistance System (EFAS) found that TEFAP commodities account for about 14 percent of all food distributed through the EFAS (Ohls and Saleem-Ismail, 2002). Nationally, 55 percent of emergency kitchens, 52 percent of food pantries, and 84 percent of food banks distribute TEFAP foods.

Research Review

The literature search identified no direct evaluations of TEFAP's effects on nutrition- or health-related outcomes. The potentially relevant literature includes a small number of studies that describe recipients of TEFAP food either explicitly or implicitly. These studies are summarized briefly in the sections that follow.

Characteristics of TEFAP Recipients

TEFAP recipients are generally poor and food insecure and tend to be demographically similar to the low-income population overall (Quality Planning Corporation and Abel, Daft, and Earley, 1987). A dimension often used to differentiate TEFAP recipients is whether they receive food for preparation and consumption at home or consume food available on the premises of prepared meal programs (Briefel et al., 2003: Clancy et al., 1991; Bowering et al., 1991). This distinction largely reflects the portion of recipients with a home vs. those who are homeless or whose residences lack facilities for storing or preparing food. Recipients who obtain TEFAP food for preparation at home normally get the food from food pantries or similar distribution facilities, while those who obtain food through prepared meal programs normally get it at emergency kitchens, shelters, or other residential programs, such as drug treatment or detoxification centers or battered women's shelters.

A national study of TEFAP conducted in 1986 for USDA by the Quality Planning Corporation (QPC) and Abel, Daft, and Earley (ADE) included a survey of TEFAP recipients. At the time this study was completed, TEFAP did not include commodity distribution to charitable institutions.[131] Consequently, recipients characterized in the study were those who met State-defined income-eligibility criteria and received commodities for use at home. Such recipients are only a subset of the individuals served by TEFAP today.

A 1988 study by Burt and Cohen provides information on the characteristics of individuals who receive meals through emergency food kitchens, homeless shelters, and other charitable organizations. Clancy et al. (1991) and Bowering et al. (1991) reported results from 1990 surveys of New York State food pantry and emergency kitchen users, respectively. At the time these studies were conducted, these institutions received their USDA commodities from the then-separate Commodity Distribution Program for Charitable Institutions, Soup Kitchens, and Food Banks.

The most up-to-date and complete information on the characteristics of likely TEFAP recipients comes from the recently completed study of the EFAS (Ohls and Saleem-Ismail, 2002). That study included a survey of EFAS clients served by both arms of the system—food pantries and emergency food kitchens (Briefel et al., 2003). A randomly selected and nationally representative sample of 2,397 food pantry clients and 2,425 emergency kitchen clients were interviewed in person between August and November, 2001. Overall response rates were 70 percent and 77 percent, respectively. Major findings from this survey are discussed below. Findings from the two older surveys described above, are discussed when they provide information or a perspective that is not available in the EFAS survey.

According to the EFAS survey, food pantries served 4.3 million different households, comprising 12.5 million people, during a typical month in 2001. Emergency kitchens served about 1.1 million people per month in 2001 (Briefel et al., 2003). Data on the characteristics of these EFAS recipients confirm findings from earlier studies and show that pantries and emergency kitchens serve different segments of the low-income population.

Households that used food pantries were more likely to include children than households that used emergency kitchens. Almost half (45 percent) of food pantry households included children, compared with about 20 percent of emergency kitchen households (Briefel et al., 2003). Almost 30 percent of pantry households were single adults living alone; more than two-thirds of these single adults were female. In contrast, more than 50 percent of emergency kitchen households were adults living alone, and more than 70 percent of these single adults were male. Among pantry clients, the majority racial/ethnic group was non-Hispanic White (49 percent), followed by non-Hispanic Black (31 percent), Hispanic (16 percent) and other (5 percent). Among food pantry clients, the greatest concentration of clients were non-Hispanic Black (45 percent), followed by non-Hispanic White (35 percent).

About a quarter of pantry clients and 40 percent of emergency kitchen clients were unemployed and looking for work. Employment status tracked with household composition, with households with children being more likely to have employed members than households without children. Education was a limiting factor for both groups of EFAS clients, particularly those using food pantries. Close to 46 percent of pantry clients and 39 percent of emergency kitchen clients had less than a high school education.

Both groups of EFAS clients were poor. At the time they were interviewed, 93 percent of pantry clients and 83 percent of emergency kitchen clients had incomes at or below 130 percent of the Federal poverty guideline. One-third and two-fifths, respectively, had incomes that were at or below 50 percent of the poverty line. In addition to poverty, both sets of EFAS clients experienced problems with homelessness or other challenges to daily living, such as lack of adequate food storage/cooking facilities, transportation, or working telephones. Clients of emergency kitchens were most likely to be homeless (36 percent vs. 8 percent of pantry clients).

About half of pantry clients and two-fifths of emergency kitchen clients in the EFAS survey reported

[131]The Commodity Distribution Program for Charitable Institutions, Soup Kitchens, and Food Banks was a separate program until 1996 when PRWORA merged it into TEFAP.

being in "fair" or "poor" health. (This compares with one-third of the general low-income population) (Briefel et al., 2003). The earlier survey of homeless people using emergency food assistance completed by Burt and Cohen (1988) found that there was a relatively high prevalence of mental health problems, with 20 percent reporting a history of mental hospitalization, about 20 percent reporting at least one suicide attempt, and 7 percent having been diagnosed as suffering from a major psychiatric problem. On a scale measuring current depression and demoralization, 49 percent had high enough psychological distress scores to indicate a need for immediate treatment.

More than half (55 percent) of the pantry-client households surveyed in 2001 visited a pantry once per month or less often (many providers restrict the frequency of visits) (Briefel et al., 2003). About a quarter reported visiting two or three times per month, and about 20 percent visited once per week or more often. Among users of emergency kitchens, about 13 percent received their meals at the kitchen every day. Another 43 percent received meals 2 to 5 days per week. Clients who visited kitchens almost daily tended to rely on the kitchen for multiple months; sometimes for years.

About three-quarters of pantry clients and more than two-thirds (69 percent) of kitchen clients surveyed in 2001 said they preferred to get food from a pantry or kitchen than "ask the Government for help" (although many of these households reported relying on other types of Federal assistance) (Briefel et al., 2003). More than two-thirds (69 percent) of food pantry clients and almost half (45 percent) of emergency kitchen clients combined use of emergency food assistance with participation in another food and nutrition assistance program (FANPs). However, although most EFAS clients (90 percent of food pantry clients and 82 percent of emergency kitchen clients) were income-eligible for the FSP, a substantial proportion of eligible households did not participate (45 percent of pantry-client households and 56 percent of emergency kitchen clients). Uncertainty about eligibility was the reason most commonly given for not participating.

Among food pantry clients, households that included seniors (adults 60 and older) but no children were more likely than other types of households to use only food pantries to obtain food assistance. The 1986 study of TEFAP recipients (analogous to today's EFAS pantry clients) compared the demographic profile of TEFAP recipients with a profile of FSP recipients during the same period and found that the primary difference between participants in the two programs was age. The percentage of elderly households in TEFAP was more than twice that of the FSP (38 percent vs. approximately 15 percent). As a potential explanation for this disparity, Levedahl et al. (1994) cite findings from two studies of barriers to FSP participation among the elderly (Ponza, 1990; Ponza and Wray, 1990). Many elderly apparently prefer TEFAP to the FSP because TEFAP's application and distribution procedures are less complicated (Ponza 1990, as cited in Levedahl et al., 1994). In addition, some elderly are resistant to FSP participation because FSP coupons clearly identify users as welfare recipients (Ponza and Wray, 1990, as cited in Levedahl et al., 1994).

Nutrition-Related Characteristics of Likely TEFAP Recipients

Briefel and her colleagues (2003) assessed EFAS clients' food security status using the 6-item short version of the core food security module developed by USDA (Bickel et al., 2000). They found that about three-quarters of EFAS clients were food insecure, and almost half were classified as food insecure with hunger. This compares with a national estimate of food insecurity of 11 percent overall and 32 percent for low-income households (Nord et al., 2002). The prevalence of food insecurity varied by household composition. The households with the greatest rates of food security were those with seniors and no children.

Analyses showed that material hardships and food insecurity were more severe among EFAS client households that used two or more forms of emergency food assistance than for EFAS client households that combined emergency food assistance with participation in other FANPs. Nonetheless, three-quarters of households that combined EFAS services with FANP participation experienced food insecurity.

Burt and Cohen (1988) examined the eating patterns of homeless people who used emergency kitchens and shelters and compared them with homeless people who did not use these services. To obtain information on homeless people who did not use emergency kitchen or shelter services, the researchers interviewed homeless individuals at congregating sites—for example, bus stations, culverts, and other homeless "encampments." This sample of nonservice-using homeless individuals—those who had not used any kind of shelter or emergency kitchen for the past week—was small and not necessarily representative. Although not generalizable, the information obtained provides useful insights about differences between the service-using and nonservice-using homeless (akin to TEFAP recipients and eligible nonrecipients).

Generally, all homeless people sampled tended to eat less frequent and less adequate meals than the overall population. Homeless people who did not use either emergency kitchens or shelters fared worse than those who did use these services. Homeless nonusers ate an average of 1.36 meals per day compared with 1.92 meals per day for homeless service users and 3 or more meals per day for the average low-income person. Homeless nonusers were also more likely to have gone 1 or more days without eating during the previous week than homeless service users, averaging 1.35 days per week without eating compared with 0.66 days per week for homeless service users. Homeless nonusers were more likely than homeless service users to describe their diets as fair or poor and less likely to have eaten foods from five core food groups.

The researchers did not assess the nutrient content of the meals actually consumed by homeless persons or their total nutrient intake over the course of the day.[132] Available information on the number of meals eaten per day and the number of days in which nothing was eaten strongly suggests that substantial differences may exist between the food served in shelters and emergency kitchens and the food actually consumed routinely by homeless persons.

[132]The QED/ADE study (1987) included an assessment of the potential nutrient contribution of an average TEFAP package to the diets of regular program participants. The packages were found to provide significant amounts of protein and key vitamins and minerals but were high in saturated fat, cholesterol, and sodium. At the time that this analysis was done, only seven commodity foods were offered and cheese was a major component of the package. TEFAP food packages offered today have considerably more variety and have been designed to be lower in fat, cholesterol, and sodium.

Summary

No research completed to date has examined the impact of TEFAP on nutrition- and health-related outcomes of program participants. The recently completed surveys of providers in the EFAS (Ohls and Saleem-Ismail, 2002) and EFAS clients in food pantries and emergency kitchens (Briefel et al., 2003) provide researchers and policymakers with a detailed and up-to-date picture of the organizational system and programs that distribute TEFAP foods and the characteristics and experiences of likely recipients of TEFAP foods. These data provide a solid foundation for future research that may examine nutrition and health characteristics of TEFAP recipients and, potentially, the influence of the program on these characteristics.

However, any evaluation of the effects of TEFAP at the participant level will face some formidable challenges. First, TEFAP foods compose only part of the package delivered by the participating programs and, because the package delivered often depends on voluntary contributions, its content generally fluctuates over time. Moreover, individuals tend to receive these foods on an episodic basis, often for only a single instance. Second, many recipients of TEFAP foods obtain and consume food from a wider-than-normal variety of sources, ranging from supermarkets to food pantries, prepared and perishable food recovery programs, emergency kitchens, shelters, family, friends, panhandling, dumpsters, and garbage cans. Usual approaches to measuring food consumption may not be effective for some TEFAP recipients because of their unusual circumstances. Finally, in addition to the problems of defining the TEFAP intervention and measuring potential outcomes, it will be difficult to construct an appropriate representation of the counterfactual—that is, defining and accessing an appropriate comparison/control group of eligible individuals who do not receive TEFAP foods.

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Chapter 10

Nutrition Services Incentive Program

The Nutrition Services Incentive Program (NSIP), formerly known as the Nutrition Program for the Elderly (NPE), is a U.S. Department of Agriculture (USDA) program that provides cash and/or commodities to agencies or organizations that sponsor Elderly Nutrition Program (ENP) sites. The ENP, which is administered by the U.S. Department of Health and Human Services (HHS), Administration on Aging (AoA), is the primary vehicle for the organization and delivery of nutrition and support services to the Nation's elderly. The program provides meals in both group and home settings. Although any person over the age of 60 is eligible to participate, local programs try to target elders with the greatest nutritional and/or social needs. In recent years, the home-delivered meals component of the program has grown dramatically, reflecting an increase in the number of frail, homebound elderly.

Program Overview

The ENP was designed specifically to address problems of inadequate dietary intake and social isolation among the elderly. It began as a 3-year pilot program in 1968 and was permanently authorized in 1972, as a new title of the Older Americans Act (OAA). In enacting the program, Congress cited "an acute need for national policy which provides older Americans, particularly those with low incomes, with low-cost, nutritionally sound meals served in strategically located centers ... Besides promoting better health ... such a program would reduce the isolation of old age, offering Americans an opportunity to live their remaining years in dignity" (P.L. 92-258, Section 701).

The ENP provides daily meals to people age 60 and over in group settings (congregate feeding programs) and, when appropriate, at home (Meals-on-Wheels).[133] Spouses of age-eligible individuals are also eligible to participate, regardless of age. In addition, disabled people who live in elderly housing facilities, people who accompany elderly participants to congregate feeding sites, and volunteers who assist in the meal service may also receive meals through the ENP. Each recipient may contribute as much as he or she wishes toward the cost of the meal, but meals are free to those who cannot make any contribution.

The program is available to all age-eligible individuals, regardless of household income. However, the goal is to target those with the greatest nutritional, social, or economic need, particularly low-income minorities and elders living in rural areas (Wellman et al., 2002). Because the program is not means-tested, the ENP is the primary service system for elders whose incomes may be slightly greater than the income-eligibility requirements used for other programs, such as the Food Stamp Program or the Commodity Supplemental Food Program (Wellman et al., 2002).

The ENP is administered by a network of agencies devoted to the aging population, including State and Indian Tribal Organization (ITOs) units on aging, within-State area agencies on aging, and local delivery sites.[134] The program has grown substantially since its inception. In 1975, the program provided 48.5 million meals (HHS/AoA, 2002). In 1980, ENP providers served 168.4 million meals, an increase of almost 250 percent. The program continued to grow during the 1980s and 1990s, although at a slower pace. In FY 2001, the ENP served 253 million meals (USDA, Food and Nutrition Service (FNS), 2003).[135]

Much of the increase in the ENP during the 1980s and 1990s was in home-delivered meals. In 1978, a program regulation that limited home-delivered meals to 10 percent of total meals was rescinded and separate authorizations were established for home-delivered and congregate meals (HHS/AoA, 2002). Subsequently, the relative size of the home-delivered meals component of the program began to increase steadily. In 1980, home-delivered meals represented 22 percent of all ENP meals. In 1991, home-delivered meals accounted for 43 percent of all ENP meals. By the end of the decade, home-delivered meals had increased to more than half (54 percent) of all ENP meals (HHS/AoA, 2002). This trend reflects an

[133]To be eligible for home-delivered meals, participants must be homebound or otherwise isolated (Wellman et al., 2002).

[134]Operation of the ENP by ITOs was authorized separately in 1978 under Title VI of the OAA. States and ITOs continue to be authorized under separate titles of the OAA, but ENP sites in both settings operate under the same program regulations.

[135]USDA stopped maintaining data on the number of meals served in the ENP after FY 2001 because of changes in the program's administrative structure, as described in subsequent sections.

increased need for home-delivered meals as well as the availability of increased funding. Even with this dramatic growth, many ENP sites have waiting lists for home-delivered meals (Ponza et al., 1996).

USDA's involvement in the ENP began in 1975 when Congress authorized USDA to donate commodities to the ENP. The USDA program, known as the Nutrition Program for the Elderly (NPE), provided commodities to States and ITOs, which, in turn, distributed them to local ENP sites. In 1977, P.L. 95-65 allowed States and ITOs to elect to receive their NPE entitlement in the form of cash rather than commodities. Over time, the predominant type of support provided by the NPE shifted from commodities to cash. In FY 1999, only 2 percent of the $140 million NPE appropriation was distributed to ENP meal providers as commodities (HHS/AoA, 2002).

When the ENP was reauthorized in FY 2000, the name for the USDA program was changed to the Nutrition Services Incentive Program (NSIP). In addition, the model for administration of the program was changed from a simple reimbursement model to an allocation model. Rather than reimbursing States and ITOs on a per meal basis based on the number of meals served the previous fiscal year, NSIP funds are now distributed to States and ITOs based on the number of meals served *relative to the total number of meals served by all States and ITOs*. The reason for this change was a desire to reward States and ITOs for efficient use of cash and/or commodities in providing meals to older adults (USDA/FNS, 2002).

In FY 2003, responsibility for the administration of the NSIP was transferred from USDA to HHS, although USDA continues to provide financial support and donated commodities. In FY 2002, USDA's contribution to the ENP was $152 million (USDA/FNS, 2003).

Program Services

ENP providers are required to offer participants at least one "hot or other appropriate" meal per day 5 or more days per week. Providers may elect to provide additional meals. Congregate meal sites must be located in close proximity to areas with large concentrations of elderly residents and, to the extent possible, be within walking distance of participants' homes. When feasible, programs provide transportation for participants who are unable to travel to the meal site on their own. Home-delivered meals can be either hot or cold.

Historically, lunch has been the focal point of the ENP, and most congregate and home-delivered meal programs served lunch only 5 days per week. As the program has matured, however, local providers have incorporated service innovations that have allowed them to better meet participants' needs. In a 1988 survey of 450 ENP project sites, Balsam and Rogers (1991) found that many projects had expanded well beyond serving only lunch, particularly with regard to home-delivered meals. Half of the sites providing home-delivered meals offered meals on weekends and one in five offered supper. Comparable statistics for congregate meal sites were 17 percent and 10 percent, respectively. Other innovations reported by Balsam and Rogers included contracting with restaurants and diners to provide meals, exclusively targeting meals served at a given site to a particular racial/ethnic group, and regularly scheduled visits to congregate feeding sites by nursing home residents.

ENP sites have also developed noteworthy approaches to maximizing available Federal funding in order to serve more elders and provide them with needed services. The most recent national evaluation of the ENP "estimated that government funding investments in the ENP were tripled by the program's innovative use of volunteers, the collection of contributions by elders to the costs of meals, and the supplementation of Federal resources with State grants and private donations" (Balsam et al., 2000).

ENP funds can also be used for nutrition education and other appropriate services (O'Shaughnessy, 1990). Over time, the ENP has become an integral component of a comprehensive and coordinated system of home- and community-based services (HCBC) (Wellman et al., 2002). Services provided by ENP sites include transportation, shopping assistance, health screenings, wellness programs, information and referral services, and recreational and social activities.

Nutrition Standards for ENP Meals

In the early 1990s, concerns were raised about the nutritional integrity of the ENP. During the OAA reauthorization hearings in 1992, several professional groups involved in the ENP, including the American Dietetic Association and the National Association of Nutrition and Aging Service Programs, encouraged Congress to incorporate minimum standards for nutrition services provided under the OAA. The majority of the recommendations made in the hearings were ultimately incorporated into law as part of the 1992 Amendments to the OAA (P.L. 102-375).

Specifically, the 1992 Amendments stipulated that ENP meals must comply with the *Dietary Guidelines for Americans* (DGAs) and provide the following:

- A minimum of one-third of the Recommended Dietary Allowances (RDAs) if one meal per day is offered.
- A minimum of two-thirds of the RDAs if two meals per day are offered.
- 100 percent of the RDAs if three meals per day are offered.

These standards represent a substantial change from previous practice. Before 1992, some States encouraged ENP sites to consider the DGAs, but neither Federal nor State guidelines required that ENP meals be consistent with the DGAs. With regard to the RDA standards, the 1992 regulations shifted the focus from the individual meal to the total meal package. Previous regulations required that *each meal* supply one-third of the RDA, regardless of the type of meal or the total number of meals offered. The switch to standards that considered the total meal package provided more flexibility in meal planning because it allowed program planners to distribute nutrients across multiple meals as long as the total combination of meals offered provided participants with the opportunity to consume specified levels of nutrients.

Research Overview

No one has studied the effectiveness of the NSIP (or the former NPE), per se. To understand the impact of the NSIP, one has to look to research on the larger program, the ENP. The literature search identified two nationally representative studies of the ENP as well as 11 smaller local studies.[136] Characteristics of these studies are summarized in table 41. Studies are divided into three groups. Group I includes national evaluations, Group IIA includes local studies that focused on congregate ENP programs, and Group IIB includes local studies that examined the home-delivered meals component of the ENP.

The first national evaluation of the ENP was conducted for the AoA by Kirschner Associates, Inc., and Opinion Research Corporation (ORC). The study was done in two waves, with data collected in 1976-77 (reported in 1979) and in 1982 (reported in 1983). In this partially longitudinal design, 42 percent of wave I participants were re-interviewed in wave II.

The most recent national evaluation, the National Evaluation of the Elderly Nutrition Program, 1993-95, is the most comprehensive evaluation of the ENP completed to date (Ponza et al., 1996). The evaluation focused largely on dietary intake, although the social support aspect of the program was also assessed. ENP participants were compared with the elderly U.S. population in general, using data from the third National Health and Nutrition Examination Survey (NHANES-III), as well as with eligible nonparticipants, identified through Medicare beneficiary data.

Of the 11 local studies that attempted to measure the impact of ENP participation on nutrition and health outcomes, 8 (Group IIA in table 41) looked at congregate meals and 3 (Group IIB) looked at home-delivered meals. Sample sizes for all of these studies were substantially smaller than those of the nationally representative studies. Four studies had samples of less than 100 (Gilbride et al., 1998; Steele and Bryan, 1986; LeClerc and Thornbury, 1983; Singleton et al., 1980). Samples for the remaining seven studies ranged from 135 to 547.

Identifying Nonparticipant Comparison Groups

All of the impact studies completed to date have used quasi-experimental designs. Most studies compared program participants with a similar group of eligible nonparticipants at a single point in time. Nearly all of the studies defined program participants as those who ate an ENP meal during the preceding 24-hour period.

Researchers have used several different methods to identify nonparticipant comparison groups and have had varying degrees of success in establishing comparability between groups. Many of the local studies identified nonparticipants from program waiting lists. While this approach may seem like a reasonable way to minimize potential selection bias, it may lead to problems with the comparability of treatment and control groups. Not all ENP sites, particularly those that serve congregate meals, have waiting lists. Sites that do have waiting lists and the individuals included on those lists may differ from sites that do not have waiting lists and the individuals who participate in those sites. Moreover, individuals on waiting lists may be different from those receiving

[136]Studies that assessed the nutrient content of ENP meals and/or the contribution of ENP meals to the nutrient intake of participants—without comparison to nonparticipants—are not included in table 41 or the summary tables presented later in this section, but have contributed to this review. These sources include Stevens et al. (1992), Vaughan and Manore (1988), Grandjean et al. (1981), Caliendo and Smith (1981), Caliendo (1980), and Caliendo and Batcher (1980).

Table 41—Studies that examined the impact of the Elderly Nutrition Program on nutrition and health outcomes

Study	Outcome(s)	Data sources[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Group I: National evaluations							
Ponza et al. (1996) (National Evaluation of the ENP—1993-95)	Dietary intake and social contacts	Random sample of ENP participants (both congregate and home-delivered) and random sample of nonparticipants selected from HCFA Medicare beneficiary file (1993-95)	24-hour dietary recall and in-person interview	ENP-eligible elderly (n=2,699)	Participant vs. nonparticipant	Received ENP meal on dietary recall day (did not necessarily consume it)	Multivariate regression; attempted to control for selection bias
Kirschner and Associates and Opinion Research Corporation - Wave II (1983)	Dietary intake and socialization	Participants in 70 randomly selected ENP sites (both congregate and home-delivered), random sample of participants' neighbors, and former participants (1976-77)	24-hour dietary recall and isolation index	ENP-eligible elderly (n=3,411)	Participant vs. nonparticipant and comparisons to Wave I participants still enrolled in congregate sites	Ate ENP meal on dietary recall day	Chi-square tests
Kirschner and Associates and Opinion Research Corporation - Wave I (1979)	Dietary intake and socialization	Participants in 91 randomly selected ENP sites (congregate only) and random sample of participants' neighbors (1982)	24-hour dietary recall and isolation index	ENP-eligible elderly (n=4,563)	Participant vs. nonparticipant	Ate ENP meal on dietary recall day	No statistical tests conducted
Group IIA: State and local studies of congregate meals							
Gilbride et al. (1998)	Dietary intake and nutritional risk	Residents in HUD elderly housing facilities in metropolitan New York City; nonparticipants from facilities that did not have ENP (dates not reported	2 24-hour dietary recalls, food frequency, 5-day food records, and level-one screen from Nutrition Screening Initiative checklist	ENP-eligible elderly (n=40)	Participant vs. nonparticipant	Currently receiving ENP meals	No statistical tests conducted

See notes at end of table.

Continued—

Table 41—Studies that examined the impact of the Elderly Nutrition Program on nutrition and health outcomes—Continued

Study	Outcome(s)	Data sources[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Neyman et al. (1996)	Dietary intake, weight status, nutritional biochemsitries	Participants and nonparticipants at 9 ENP sites in 2 northern California counties (dates not reported)	3-day food record, venous blood sample, height and weight	ENP-eligible elderly (n=135)	Participant vs. nonparticipant	Ate ENP meal on at least 1 food record day	Multifactorial analysis of variance
Czajka-Narins et al. (1987)	Dietary intake, weight status, and nutritional biochemistries	Participants in 6 ENP sites in Missouri; nonparticipants from senior center that did not serve meals (dates not reported)	1-day food record, 24-hour recall, food frequency, venous blood sample, height, weight, and tricep skinfolds	ENP-eligible elderly, over 75 years old (n=185)	Participant vs. nonparticipant	Regular participation: Ate at ENP meal site 2-5 times per week Irregular participation: Ate at ENP site less than twice per week, but at least once per week during last 4 months	Chi-square tests and analysis of variance
LeClerc and Thornbury (1983)	Dietary intake	Participants in 1 ENP site in central Maine; nonparticipants from federally-subsidized housing units in same area (dates not reported)	3-day food records	ENP-eligible, low-income elderly (n=53)	Participant vs. nonparticipant	Ate ENP meal 3-5 times per week	Bivariate t-tests and analysis of variance
Nordstrom et al. (1982)	Iron intake and iron status	Participants in 6 ENP sites in Missouri; nonparticipants from senior center that did not serve meals (1975)	1-day food record and venous blood sample	ENP-eligible elderly (n=320)	Participant vs. nonparticipant	Ate ENP meal on food record day	Analysis of variance

See notes at end of table.

Continued—

Table 41—Studies that examined the impact of the Elderly Nutrition Program on nutrition and health outcomes—Continued

Study	Outcome(s)	Data sources[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Kohrs et al. (1980)	Dietary intake, weight status, and nutritional biochemistries	Participants in 6 ENP sites in Missouri; nonparticipants from senior center that did not serve meals (1975)	1-day food record, 24-hour recall, food frequency, venous blood sample, height, weight, and tricep skinfolds	ENP-eligible elderly (n=547)	Participant vs. nonparticipant	Regular participation: Ate at ENP meal site 2-5 times per week Irregular participation: Ate at ENP site less than twice per week, but at least once per week during last 4 months	Chi-square tests and analysis of variance
Singleton et al. (1980)	Dietary intake	Participants in 7 ENP sites in southern Louisiana; nonparticipants from 2 senior centers that did not serve meals (dates not reported)	24-hour dietary recall	ENP-eligible, low-income elderly females (n=97)	Participant vs. nonparticipant	Ate ENP meal on dietary recall day	Analysis of variance
Kohrs et al. (1978)	Dietary intake	Participants in 6 ENP sites in Missouri; nonparticipants from senior center that did not serve meals (1973)	1-day food record	ENP-eligible elderly (n=466)	Participant vs. nonparticipant	Ate ENP meal on food record day	Analysis of variance
Group IIB: State and local studies of home-delivered meals							
Edwards et al. (1998)	Food security, diet diversity, and diabetic control	Random sample of diabetic recipients of home-delivered meals in New York State and random sample of nonparticipants from a waiting list (1986-87)	In-person interview and mail survey of respondents' physicians	ENP-eligible, homebound diabetic elderly (n=154)	Participant vs. nonparticipant	Currently receiving ENP meals at least 2 times per week	Multivariate regression

See notes at end of table.

Continued—

Table 41—Studies that examined the impact of the Elderly Nutrition Program on nutrition and health outcomes—Continued

Study	Outcome(s)	Data sources[1]	Data collection method	Population (sample size)	Design	Measure of participation	Analysis method
Ho-Sang (1989)	Dietary intake and weight status	Recipients of home-delivered meals in New York State; nonparticipants from waiting lists for other programs (dates not reported)	24-hour dietary recall, height, weight, and tricep skinfolds	ENP-eligible, homebound elderly (n=448)	Participant vs. nonparticipant	Currently receiving ENP meals	Bivariate t-tests and multivariate regression
Steele and Bryan (1986)	Dietary intake	Recipients of home-delivered meals from 1 site in North Carolina; nonparticipants from a waiting list (1982-83)	24-hour dietary recall and diet history	ENP-eligible, homebound elderly (n=54)	Participant vs. nonparticipant	Currently receiving 1 ENP meal per day, 5 days per week	Bivariate t-tests

[1] All studies were primary data collection efforts.

meals because of the criteria sites use to determine who gets on the list and, once on the list, who gets served first (Ponza et al., 1996).

Neither of the national evaluations of the ENP used waiting lists to define comparison groups. The Kirschner/ORC study (1979, 1983) drew nonparticipants from neighbors of participants and former participants. The 1993-95 National Evaluation of the ENP (Ponza et al., 1996) used Medicare beneficiary files to identify eligible nonparticipants and then contacted them by telephone to screen for age, income, disability status, and program participation.

Outcomes Examined

The nutrition- and health-related outcome most often examined in this literature is dietary intake. Only a few studies, all of which were local studies, examined nutritional biochemistries or weight status (Neyman et al., 1996; Ho-Sang, 1989; Czajka-Narins et al., 1987; Nordstrom et al., 1982; Kohrs et al., 1980). The two most recent studies (Ponza et al., 1996; Gilbride et al., 1998) included a general measure of nutritional risk. However, only Gilbride et al. (1998) compared participants and nonparticipants on this measure and no statistical tests were conducted. One study examined the impact of the ENP on food security (Edwards et al., 1993). And finally, the two national evaluations (Kirschner/ORC, 1979, 1983; Ponza et al., 1996) included assessment of social interaction.

Limitations

Many of the identified studies included only simple bivariate comparisons of participant and nonparticipant groups. Although most authors attempted to demonstrate comparability of participant and nonparticipant groups on “key” variables, the lack of more sophisticated analytical controls for noncomparability substantially limits the credibility of study findings.

Most of the more recent studies (for example, Edwards et al., 1998; Ponza et al., 1996; Ho-Sang, 1989) used multivariate regression techniques to control for differences in measured characteristics. However, only the 1993-95 National Evaluation of the ENP attempted to address potential selection bias through statistical modeling. Ponza and his colleagues (1996) estimated three selection-bias models but ultimately considered the results unreliable. They based their findings on regression-adjusted comparisons from a one-stage model, appropriately cautioning readers that selection bias may play a role in reported results.

Research Results

This section summarizes findings from the available research. The discussion is organized into six sections, each of which focuses on reported effects of the ENP on a different outcome or group of outcomes. The outcomes examined include intake of food energy and nutrients, nutritional biochemistries, weight status, socialization, food security, and nutritional risk.

All of the studies that compared the nutrient content of ENP with the minimum Federal requirement of one-third of the RDA (per meal) found that ENP meals served to participants satisfied this standard (Ponza et al., 1996; Stevens et al., 1992; Kohrs, 1986; Kirschner/ORC, 1983; Caliendo, 1980; and Kohrs et al., 1978). Thus, one can assume that participants generally had access to the nutritional benefit the ENP was designed to deliver.

Impacts on Intake of Food Energy and Nutrients

Most studies that examined dietary outcomes used a single 24-hour recall. Comparisons between participants and nonparticipants were based on mean intakes, most often expressed as proportions of the RDAs.

In addition to the usual problems with 24-hour recall data and comparisons to RDA benchmarks (see chapter 2), use of the RDA in assessing intakes of elderly persons presents unique problems (Dwyer and Mayer, 1997; Ponza et al., 1996; Ponza et al., 1994; Posner, 1979). The RDAs, as they existed at the time the reviewed research was conducted, provided a single recommendation for all males over the age of 51 and a corresponding recommendation for all females over the age of 51 (National Research Council (NRC), 1989). There is good evidence, however, that nutrient needs actually differ for adults over the age of 60 or 70 (Russell and Suter, 1993). In addition, physiologic changes associated with aging, degenerative changes related to chronic disease, and/or pharmacologic or other interventions can influence nutrient absorption, use, or excretion among the elderly (Ponza et al., 1994). Consequently, the available information on the impact of the ENP on participants’ intake of food energy and nutrients must be considered even more tentative than the information available for most other food assistance and nutrition programs (FANPs).[137]

[137]The Dietary Reference Intakes (DRIs) which have replaced the traditional RDAs (see chapter 2), define separate standards for adults between the ages of 51 and 70 and those over the age of 70.

Findings for all studies that examined the impact of the ENP on the dietary intake of older adults are summarized in table 42. The table is divided into four sections: food energy and macronutrients, vitamins, minerals, and other dietary components. The table clearly illustrates whether findings apply to congregate meals, home-delivered meals, or both types of meals. The text follows the same general organization as table 42, but findings related to vitamins and minerals are discussed in one section.

In the interest of providing a comprehensive picture of the body of research, both significant and nonsignificant results are reported in table 42 and in all other "findings" tables. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned, however, to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

In interpreting available data on the impact of the ENP on the dietary intake of older adults, findings from the 1993-95 National Evaluation of the ENP (Ponza et al., 1996) are given the most weight. Despite a lingering potential for selection bias, this study provides the best available information on potential nutrition- and health-related impacts of the ENP. The study, which was national in scope and is the most comprehensive study done to date, was implemented with great care and is based on relatively recent data. Most importantly, the study used appropriate analytic techniques to control for between-group differences in measured characteristics rather than relying on unadjusted bivariate comparisons. Study authors were also careful to avoid estimating impacts on outcomes that are particularly vulnerable to selection bias, such as food security (food-insecure individuals may seek out the ENP) and measures of nutritional status beyond intake of energy and nutrients from ENP meals (ENP sites target the most vulnerable elderly).

Food Energy and Macronutrients

In the 1993-95 National Evaluation of the ENP, Ponza et al. (1996) found that both congregate and home-delivered ENP participants had significantly higher energy intakes than nonparticipants. Comparable results were reported in the other national study that looked at energy intake (Kirschner/ORC, 1983), as well as in one local study (Kohrs, et al., 1978).

Findings from the remaining studies that included statistical analyses were inconsistent, and none of the differences between participants and nonparticipants were statistically significant. Most of these studies had very small samples and relied on bivariate analyses.

The 1993-95 National Evaluation of the ENP also found that ENP participants consumed a significantly greater amount of protein than nonparticipants. However, the difference between participants and nonparticipants in the percentage of calories derived from protein was not significant (participants consumed more energy *and* more protein).

Data from national surveys of food and nutrient intake indicate that older Americans, like their younger counterparts, typically exceed recommended intakes of both total fat and saturated fat, expressed as a percentage of total energy intake (Dwyer and Mayer, 1997). The ENP does not appear to influence this situation one way or the other, despite the previously described changes in program regulations that incorporated the DGAs into the program's nutrition standards. Neither of the studies that were completed after 1992 (when the DGAs were incorporated) and included statistical analyses found significant differences between ENP participants and nonparticipants in the intake of fat or saturated fat, relative to energy intake (Ponza et al., 1996; Neyman et al., 1996). These findings were true whether ENP meal(s) were consumed in congregate sites or at home. Findings from older studies (conducted before the 1992 policy change) are similar.

Vitamins and Minerals

Both of the national evaluations found that ENP participants consumed significantly greater amounts of a wide array of vitamins and minerals than nonparticipants. The earliest national evaluation (Kirschner/ORC, 1983) reported that ENP participants consumed significantly more than nonparticipants of all of the vitamins and minerals examined: vitamin A, vitamin C, niacin, riboflavin, thiamin, calcium, and iron. The more recent 1993-95 National Evaluation of the ENP (Ponza et al., 1996) reported the same pattern of findings for ENP participants who received congregate meals. ENP participants who received home-delivered meals also had higher mean intakes than nonparticipants, but some of these differences did not reach statistical significance. In addition, the 1993-95 National Evaluation found higher intakes among ENP participants for a number of vitamins and minerals that were not measured in the earlier study: vitamins B_6, B_{12}, D, and E, folate, magnesium, phosphorus, potassium, and zinc.

Table 42—Findings from studies that examined the impact of the Elderly Nutrition Program on participants' dietary intakes

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Food energy and macronutrients				
Food energy	***Both meal types*** Ponza (1996) [national] Kirschner (1983) [national] ***Congregate only*** Kohrs (1978) [6 sites]	***Congregate only*** Czajka-Narins (1987) [6 sites] LeClerc (1983) [1 site] Kohrs (1980) [6 sites] Singleton (1980) [7 sites] Kirschner (1979) [national]	***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites] ***Home only*** Steele (1986) [1 site]	
Protein	***Both meal types*** Ponza (1996) [national][1] Kirschner (1983) [national] ***Congregate only*** Czajka-Narins (1987) [6 sites] Kohrs (1978) [6 sites]	***Congregate only*** LeClerc (1983) [1 site] Kirschner (1979) [national] ***Home only*** Ho-Sang (1989) [6 sites] Steele (1986) [1 site]	***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites] Kohrs (1980) [6 sites] Singleton (1980) [7 sites]	
Carbohydrates		***Both meal types*** Ponza (1996) [national] ***Congregate only*** Neyman (1996) [9 sites] Czajka-Narins (1987) [6 sites] LeClerc (1983) [1 site] Kohrs (1980) [6 sites] Singleton (1980) [7 sites]	***Congregate only*** Gilbride (1998) [3 sites] ***Home only*** Ho-Sang (1989) [6 sites]	***Home only*** Steele (1986) [1 site]
Fat		***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites] Kohrs (1980) [6 sites] ***Home only*** Ponza (1996) [national] Ho-Sang (1989) [6 sites]	***Congregate only*** Ponza (1996) [national] Czajka-Narins (1987) [6 sites] LeClerc (1983) [1 site] Singleton (1980) [7 sites] ***Home only*** Steele (1986) [1 site]	

See notes at end of table.

Continued—

Table 42—Findings from studies that examined the impact of the Elderly Nutrition Program on participants' dietary intakes—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Saturated fat			***Both meal types*** Ponza (1996) [national] ***Congregate only*** Czajka-Narins (1987) [6 sites] Kohrs (1980) [6 sites]	
Vitamins				
Vitamin A	***Both meal types*** Ponza (1996) [national] Kirschner (1983) [national] ***Congregate only*** Singleton (1980) [7 sites]	***Congregate only*** Neyman (1996) [9 sites] {females} Czajka-Narins (1987) [6 sites] LeClerc (1983) [1 site] Kohrs (1980) [6 sites] Kirschner (1979) [national] ***Home only*** Ho-Sang (1989) [6 sites]	***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites] {males} Kohrs (1978) [6 sites] ***Home only*** Steele (1986) [1 site]	
Vitamin B_6	***Both meal types*** Ponza (1996) [national]	***Congregate only*** Singleton (1980) [7 sites]	***Congregate only*** Neyman (1996) [9 sites]	
Vitamin B_{12}	***Home only*** Ponza (1996) [national]	***Congregate only*** Neyman (1996) [9 sites] {females} Ponza (1996) [national] Singleton (1980) [7 sites]		***Congregate only*** Neyman (1996) [9 sites] {males}

See notes at end of table.

Continued—

Table 42—Findings from studies that examined the impact of the Elderly Nutrition Program on participants' dietary intakes—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Vitamin C	***Both meal types*** Kirschner (1983) [national] ***Congregate only*** Ponza (1996) [national]	***Congregate only*** Neyman (1996) [9 sites] Czajka-Narins (1987) [6 sites] Kohrs (1980) [6 sites] Kirschner (1979) [national] Kohrs (1978) [6 sites] ***Home only*** Ponza (1996) [national]	***Congregate only*** Gilbride (1998) [3 sites] LeClerc (1983) [1 site] Singleton (1980) [7 sites] ***Home only*** Ho-Sang (1989) [6 sites] Steele (1986) [1 site]	
Vitamin D	***Both meal types*** Ponza (1996) [national]	***Home only*** Ho-Sang (1989) [6 sites]	***Congregate only*** Gilbride (1998) [3 sites]	
Vitamin E	***Congregate only*** Ponza (1996) [national]	***Congregate only*** Gilbride (1998) [3 sites] Singleton (1980) [7 sites] ***Home only*** Ponza (1996) [national]	***Congregate only*** Neyman (1996) [9 sites]	
Folate	***Both meal types*** Ponza (1996) [national]	***Congregate only*** Neyman (1996) [9 sites] ***Home only*** Ho-Sang (1989) [6 sites]	***Congregate only*** Gilbride (1998) [3 sites]	
Niacin	***Both meal types*** Kirschner (1983) [national] ***Congregate only*** Ponza (1996) [national] Czajka-Narins (1987) [6 sites] Kohrs (1978) [6 sites]	***Congregate only*** Neyman (1996) [9 sites] {males} Kohrs (1980) [6 sites] {females} Kirschner (1979) [national] ***Home only*** Ponza (1996) [national] Steele (1986) [1 site]	***Congregate only*** Neyman (1996) [9 sites] {females} LeClerc (1983) [1 site] Kohrs (1980) [6 sites] {males} Singleton (1980) [7 sites]	

See notes at end of table.

Continued—

Table 42—Findings from studies that examined the impact of the Elderly Nutrition Program on participants' dietary intakes—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Riboflavin	***Both meal types*** Ponza (1996) [national] Kirschner (1983) [national] ***Congregate only*** Kohrs (1978) [6 sites]	***Congregate only*** Czajka-Narins (1987) [6 sites] LeClerc (1983) [1 site] Kohrs (1980) [6 sites] Singleton (1980) [7 sites] Kirschner (1979) [national] ***Home only*** Ho-Sang (1989) [6 sites]	***Congregate only*** Neyman (1996) [9 sites] ***Home only*** Steele (1986) [1 site]	
Thiamin	***Both meal types*** Kirschner (1983) [national] ***Congregate only*** Ponza (1996) [national]	***Congregate only*** Czajka-Narins (1987) [6 sites] {irregular participation} LeClerc (1983) [1 site] Kohrs (1980) [6 sites] Kirschner (1979) [national] ***Home only*** Ponza (1996) [national] Ho-Sang (1989) [6 sites]	***Congregate only*** Neyman (1996) [9 sites] Czajka-Narins (1987) [6 sites] {regular participation} Singleton (1980) [7 sites] Kohrs (1978) [6 sites]	***Home only*** Steele (1986) [1 site]
Minerals				
Calcium	***Both meal types*** Ponza (1996) [national] Kirschner (1983) [national] ***Congregate only*** Czajka-Narins (1987) [6 sites] Kohrs (1978) [6 sites]	***Congregate only*** LeClerc (1983) [1 site] Kohrs (1980) [6 sites] Singleton (1980) [7 sites] Kirschner (1979) [national] ***Home only*** Ho-Sang (1989) [6 sites]	***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites] ***Home only*** Steele (1986) [1 site]	
Copper			***Congregate only*** Neyman (1996) [9 sites] {females}	***Congregate only*** Neyman (1996) [9 sites] {males}

See notes at end of table.

Continued—

Table 42—Findings from studies that examined the impact of the Elderly Nutrition Program on participants' dietary intakes—Continued

	Significant impact	No significant impact		Significant impact
Outcome	Participants consumed more	Participants consumed more/same	Participants consumed less	Participants consumed less
Iron	***Both meal types*** Kirschner (1983) [national]	***Both meal types*** Ponza (1996) [national] ***Congregate only*** Czajka-Narins (1987) [6 sites] {irregular participation} LeClerc (1983) [1 site] Kohrs (1980) [6 sites] {females} Singleton (1980) [7 sites] Kirschner (1979) [national] ***Home only*** Ho-Sang (1989) [6 sites]	***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites] Czajka-Narins (1987) [6 sites] {regular participation} Kohrs (1980) [6 sites] {males}	***Congregate only*** Nordstrom (1982) [6 sites] Kohrs (1978) [6 sites] ***Home only*** Steele (1986) [1 site]
Magnesium	***Both meal types*** Ponza (1996) [national]		***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites]	
Phosphorus	***Both meal types*** Ponza (1996) [national]	***Congregate only*** Singleton (1980) [7 sites]	***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites] {females} ***Home only*** Steele (1986) [1 site]	***Congregate only*** Neyman (1996) [9 sites] {males}
Potassium	***Both meal types*** Ponza (1996) [national]	***Home only*** Ho-Sang (1989) [6 sites]	***Home only*** Steele (1986) [1 site]	
Selenium			***Congregate only*** Neyman (1996) [9 sites] {males}	***Congregate only*** Neyman (1996) [9 sites] {females}
Zinc	***Both meal types*** Ponza (1996) [national]		***Congregate only*** Gilbride (1998) [3 sites] Neyman (1996) [9 sites]	

See notes at end of table.

Continued—

Table 42—Findings from studies that examined the impact of the Elderly Nutrition Program on participants' dietary intakes—Continued

Outcome	Significant impact: Participants consumed more	No significant impact: Participants consumed more/same	No significant impact: Participants consumed less	Significant impact: Participants consumed less
Other dietary components				
Cholesterol		***Both meal types*** Ponza (1996) [national] ***Congregate only*** Gilbride (1998) [3 sites][1] Neyman (1996) [9 sites]	***Congregate only*** Czajka-Narins (1987) [6 sites] Kohrs (1980) [6 sites] ***Home only*** Ho-Sang (1989) [6 sites] Steele (1986) [1 site]	
Fiber			***Congregate only*** Neyman (1996) [9 sites]	
Sodium		***Home only*** Ho-Sang (1989) [6 sites] Steele (1986) [1 site]	***Both meal types*** Ponza (1996) [national] ***Home only*** Steele (1986) [1 site]	

Notes: Cell entries show the senior author's name, the publication date, the scope of the study (for example, national vs. 1 city or 1 State), and the research approach (P-N = participant vs. nonparticipant study, D-R = dose response study). Where study findings pertain only to a specific subgroup, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in Chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Neither Gilbride et al. (1998) nor Kirschner/ORC (1979) included tests of statistical significance.

[1] Significant results for absolute value only (gm per day). As a percentage of food energy, there was no difference between groups.

In general, the smaller studies did not find significant differences between ENP participants and nonparticipants in vitamins and minerals. As illustrated in table 42, significant differences that were reported included findings that were consistent with the large, national studies (ENP participants consumed more nutrients), and some that were inconsistent (ENP participants consumed fewer nutrients).

Other Dietary Components

The 1993-95 National Evaluation of the ENP found no significant difference between cholesterol intakes of ENP participants and nonparticipants. This finding was true for both congregate and home-delivered ENP participants. Mean cholesterol intakes of all groups were well within the recommended range.

Similarly, the 1993-95 National Evaluation found no significant difference between ENP participants (congregate or home-delivered) and nonparticipants in mean sodium intake. However, the researchers did point out that excessive sodium intake may be a problem for some ENP participants. The average intake of sodium among congregate-meal participants exceeded the recommended maximum. Moreover, both types of ENP participants received more than one-third of the recommended daily maximum for sodium through program meals.

Only one small, local study examined intake of dietary fiber (Neyman et al., 1996). The authors found that participants in congregate ENP sites consumed less fiber than eligible nonparticipants, but the difference was not statistically significant.

Impacts on Nutritional Biochemistries

Four of the small, local studies attempted to assess the impact of the ENP on selected nutritional biochemistries (Neyman, et al., 1996; Czajka-Narins et al., 1987; Nordstrom et al., 1982; Kohrs et al., 1980). All of these studies were limited to congregate feeding sites.

Findings from these studies, summarized in table 43, must be interpreted with caution. None of the researchers attempted to control for selection bias or used analytic techniques to control for measured differences in characteristics of participants and nonparticipants. The fact that the ENP specifically targets individuals with nutritional risks may account for the "negative" findings reported by Czajka-Narins et al. (1987) and Neyman et al. (1996).

All four studies examined iron status using mean levels of hematocrit, hemoglobin, and/or serum iron. Findings for Nordstrom et al. (1982) are summarized in a footnote in table 43. Data could not be included in the table because authors did not report point estimates. Two of the four studies reported significant differences between ENP participants and nonparticipants on one or more measures of iron status for specific subgroups of the population. The pattern of findings was not consistent, however, and there were more "negative" than "positive" differences.

Three of the four studies evaluated serum albumin levels. Serum albumin is used as an indicator of malnutrition (inadequate protein intake) among the elderly. All three studies found that mean serum albumin levels were within the normal range and that the prevalence of less-than-acceptable values did not differ by participation status. However, analyses that compared mean serum albumin values by age and gender found some statistically significant differences between participants and nonparticipants. Neyman et al. (1996) found that male ENP participants had significantly higher serum albumin levels than male nonparticipants. Czajka-Narins et al. (1987) found that the opposite was true for females over 75 who participated in the ENP two or more times per week.

The same three studies examined serum levels of vitamin A, a long-term measure of nutrient intake. Studies by Czajka-Narins et al. (1987) and Kohrs et al. (1980) found that ENP participants had significantly higher levels of serum vitamin A, on average, than did nonparticipants. Note that both of these studies reported that ENP participants consumed more vitamin A than nonparticipants, but the differences were not statistically significant (table 42). Kohrs and associates (1980) also found that ENP participants were significantly less likely than nonparticipants to have an abnormally low level of serum vitamin A.

Limited intake of vitamin A among the elderly had been reported by several investigators (Kim et al., 1993; Kirschner/ORC, 1983; LeClerc and Thornbury, 1983;). Kohrs (1982) emphasizes that "almost one-half of ENP nonparticipants are at risk for vitamin A deficiency" and that improvement in vitamin A status appears to be one of the most important benefits of the ENP.

Kohrs et al. (1980) and Czajka-Narins et al. (1987) also looked at serum levels of vitamin C, which are affected by short-term (rather than long-term) dietary intake. Neither study found a significant difference in mean levels of serum vitamin C. However, Kohrs et al. (1980) found that ENP participants were significantly

Table 43—Findings from studies that examined the impact of the Elderly Nutrition Program on biochemical indicators of nutritional status

Outcome	Significant impact: Participants higher	No significant impact: Participants higher/same	No significant impact: Participants lower	Significant impact: Participants lower
Hematocrit[1]		Neyman (1996) [9 sites][2] Czajka-Narins (1987) [6 sites] {males; regular participants}[2] Kohrs (1980) [6 sites] {males}[2]	Czajka-Narins (1987) [6 sites] {females} Kohrs (1980) [6 sites] {females}[2]	Czajka-Narins (1987) [6 sites] {males; irregular participants}
Hemoglobin[1]	Neyman (1996) [9 sites] {females}[2]	Czajka-Narins (1987) [6 sites] {males; regular participants}[2] Kohrs (1980) [6 sites] {males}[2]	Neyman (1996) [9 sites] {males}[2] Czajka-Narins (1987) [6 sites] {females} Kohrs (1980) [6 sites] {females}[2]	Czajka-Narins (1987) [6 sites] {males; irregular participants}
Serum iron[1]			Neyman (1996) [9 sites] {males}[2] Czajka-Narins (1987) [6 sites] Kohrs (1980) [6 sites][2]	Neyman (1996) [9 sites] {females}[2]
Albumin	Neyman (1996) [9 sites] {males}[2]	Neyman (1996) [9 sites] {females}[2] Czajka-Narins (1987) [6 sites] {males} Kohrs (1980) [6 sites] {males}[2]	Kohrs (1980) [6 sites] {females}[2]	Czajka-Narins (1987) [6 sites] {females; regular participants}[3]
Total protein	Czajka-Narins (1987) [6 sites] {females; irregular participants}	Czajka-Narins (1987) [6 sites] {males}		
Vitamin A	Czajka-Narins (1987) {females} Kohrs (1980) [6 sites][4]	Czajka-Narins (1987) [6 sites] {males} Neyman (1996) [9 sites] {females}	Neyman (1996) [9 sites] {males}	
Vitamin C		Czajka-Narins (1987) [6 sites] {males} Kohrs (1980) [6 sites][4]	Czajka-Narins (1987) [6 sites] {females}	
Vitamin E		Neyman (1996) [9 sites]		
Folate		Neyman (1996) [9 sites] {males}	Neyman (1996) [9 sites] {females}	
Zinc			Neyman (1996) [9 sites]	

See notes at end of table.

Continued—

Table 43—Findings from studies that examined the impact of the Elderly Nutrition Program on biochemical indicators of nutritional status—Continued

Outcome	Significant impact	No significant impact		Significant impact
	Participants higher	Participants higher/same	Participants lower	Participants lower
Cholesterol		Neyman (1996) [9 sites] {females}[2]	Neyman (1996) [9 sites] {males}[1] Czajka-Narins (1987) [6 sites] Kohrs (1980) [6 sites][2]	
HDL Cholesterol		Neyman (1996) [9 sites]		
LDL Cholesterol		Neyman (1996) [9 sites] {females}	Neyman (1996) [9 sites] {males}	
Triglycerides		Neyman (1996) [9 sites] {females}	Neyman (1996) [9 sites] {males}	

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 1 city or 1 State). Where study findings pertain only to a specific subgroup, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

All findings are for congregate meal participants only and, unless otherwise noted, are based on mean values of the indicator.

[1] Nordstrom et al. (1982) looked at the impact of the ENP on iron status. They found no significant effect of participation on hematocrit, hemoglobin, serum iron, or mean cell hematocrit concentration, but did not report whether values were higher or lower for participants compared with nonparticipants.

[2] Authors also looked at percentage of individuals with abnormally low values and found no significant differences between participants and nonparticipants.

[3] As shown in Table 3 of their report. Report text, however, describes the opposite finding (i.e., a significant positive impact of ENP).

[4] Authors also looked at percentage of individuals with abnormal values and found that, compared with nonparticipants, significantly fewer ENP participants had abnormally low values.

less likely than nonparticipants to have less-than-normal levels of serum vitamin C.

Neyman et al., 1996 examined blood levels of vitamin E, folate, and zinc but reported no significant differences between ENP participants and nonparticipants.

Finally, Neyman et al. (1996), Czajka-Narins et al. (1987), and Kohrs et al. (1980), looked at serum cholesterol levels. Neyman and associates (1996) also examined levels of HDL and LDL cholesterol and triglycerides. All of these studies reported that, although ENP participants tended to have lower cholesterol levels than nonparticipants, the differences between the two groups were not significant. This finding was true for both mean cholesterol levels and for the prevalence of elevated cholesterol levels. Neyman et al. (1996) found no significant effect on HDL cholesterol, LDL cholesterol, or triglycerides.

Impacts on Weight Status

Four local studies assessed the impact of the ENP on weight status (Neyman et al., 1996; Ho-Sang, 1989; Czajka-Narins et al., 1987; Kohrs et al., 1980). Findings from these studies, like those related to nutritional biochemistries, are subject to substantial concern about selection bias and must be interpreted with caution.

All four studies used data on height and weight to calculate indices of obesity and thinness, including body mass index (BMI),[138] ponderal index,[139] and the percentage of desirable weight (table 44). Tricep skinfold thickness was used to assess fatness as well as depletion of energy stores.

Kohrs and associates (1980) found that the prevalence of obesity was not significantly related to frequency of participation in the ENP, despite the fact that mean energy intake was greater among participants (table 42). In fact, there was an association (nonsignificant) between lower body weight (based on BMI, ponderal index, and percentage of desirable weight) and program participation. Czajka-Narins et al. (1987) found a comparable pattern among elders over age 75, with the association between ENP participation and lower body weight reaching statistical significance for males. In addition, Kohrs et al. (1980) found that, compared with female nonparticipants, a significantly greater percentage of female ENP participants were thin or wasted.

[138]BMI = [Weight (kg)] / [Height (cm)^2].

[139]Ponderal index is calculated as height (in inches) divided by the cube root of weight (in pounds).

Being too thin is not desirable, particularly among the elderly. These findings suggest that thinner, and perhaps more frail elderly, may be self-selecting into the ENP (on their own volition or because they are targeted by the program). However, the available data are too limited to support a firm conclusion about the relationship between the ENP and the prevalence of thinness/wasting. It remains an interesting question for future research.

Impacts on Socialization

As noted in the introduction to this chapter, the ENP was intentionally designed to address the psychological and sociological needs of the elderly as well as their nutritional needs. The two national evaluations of the program are the only identified studies that attempted to systematically measure social outcomes of participants, relative to a group of eligible nonparticipants (Ponza et al., 1996; Kirschner/ORC, 1979, 1983). The studies employed two different measures, and results were divergent.

In the earliest national evaluation, Kirschner/ ORC (1979, 1983) classified respondents based on isolation using a five-point index: (1) living alone, (2) having too few friends, (3) having no one to confide in, (4) having children that do not visit, and (5) feeling lonely more often. Using multiple regression techniques, the authors found that “being extremely isolated” was significantly associated with use of ENP-sponsored shopping assistance.

The measure of socialization used in the 1993-95 National Evaluation (Ponza et al., 1996) was the number of social contacts per month. The authors found that ENP participants had significantly more social contacts per month than nonparticipants. As expected, the data also showed that homebound participants had less contact than those who attended congregate meal sites.

Impacts on Food Security

The issue of food security among ENP participants has not been well researched and the relationship is a complicated one. Evidence from a national survey of the elderly (Cohen et al., 1993) indicates that elderly FANP participants report higher levels of food insecurity than those who do not participate in FANPs. Those reporting participation in more than one FANP had the highest level of food insecurity. Elderly persons participating in two or more FANPs were more likely to have faced the choice between buying food and paying for medications than elderly persons participating in only one FANP. These patterns presumably do not

Table 44—Findings from studies that examined the impact of the Elderly Nutrition Program on participants' weight status

	Significant impact	No significant impact		Significant impact
Outcome	Participants less obese/ more thin	Participants less obese/ more thin	Participants more obese/ less thin	Participants more obese/ less thin
Body mass index (BMI)	***Congregate only*** Czajka-Narins (1987) [6 sites] {males} Kohrs (1980) [6 sites] {females}[1]	***Congregate only*** Czajka-Narins (1987) [6 sites] {females} Kohrs (1980) [6 sites] {except subgroup noted} ***Home only*** Ho-Sang (1989) [6 sites] {New York City}[2]	***Congregate only*** Neyman (1996) [9 sites]	
Ponderal index	***Congregate only*** Czajka-Narins (1987) [6 sites] {males} Kohrs (1980) [6 sites] {females}[1]	***Congregate only*** Czajka-Narins (1987) [6 sites] {females} Kohrs (1980) [6 sites] {except subgroup noted}		
Percent of desirable weight	***Congregate only*** Kohrs (1980) [6 sites] {females}[1]	***Congregate only*** Kohrs (1980) [6 sites] {except subgroup noted}	***Home only*** Ho-Sang (1989) [6 sites]	
Tricep skinfold thickness		***Home only*** Ho-Sang (1989) [6 sites] {females; upstate New York} Ho-Sang (1989) [6 sites] {males; New York City}	***Congregate only*** Czajka-Narins (1987) [6 sites] Kohrs (1980) [6 sites] ***Home only*** Ho-Sang (1989) [6 sites] {females; New York City } Ho-Sang (1989) [6 sites] {males; upstate New York}	

Notes: Cell entries show the senior author's name, the publication date, and the scope of the study (for example, national vs. 1 city or 1 State). Where study findings pertain only to a specific subgroup, the cell entry also identifies the subgroup {in brackets}.

Nonsignificant results are reported in the interest of providing a comprehensive picture of the body of research. As noted in Chapter 1, a consistent pattern of nonsignificant findings may indicate a true underlying effect, even though no single study's results would be interpreted in that way. Readers are cautioned to avoid the practice of "vote counting," or adding up all the studies with particular results. Because of differences in research design and other considerations, findings from some studies merit more consideration than others. The text discusses methodological limitations and emphasizes findings from the strongest studies.

Unless otherwise noted, findings are based on mean values relative to norms for obesity (where they exist) and/or percentage of persons classified as obese.

[1] Female ENP participants were more likely to be classified as thin than female nonparticipants.

[2] The author reports that, for the upstate New York subgroup, there were no significant differences between ENP participants and nonparticipants in height, weight, or BMI. However, data are not reported and no information is provided on the direction of differences between groups.

reflect an impact of FANP participation, but indicate that individuals who choose to participate in FANPs are more food-insecure than those who do not.

Only one of the identified studies attempted to assess the impact of ENP participation on food security (Edwards et al., 1993). The study focused on a very restricted sample of elderly diabetics who were either receiving home-delivered meals or were on a waiting list for home-delivered meals. Food-insecure individuals were defined as those who reported that they did not have enough money to purchase the foods they needed or had some other difficulty in obtaining food. In this context, the ENP was found to have a positive effect on food security. Elderly diabetics who were receiving home-delivered meals were less likely than comparable elders on a waiting list to be classified as food insecure or to go one or more days per month without food.

Ponza and his colleagues (1996) also assessed food security among ENP participants. Comparable data were not collected for nonparticipants, however. Instead, the authors compared findings for ENP participants with data for the U.S. elderly population overall. Food security was measured using a subset of four of the questions used in the Cohen et al. study (1993) (described earlier). Results indicated that, although most ENP participants reported having enough food to eat, they were much more likely to experience food insecurity than elderly persons in the overall U.S. population.

Impacts on Nutritional Risk

Assessing the nutritional status of elderly individuals is difficult because the factors that determine risk are complex and interdependent. Moreover, nutritional risk among the elderly is influenced by variables that are not considered for most other age groups, including socialization, physical functioning and mobility (frailty), and behavioral elements. To address this issue, the Nutrition Screening Initiative (NSI), a national collaborative effort of professional organizations committed to identifying and treating nutritional problems among the elderly, developed a two-tiered approach to screening for potential nutrition-related problems.[140]

[140]The NSI and associated nutrition screening tools are described in detail elsewhere (Gilbride et al., 1998; Ponza et al., 1996; Posner et al., 1993; Food Research and Action Center, 1987).

The Level 1 screen (table 45) is a simple checklist that can be completed largely by an elderly individual himself or herself, with some additional information obtained through an interview with a social service or health care provider. No laboratory tests or special measurements are required. The Level 2 nutrition screen encompasses a more indepth assessment by a health professional, including measurement of anthropometric, biochemical, clinical, and dietary indicators of nutritional status as well as an assessment of functional status.

The Level 1 NSI screen is currently used in many ENP programs (Dwyer and Mayer, 1997) as a means of identifying individuals who might benefit from a specific nutrition-related service (for example, home-delivered meals, assistance with shopping or cooking, or nutrition education).[141] Research has shown that the Level 1 screen reliably identified individuals at risk for nutrition-related problems (Posner, 1993). There is some concern, however, that the specificity of the measure is less than desirable; that is, it may produce too many “false positives” or overestimate the prevalence of significant nutritional risk (Dwyer and Mayer, 1997).

Using the Level 1 NSI screen on a small elderly population in New York City, Gilbride et al. (1998) found that the level of nutritional risk among congregate meal participants was twice that of a group of comparable elders who did not eat at congregate meal sites. The authors did not assess the statistical significance of this difference.

Ponza et al. (1996) used an approximation of the Level 1 NSI screen to assess nutritional risk among both congregate and home-delivered meal participants. Overall, 64 percent of congregate and 88 percent of home-delivered participants had characteristics associated with moderate to high nutrition risk. No comparisons were made to nonparticipants.

Most of the other published research related to the NSI and use of the NSI screen is descriptive research. However, the increasing use of NSI tools in ENP sites and in other social and health care service delivery sites may lead to outcomes-focused research.

[141]Key elements of the Level 1 screen have been incorporated into a simple self-assessment tool called the DETERMINE checklist. The DETERMINE checklist is also widely used in ENP delivery sites and by other groups and organizations working with older adults (Dwyer and Mayer, 1997).

Summary

Since the inception of the ENP, two national evaluations and a number of smaller local studies have attempted to assess the effectiveness of the program in meeting its goals. All of these studies used quasi-experimental designs (participant vs. nonparticipant), with nonparticipants identified in a variety of ways. Selection bias is a serious issue in all of this research. However, only the most recent national study (Ponza et al., 1996) addressed the problem systematically (although inconclusively). Moreover, much of the available research used unsatisfactory analysis techniques, presenting simple bivariate comparisons with no statistical controls for differences in measured characteristics of participants and nonparticipants.

By all accounts, the ENP is meeting its goal of providing low-cost, nutritionally sound meals to participating elders. Program meals comply with the *Dietary Guidelines for Americans* and most often far exceed the minimum of one-third of the RDA per meal as required by law.

The available research suggests that the ENP is providing elderly participants with more energy and nutrients than they might otherwise consume. The two national evaluations report increased consumption of food energy, protein, and a broad array of vitamins and minerals. The smaller studies generally did not find significant differences, which may be due to the general absence of analytical controls for pre-existing participant/nonparticipant differences as well as small sample sizes.

Table 45—Level 1 Nutrition Screen from the Nutrition Screening Initiative

If any one of the following is true, the individual may be at risk of poor nutritional status:

Body weight
Has lost 5 lb or 5% of body weight in 1 month
Has lost or gained 10 lb or 10% of body weight in the past 6 months
BMI <21[1]
BMI >28[1]

Eating habits
Does not have enough food to eat each day
Usually eats alone
Does not eat anything on one or more days per month
Has a poor appetite
Is on a special diet
Eats vegetables 2 or fewer times daily
Consumes milk or milk products once or not at all daily
Consumes fruit or fruit juice once or not all daily
Eats breads, cereals, pasta, rice, or grains 5 or fewer times daily
Has difficulty chewing or swallowing
Has more than 1 alcoholic drink per day (if woman); more than 2 drinks per day (if man)
Has pain in mouth, teeth, or gums

Living environment
Lives on an income of <$6,000 per year per individual in the household
Lives alone
Is housebound
Is concerned about home security
Lives in a home with inadequate heating or cooling
Does not have stove and/or refrigerator
Is unable or prefers not to spend money on food (<$25-$30 per person per week spent on food)

Functional status
Usually or always needs assistance with any of the following:

Bathing	Eating	Traveling outside home
Dressing	Toileting	Preparing food
Grooming	Walking or moving about	Shopping for food or other necessities

[1]BMI = Body Mass Index = [Weight (kg)] / [Height (cm)^2].

Source: *American Board of Family Practice Reference Guide for Geriatric Patients*, "A Dietary Assessment—Table 5.." (http://www.familypractice.com/references/guidesframe.htm). Accessed June 2003.

While all studies of the impact of the ENP are subject to selection bias, studies that looked at measures other than dietary intake (weight status, nutritional biochemistries, socialization, food security, and nutritional risk) are especially prone to this problem because the program specifically targets elders who are at nutritional or social risk. The limited information that is available suggests that ENP participation is not associated with obesity and that, in fact, thinner, more frail elderly may self-select into the program. With the possible exception of serum vitamin A, which was positively associated with participation in the ENP, drawing firm conclusions about the impact of the ENP on nutritional biochemistries is not possible.

Evidence of the ENP's impact on reducing social isolation and promoting quality of life among the elderly is mixed. While the perceived benefit of social and support services is quite high, only the two national evaluations attempted to systematically measure social outcomes of ENP participants, relative to a group of eligible nonparticipants. The two studies employed different measures of socialization and reported divergent results.

Only one study examined the impact of the ENP on food security. In a very restricted sample of elderly diabetics, the study found that ENP participants receiving home-delivered meals were less food-insecure than nonparticipants on a waiting list for home-delivered meals. On the other hand, Ponza et al. (1996) found that, compared with the overall elderly U.S. population, ENP participants were much more likely to experience food insecurity.

Finally, one small study compared ENP participants and nonparticipants on a relatively simple, yet comprehensive, measure of nutritional risk. The authors report that the rate of nutritional risk among congregate ENP participants was twice that of nonparticipants. The research, however, used no statistical techniques to control for differences between groups or to assess the statistical significance of the observed difference.

The importance of the ENP as a component of the nutrition safety net will continue to increase in coming years as the population ages. Future research on the impacts of the ENP would benefit from a greater focus on impacts among the homebound who are most at risk for poor nutrition and health outcomes and represent an ever-increasing component of the program. In addition, given the focus of the program on social as well as nutritional needs, future research should include comprehensive assessment of the impact of the ENP on both food security and nutritional risk.

Most importantly, future research should emphasize longitudinal rather than cross-sectional designs. Although more costly, longitudinal studies would provide a firmer foundation for studying impacts beyond dietary intakes and for examining the influence of the ENP on seniors' nutrition status, health status, and quality of life over time (Roe, 1989; Posner, 1979).

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Chapter 11

Nutrition Assistance Program in Puerto Rico, American Samoa, and the Northern Marianas

The Nutrition Assistance Program (NAP) in Puerto Rico, American Samoa, and the Northern Marianas provides food and nutrition assistance to low-income individuals through block grants to territory administrative agencies. The territories provide cash or checks to eligible participants. The NAP replaced the Food Stamp Program (FSP), which operated in the territories from 1975 through 1982.

All of the research to date on the NAP has centered on the program in Puerto Rico. Most of this research focused on assessing the impact of replacing the FSP with the NAP but also provides some information about the impact of the NAP per se.

Program Overview

The FSP was introduced in Puerto Rico in FY 1975 and grew rapidly. By 1977, the FSP in Puerto Rico was larger, in terms of both the percentage of the population participating and the total value of benefits issued each month, than any of the programs operating in the 50 States (U.S. General Accounting Office (GAO), 1978). A 1983 study by the Food and Nutrition Service (FNS), U.S. Department of Agriculture (USDA), found that about 56 percent of the Puerto Rican population participated in the FSP in FY 1981. The FSP in Puerto Rico accounted for about 8 percent of FSP participation overall and 8 percent of total Federal expenditures. Although FSP eligibility and program operation standards were identical for the 50 States and Puerto Rico, deductions and monthly benefits were typically lower for Puerto Rican participants.

In response to concerns about the size, expense, and management of the FSP in Puerto Rico, the 1981 Omnibus Budget Reconciliation Act (OBRA) abolished the program and replaced it with an $825 million block grant. Puerto Rican authorities designed the Nutrition Assistance Program (NAP) to administer the block grant beginning in July 1982. The switch from the FSP to a cash delivery system was permanently authorized in September 1985.

The objectives of the NAP and the FSP are identical: to provide low-income households with access to a nutritious diet through increased food purchasing power. Both programs have monthly benefits that vary by household size and net income, and both programs are available to all applicants who meet specified eligibility criteria.

There are three major differences between the NAP and the FSP, however (GAO, 1992). First, NAP benefits are distributed as checks (cash) rather than food stamps (coupons). This switch in the form of the food assistance benefit was motivated by the expectation of considerable savings in administrative costs. (A subsequent study, Beebout et al., 1985, estimated those savings at about $6 million annually.) Distributing benefits as checks was also intended to reduce fraud and theft and to eliminate the problem of food stamp trafficking. Trafficking—exchanging coupons for cash at a reduced value—was known to be widespread in Puerto Rico and was believed to have resulted in a loss of benefits to program participants.

Second, the cash benefits provided by the NAP are not restricted. That is, NAP recipients may elect to spend the cash they receive on something other than food. Food stamp coupons, on the other hand, can be redeemed only for food.

The third difference between the NAP and the FSP is that benefits available through the NAP are constrained by the size of the block grant. The initial NAP block grant of $825 million was $90 million (or 10 percent) less than the FY 1981 FSP allotment.[142] Program administrators had to incorporate stricter eligibility requirements and reduced benefit levels in order to allocate the diminished funds. Relative to the participation rate at the end of the FSP, the 1984 participation rate for the NAP was down 111,000 households, a decline of about 22 percent. Weekly food assistance benefit levels fell an average of $6, a 14-percent decrease (in constant 1984 dollars).

The annual block grant for the NAP in Puerto Rico was held constant at $825 million from FY 1982

[142] Puerto Rican authorities protested the reduction in the total value of the benefit package. Their assertions about potential deleterious effects were countered by arguments that the block grant would provide more flexibility than the FSP and would result in administrative savings (Andrews and Pinchuk, 1984).

through FY 1986. Increases since then have averaged 3-4 percent annually. In FY 2002, the block grant was $1.35 billion (USDA/FNS, 2003).[143]

Participation in the NAP in Puerto Rico has declined somewhat since FY 1991 when, on average, 1.5 million people received NAP benefits. Participation has been roughly level, at around 1.3 million, since FY 1994. The Puerto Rican population has grown steadily throughout this period, however, which means that the percentage of the population receiving assistance has generally declined.[144]

Research Review

All published research investigating the nutrition-related impacts of the NAP has focused on the NAP in Puerto Rico. Three such studies were identified in the literature search.[145] Study characteristics are summarized in table 46. The most widely recognized study in this area is the study completed by Beebout et al. in 1985. This study, as well as the more recent study by Bishop and his colleagues (1996), focused mainly on assessing the impact of replacing the FSP with the NAP but also provides some information on impacts of the NAP itself. The third study (Hama, 1993) compared NAP participants with nonparticipants in 1984, the second full year of operations under the block grant.

All three studies used data from the 1977 Puerto Rico Supplement to the Nationwide Food Consumption Survey (NFCS) and/or the 1984 Puerto Rico Household Food Consumption Survey (HFCS). The former survey was conducted while the FSP was still in place. Data for the latter were collected early in the life of the NAP.

The 1977 and 1984 survey samples were both representative of the Puerto Rican population of housekeeping households,[146] and the data collection methodologies were almost identical. Data were obtained from the person identified as most responsible for meal planning and preparation. A 7-day, aided-recall questionnaire was used to obtain information about food used from household supplies. For each food item used, information was obtained on the kind of food (for example, ground beef or whole milk), the form of the food (fresh, canned, or frozen), the quantity used, the price paid (if appropriate), and the source (purchased, home-produced, gift, or payment). Data were also collected on snacks and refreshments eaten by guests and on the number of meals eaten away from home and associated expenditures.

The studies by Beebout et al. (1985) and Hama (1993) examined impacts on household food expenditures. All three studies examined impacts of the NAP on nutrient availability at the household level.[147] These two outcomes are related. The hypothesis is that food assistance benefits lead to an increase in food expenditures, which leads to an increase in the amount of nutrients available to the household. In theory, an increase in nutrient availability leads to an increase in nutrient intake at the individual level; however, none of the available studies of the NAP looked at nutrient intake or at other nutrition- and health-related outcomes.

Impact on Food Expenditures

Both Beebout et al. (1985) and Hama (1993) estimated impacts of the NAP on household food expenditures. Beebout and his colleagues reported a positive effect, while Hama reported a negative effect. Theoretical and methodological considerations limit the credibility of Hama's finding, as discussed below.

The study conducted by Beebout et al. was intended principally to evaluate the impact of the NAP relative to the FSP. With regard to household food expenditures, the research question was whether the change from the FSP to the NAP was associated with a change in the amount of money households spent on food. The study used the 1977 NFCS data (collected when the FSP was in place) and the 1984 HFCS data (collected early in the life of the NAP). Analyses attempted to separate the effect of switching from food stamp (coupon) benefits to cash (checks) from the effect of the tighter eligibility criteria and reduced benefit levels associated with the NAP.

[143] The FY 2002 block grants for the Pacific Islands covered under the program (American Samoa and the Northern Marianas) were $5.3 million and $6.1 million, respectively.

[144] Information on participation figures for the Pacific Islands was not available.

[145] In 1990, Congress directed the GAO to study the NAP to determine whether NAP recipients were receiving the same nutritional benefits as other U.S. citizens receiving food assistance benefits. GAO determined that such a study could not be completed because of time and costs constraints. Consequently, the GAO prepared a report that summarized available research (GAO, 1992).

[146] Housekeeping households are those with at least one member having 10 or more meals from the home food supply.

[147] "Nutrient availability" reflects the nutrient content of foods used from the household food supply. This measure differs from nutrient intake because it (1) includes food that is wasted, fed to pets, or eaten by guests and (2) does not include food that is obtained and eaten outside of the household (for example, restaurant meals and foods eaten as a guest in other homes).

Table 46—Studies that examined the impact of the Nutrition Assistance Program in Puerto Rico on household food expenditures and/or nutrient availability

Study	Outcome(s)	Data source[1]	Population (sample size)	Design	Measure of participation	Analysis method
Bishop al. (1996)	Household nutrient availability	1977 Puerto Rico supplement to the NFCS and 1984 Puerto Rico HFCS	Participant and income-eligible nonparticipant households using 1977 eligibility criteria (n= 3,995)	Pre-cashout compared with cashout (1977 vs. 1984)	Participation dummy	Stochastic dominance
Hama (1993)	Household food expenditures Household nutrient availability	1984 Puerto Rico HFCS	Participant and nonparticipant (including ineligible) households (n=1,559)	Participant vs. nonparticipant	Participation dummy	Multivariate regression
Beebout et al. (1985)	Household food expenditures Household nutrient availability	1977 Puerto Rico supplement to the NFCS and 1984 Puerto Rico HFCS	Participant and income-eligible nonparticipant households using 1977 eligibility criteria (n= 3,995)	Pre-cashout compared with cashout (1977 vs. 1984)	Group membership dummy, participation dummy, and benefit amount	Multivariate regression, with 2-equation selection-bias models

[1] Data sources:
NFCS = Nationwide Food Consumption Survey.
HFCS = Household Food Consumption Survey.

Beebout and his colleagues estimated the marginal propensity to spend on food (MPSF) out of food stamp benefits (based on the 1977 NFCS data) and out of NAP cash benefits (based on the 1984 HFCS data). They reported a positive and significant impact for NAP participation, with an MPSF of 0.21 for at-home food expenditures and 0.23 for away-from-home food expenditures. These effects translate into an estimated impact on weekly food expenditures of $2.39 per Adult Male Equivalent (AME) per week for at-home food expenditures and $2.61 per AME per week for total food expenditures. The MPSF for NAP income was greater than the MPSF from ordinary income, but the statistical significance of the difference was not tested.

In comparing estimated impacts for the FSP and the NAP, the authors found that point estimates for the MPSF out of program benefits was positive and significant for both programs and that differences between NAP and FSP coefficients were small and not statistically significant. These results suggest that both the FSP and the NAP increased food expenditures and that the relative impact of both programs was roughly equivalent. While there is no reason to question the basic finding—that the NAP leads to an increase in food expenditures—there is reason to question the broader finding—that the impact of the NAP (cash) is equivalent to the impact of the FSP (coupons). The study's reliance on a pre-/post-design and use of survey samples that are separated by a 7-year interval makes it considerably weaker than other studies that have looked at the differential impact of cash and coupons. In addition, some have argued that the FSP in Puerto Rico was essentially "cashed out" before the NAP was instituted (Moffitt, 1989). That is, FSP coupons were used as a second form of currency even before the changeover.

As discussed in detail in chapter 3, the strongest study completed to date on the impact of cashing out food stamps (Ohls et al., 1992) "establishes firmly that the coupon format of food stamps causes the FSP to increase household expenditures on food at home by a greater amount than would occur if the households received the same benefit amount as cash" (Burstein et al., 2004). This finding, coupled with the relative weakness of the NAP vs. FSP comparison in the Beebout et al. study, suggests that the positive impact of the NAP on household food expenditures may, in fact, be less than the impact that would occur under the FSP.

The only other available study of the impact of the NAP on household food expenditures was completed by Hama (1993). Hama used the 1984 HFCS data set (collected early in the life of the NAP) to compare NAP participants with nonparticipants. The nonparticipant sample included both eligible and ineligible households.

Hama did not produce MPSF estimates. Rather, she estimated the average difference between participant and nonparticipant households' weekly food expenditures, controlling for household income, household size, and urbanization. Her conclusions were very different from those reported by Beebout et al. (1985). Hama found that NAP households spent about $5 less per week on at-home food expenditures than did nonparticipating households, a statistically significant effect.[148] However, serious limitations in Hama's analysis undermine the credibility of her result.

First of all, Hama's result does not necessarily indicate that NAP households spent less for food than they would have spent in the absence of the benefit. Rather, it implies that NAP households spent less for food than the amount that would have been spent by nonparticipating households with the same total income. One likely reason for this odd finding is selection bias.[149] Such bias may be exacerbated by Hama's use of the entire sample (rather than limiting the analysis to low-income households), in conjunction with an assumed linear relationship between income and food expenditures. If a curvilinear specification were more appropriate, as some researchers argue, then the households on the extreme ends of the income distribution would tend to have actual expenditures below their predicted expenditures (Moffitt, 1989). The negative coefficient for the dummy variable identifying participant households (all at the low end of the income range) may have simply reflected a preponderance of negative residuals at the low end of the income range due to a poor fit to a straight line. Had the sample been limited to low-income households, then the participation dummy would be independent of income and this potential source of bias would have been eliminated.

Impact on Household Nutrient Availability

All three of the identified studies examined the impact of the NAP on availability of food energy and selected nutrients at the household level. Analyses focused on nutrients considered to be potentially low in the diets

[148]Note that Hama's results are reported in $/household/week, whereas Beebout et al. (1985) reported results in $/AME/week.

[149]Beebout et. al. (1985) attempted to control for selection bias in their models and limited the analysis sample to low-income, program-eligible households.

of Puerto Rican households (calcium, iron, magnesium, vitamin A, and vitamin B_6). Bishop and his colleagues also studied availability of riboflavin and niacin. The analysis methods used in the three studies were widely divergent.

The models used by Beebout and his colleagues (1985) assumed that NAP impacts on nutrient availability stemmed from impacts on food expenditures. The authors first estimated NAP impacts on food expenditures. Then, in a separate model, they estimate the relationship between food expenditures and availability of a particular nutrient. Next, to get the estimated impact of the NAP on the availability of a given nutrient, they multiplied the estimated NAP impact on food expenditures by the coefficient for the relationship between expenditures and a particular nutrient. Thus, the models assumed that at-home food expenditures from NAP benefits generate the same nutrient values as equal at-home food expenditures from ordinary income.

Using this methodology, Beebout et al. concluded that the NAP reduced the percentage of participating households that failed to attain 100 percent of the RDA for food energy and for five vitamins and minerals. Reductions between 5.0 percentage points (food energy) and 6.7 percentage points (magnesium) were estimated. No tests of statistical significance were provided for the impact of the NAP per se. The significance of differences between the FSP and the NAP was tested, and none of the differences was significantly different from zero.

Bishop et al. (1996) used the 1977 NFCS data and the 1984 Puerto Rico HFCS data to determine whether household nutrient availability in the Puerto Rican population as a whole was different when the FSP was in effect than when the NAP was in effect.[150] They compared household nutrient availability among all island residents in 1977 vs.1984. In each data set, they also compared program participants (FSP participants in the 1977 dataset and NAP participants in the 1984 data set) with nonparticipants.

The authors compared distributions of household nutrient availability, with particular focus on households at the lowest end (lowest quintile) of the income distribution. They used stochastic dominance methods, which essentially compared household nutrient availability at each of five income quintiles. If one population had higher mean availability at all five income quintiles, then it was said to have first degree stochastic dominance (FSD) over the second population. T-tests were also used to compare means at each of the five income quintiles. For each nutrient analyzed, the authors reported whether or not there was FSD (higher means at all income levels) and whether or not there was a statistically significant difference in means at the lowest quintile of income. The authors also carried out subsample analyses that compared the poorest quintile of participant and nonparticipant households in each data set, using ordinary least squares regression. For purposes of this review, results of these analyses are the most relevant.

Results of the 1984 versus 1977 analysis showed that the distributions of energy availability before and after the NAP were not significantly different. Results for nutrients varied. Some distributions improved significantly after the NAP (iron, vitamin A, and niacin), some worsened significantly (calcium and riboflavin), and some remained the same (magnesium and vitamin B_6). In examining impacts by income quintiles, the authors noted that all of the improvements reached the lowest income quintile while the negative changes did not.

Bishop and his colleagues also compared energy and nutrient availability among NAP participants and nonparticipants, using only the 1984 data set. They restricted the sample to households in the lowest quintile of the nutrient distribution under consideration. Among these high-risk households, NAP participation was associated with greater availability of food energy and six of the seven nutrients examined (all but calcium). Differences were statistically significant for iron, magnesium, and vitamin B_6.

Hama (1993) presented impacts of NAP participation on household nutrient availability, estimated from reduced-form regression equations. The estimates were positive for energy, calcium, and magnesium and negative for vitamin A and vitamin B6. No statistical test results were reported.

Summary

The available information on the nutrition-related impacts of the NAP (in Puerto Rico) must be considered to be both limited and dated. All three of the studies reviewed used the 1984 Puerto Rico Household Food Consumption Survey, which was conducted just 2 years after the NAP replaced the FSP in Puerto Rico.

[150] The sample included participants and nonparticipants, and the analysis did not differentiate between them.

Because the NAP gives households extra income, it is a foregone conclusion, given results of research on the FSP (see chapter 3), that the program will increase participants' food expenditures, on average. Not surprisingly, the only study to estimate a marginal propensity to spend (MPS_F) on food out of NAP benefits (Beebout et al., 1985) found a positive effect. The estimated MPS_F out of NAP benefits was greater than the MPS_F out of ordinary income, but the statistical significance of the difference in coefficients was not tested. The other study that examined food expenditures did not estimate the MPS_F (Hama, 1993). Results of this study imply that the MPSF out of NAP benefits would be lower than the MPSF out of ordinary income, but this result may stem from selection bias.

Evidence that the NAP affects household nutrient availability is weak but suggests that the NAP may result in small increases in the amount of energy and nutrients available to households. All three of the studies reviewed here examined impacts on household nutrient availability and found that the NAP increased availability of food energy and several vitamins and minerals. Only one study (Bishop et al., 1996) reported on the statistical significance of differences between NAP participants and nonparticipants, however, and not all of the apparently positive results were statistically significant.

Any serious understanding of current impacts of the NAP on participants' nutrition and health status will clearly require new research. The existing national survey of health and nutrition status, the National Health and Nutrition Examination Survey (NHANES), does not include Puerto Rico or the Pacific Islands. Consequently, a specialized data collection will be required to address questions about the nutrition- and health-related impacts of the NAP.

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Chapter 12

Commodity Supplemental Food Program

The Commodity Supplemental Food Program (CSFP) began in 1968. With the goal of improving the health of low-income women and their infants and children, CSFP provided supplemental foods, information about good nutrition, and a link to health care. Over time, the program's focus has expanded to include the low-income elderly, who currently make up the bulk of program participants. Little research has been done on the CSFP, and none has been done at the national level.

Program Overview

The impetus for creation of the CSFP stemmed largely from response to concerns in the late 1960s about hunger and malnutrition among vulnerable low-income populations. The "Hunger in America" report, released by the National Board of Inquiry, as well as the "Poor People's Campaign," led by several advocacy groups, were especially influential in generating the groundswell of concern that led to the program's creation (Mahony Monrad et al., 1982).

The Supplemental Food Program, as it was initially known, was developed jointly by the U.S. Departments of Agriculture (USDA) and Health, Education, and Welfare (the forerunner of the current U.S. Department of Health and Human Services). The program provided food packages, including evaporated milk, corn syrup, and "reinforced" cereals, to low-income women, infants, and preschool children. Food packages were distributed to participants—upon "determination [of need] by a competent medical authority"—through health clinics, visiting nurses, and health centers that served low-income populations (Mahoney Monrad et al., 1982).

Over time, other types of social service organizations have come to serve as local CSFP agencies. In the current configuration, not all local agencies that provide commodity foods also provide direct health services, but all are encouraged to provide health information and links. In addition, with the inception and growth of the Special Supplemental Program for Women, Infants, and Children (WIC) and the growth of interest in issues related to aging, the CSFP has shifted emphasis toward the low-income elderly.[151] Elderly participation in the CSFP began with a pilot project in FY 1982.

Today, individuals eligible to receive free commodity foods through the CSFP include low-income individuals who are elderly (at least 60 years old); women who are pregnant, breastfeeding, or up to a year postpartum; infants; and children who have not yet reached their sixth birthday. Women, infants and children are not eligible to participate in the CSFP if they participate in the WIC program. Eligible individuals must reside in the State or Indian reservation that administers the CSFP. States may determine other local requirements, such as nutritional risk or residence in a particular area. For women, infants, and children, income eligibility requirements are established by individual States (typically 185 percent of poverty). For elderly persons, income eligibility is federally set at 130 percent of poverty (USDA/Food and Nutrition Service (FNS), 2003a).

Individual States may or may not participate in the program. To initiate the CSFP or to continue it each year, States apply to USDA by submitting a plan for program operations. Each State plan includes information about all local agencies expected to administer the program in the coming year, income criteria and nutritional risk criteria to be used in determining eligibility, plans for outreach and nutrition education, procedures for monitoring local agencies, procedures for involving local agencies in planning for the following year, plans for financial management and detection of duplicate participation, and audit procedures (7 CFR, Part 247).

Local agencies use a variety of outreach and education strategies to reach potential participants. Typically, a

[151]Startup problems with the CSFP may have catalyzed the development of the WIC program. In 1970, after about a year of CSFP operations, USDA began expressing concerns that many CSFP programs were surpassing budgets and providing food to individuals who may not have been eligible. As a result, USDA temporarily stopped expansion of existing CSFP projects, prohibited initiation of new projects, and limited eligibility to pregnant women, postpartum women up to 12 months after delivery, and infants up to 12 months of age. At the same time, USDA began experimenting with an alternative pilot program that would provide pregnant and postpartum women with certificates redeemable in retail stores for infant formula, infant cereal, and milk. Concern within Congress about having curtailed the CSFP program hastened the expansion of this pilot program into what eventually became the WIC program (Mahony Monrad et al., 1982).

client comes to a local agency, has his/her eligibility confirmed, and then receives a monthly food package, along with nutrition education. Food packages are tailored to meet individual needs. Packages contain foods such as infant formula and cereal, nonfat dry and evaporated milk, juice, farina, oats, ready-to-eat cereal, rice, pasta, egg mix, peanut butter, dry beans or peas, canned meat, poultry or tuna, cheese, and canned fruits and vegetables (USDA/FNS, 2003a). Local agencies use a variety of methods to provide nutrition education.

The CSFP is one of the smaller of USDA's food and nutrition assistance programs. As noted, the program does not operate in all 50 States. In FY 2003, 32 States, the District of Columbia, and two Indian reservations were authorized to operate the program (USDA/FNS, 2003a). In FY 2002, 427 million individuals, the majority of whom were elderly, participated in the CSFP each month. The total Federal expenditure for the program was $110 million, or less than 1 percent (0.3 percent) of total USDA expenditures for food and nutrition assistance (USDA/FNS, 2003b).

Research Review

Research on the CSFP is scant. Only one study addressing impacts of the CSFP on participants was identified in the literature (Mahony Monrad et al., 1982). This 1982 evaluation, completed for USDA, collected retrospective administrative and medical records data from two CSFP project sites in Memphis, TN, and one CSFP project site in Detroit, MI. All three of the sites were large. Together, they accounted for 43 percent of all CSFP participants.

Participants from the selected CSFP sites were compared with a sample of nonparticipating pregnant women and children drawn from the same local health care facilities. Pregnant women in the treatment and comparison groups were matched with respect to race, marital status, age, number of previous pregnancies, smoking behavior, and prepregnancy weight. Children in treatment and comparison groups were matched with respect to sex, race, and birthweight. The final sample included 842 pregnant women and 472 children.

The authors provide no information on the processes by which some women and children became CSFP participants and other, apparently equivalent, individuals at the same facilities did not. One possibility is that caseloads were limited, in which case, we might expect little or no selection bias. Alternatively, participation may have been based on the individual's request, or health care providers may have made judgments about need in recommending some people for participation and not others. If the latter scenario is true, selection bias could be an important factor even though the two groups were matched on recorded characteristics.

Program exposure data, collected for participants, included number of food package pickups, number of health care visits, and participation in social welfare programs. Outcome measures collected for women included hemoglobin and hematocrit, pregnancy weight gain, and birth outcomes. Outcome measures for children included hemoglobin and hematocrit, immunization status, height-for-age, weight-for-age, and weight-for-height. Analysis methods included t-tests and multivariate analysis of covariance.

For pregnant women, impacts of CSFP were positive and statistically significant. Participants delivered infants with greater gestational age, birthweight, and birthweight adjusted for gestational age. They also had lower incidence of low birthweight and shorter length of stay in the hospital after birth. The association between participation and improved birth outcomes was significantly greater among high-risk pregnancy groups, including women with a delivery age of younger than 16 years or women who were anemic at the beginning of their participation, who received inadequate prenatal care, who had five or more previous pregnancies, or whose weight gain during pregnancy was less than 11 pounds. The amount of food received and the amount of prenatal care received both had statistically significant associations with improved birth outcomes.

For children, the amount of CSFP services received was associated with some positive outcomes, but differences in these associations across study sites, as well as severe problems with sample attrition, led the authors to avoid conclusive statements about the overall effects of the program on children. In two of the three sites, CSFP participation was associated with a significantly lower incidence of low weight-for-height.

Only one study focusing on elderly CSFP participants was identified but it was entirely descriptive. Koughan and Atkinson (1993) studied 104 elderly CSFP participants in New Orleans, LA. The researchers measured participants' height and weight and completed a screening checklist (the DETERMINE checklist developed by the Nutritional Screening Initiative (NSI)) specifically designed to determine nutritional risk among the elderly (Posner et al., 1993).

The authors reported that 80 percent of the CSFP participants studied were at moderate or high nutritional risk as measured by the DETERMINE checklist. Participants were an average of 69 years old, and about three-fourths were African-American females. The median body mass index (BMI) was 30, indicating obesity, and, of those with a BMI over 30, 50 percent were considered to be at high nutritional risk. All of those who had a BMI under 21, indicating that they were underweight for their height, were considered to be at nutritional risk.[152,153]

[152]The number of participants with BMI under 21 was not reported.

[153]This is a more conservative cutoff for defining overweight than the cutoff recommended by the Centers for Disease Control and Prevention (Schoenborn et al., 2002).

Summary

Existing literature provides very little information on which to base an understanding of the impact of the CSFP on participants' nutrition and health status. Only one study has attempted to estimate the impact of the CSFP. It found positive, statistically significant impacts for pregnant women and suggestive evidence of positive impacts for children. That study is quite dated, however, and may have been subject to selection bias. Moreover, the study population did not include the elderly, who now account for about three-quarters of program participants.

Clearly, the CSFP is a program that needs evaluation. Not only is research needed on the impacts of the program on participant nutrition and health status, but also on why States and Indian reservations choose to operate the CSFP and how it interacts with the WIC program and the Nutrition Services Incentive Program, formerly known as the Nutrition Program for the Elderly.

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Chapter 13

Food Distribution Program on Indian Reservations

The Food Distribution Program on Indian Reservations (FDPIR) provides monthly supplemental food packages to low-income households living on Indian reservations and to eligible American Indian households living in approved areas near reservations. Household eligibility to participate in the FDPIR is based on the Federal income and asset requirements used in the Food Stamp Program (FSP).

Research literature focusing specifically on the FDPIR is very sparse. The few FDPIR-specific papers and reports identified through the literature search describe the role of the FDPIR in the food supply on American Indian reservations. No scientific research has evaluated the impact of the program on nutrition- and health-related outcomes.

Program Overview

The FDPIR was authorized under the Food Stamp Act of 1977.[154] In establishing the FDPIR, Congress cited concerns that the FSP might not adequately meet the food assistance needs of low-income American Indian households living on or near reservations (Usher et al., 1990). The primary concern was that the remote location of many reservations made it difficult for American Indian households to participate in the FSP. In many instances, the distance between the reservation and the local FSP offices was substantial and/or food stores where FSP coupons could be redeemed were scarce or far away. Thus, the FDPIR was designed to provide an alternative to the FSP for low-income American Indian households living on or near reservations.

Income eligibility for the FDPIR is based on federally defined income eligibility requirements used in the FSP. However, the FDPIR does not impose FSP requirements related to employment and training or time limits for able-bodied adults without dependents (ABAWDs). All households residing on Indian reservations are eligible to participate in the program if they meet income and resource standards. Households living in approved areas near reservations or in Oklahoma are eligible to participate if at least one member of the household is a member of a federally recognized tribe.[155]

Households are individually certified by local offices and are recertified periodically at intervals not to exceed 1 year. Eligible households may choose to receive either FDPIR benefits or food stamps, but not both. Participating households receive a monthly food package weighing between 50 and 75 pounds. In FY 1998, FDPIR food packages were updated in response to an extensive review conducted the prior year. This review was recommended by the Commodity Improvement Council (CIC), which was established by the U.S. Department of Agriculture (USDA) to address concerns about the quality of foods offered in the FDPIR. A primary concern was the high fat content of food packages (Dillinger et al., 1999; Smith, et al., 1996, 1993; USDA, Food and Nutrition Service (FNS), 1995). Concerns were also raised about the lack of fresh produce and fresh or frozen meats and poultry (Dillinger et al., 1999) and about levels of sodium and sugar (USDA/FNS, 1995).

The updated food packages added several new products, including low-sodium and low-fat foods and frozen, cut-up chicken. Changes were designed to make food packages easier to use and more compatible with the preferences and nutritional needs of American Indians. The fat content of food packages was reduced, relative to total energy content, and servings of vegetables and grains were increased (USDA/FNS, 2002).

In FY 2003, more than 70 different food items were offered, including canned beef, poultry, and fish; canned fruits, vegetables, and juices; dried fruits; dehydrated potatoes; canned soups; canned spaghetti sauce; packaged macaroni and cheese and other types

[154] Earlier versions of commodity distribution programs on Indian reservations were included in the 1949 and 1963 Agriculture Acts, as well as the 1973 Agriculture and Consumer Protection Act. The Federal Government has provided limited supplies of food in various forms to American Indians since the time when most Indians living east of the Mississippi River were forcibly removed to reservations in the West and Midwest. At one point, the food distribution programs served U.S. territories in the Pacific Islands as well as Indian reservations. Most of the Pacific Island sites were phased out during the 1980s and 1990s, as the islands converted from U.S. territories to commonwealths (U.S. Department of Agriculture (USDA), Food and Nutrition Service (FNS), 2003a).

[155] In Oklahoma, which has few reservations, low-income households that include at least one American Indian and reside in designated areas (including some urban areas) may participate in the FDPIR (USDA/FNS, 2003b).

of pasta; cereals, rice and other grains; cheese; egg mix; peanuts; peanut butter; low-fat refried beans; and nonfat-dry and evaporated milks (USDA/FNS, 2003b). Staples, such as flour, cornmeal, bakery mix, corn syrup, vegetable oil, and shortening were also offered. Frozen ground beef and chicken and/or fresh produce were also available to most programs that have facilities to store and handle these foods.[156]

In addition to providing food, the FDPIR makes printed materials available to participants, such as guidance on how to use FDPIR foods as part of a healthy diet, commodity fact sheets that provide storage and preparation tips, nutrition information and recipes, and a "Nutrition Facts" booklet that lists the ingredients and nutrient composition of available commodities (USDA/FNS, 2003b). Sponsoring agencies can also apply for additional Federal funding to be used specifically for nutrition education.

The FDPIR is administered at the State and local levels by State agencies and Indian Tribal Organizations (ITOs). USDA provides food and administrative funding to the State agencies and ITOs, which are then responsible for program operations, including food storage and distribution, eligibility certification, and nutrition education. In FY 2003, the FDPIR was administered by 98 ITOs and 5 State agencies and provided benefits to approximately 243 American Indian tribes (USDA/FNS, 2003b). In FY 2002, approximately 110,000 individuals participated in the program each month, at an annual cost of $69 million (USDA/FNS, 2003c).

Research Review

Research focusing specifically on the FDPIR is sparse and there have been no impact evaluations of the program. One nationally representative study of the FDPIR has been completed (Usher et al., 1990). The primary objectives of that study, which was based on data collected in 1989, were to describe program operations, describe sociodemographic characteristics of FDPIR households, identify dietary needs and preferences of low-income American Indians and examine how the FDPIR addresses those needs, and compare availability and acceptability of the FDPIR versus the FSP in providing food assistance. Other available literature generally describes the role of the FDPIR in the food supply on Indian reservations, characteristics of the diets of specific subgroups of American Indians, and/or special nutrition and health challenges facing American Indians. Major themes from the available literature are briefly summarized below.

Characteristics of FDPIR Households

In the only nationally representative study of the FDPIR, Usher and his colleagues (1990) found that FDPIR households were very poor. Nearly 1 in 10 FDPIR households reported having no income. More than one-third had gross incomes that were equivalent to or less than 50 percent of the 1989 Federal poverty level. Only one in five households had incomes above the poverty level.

About half of all FDPIR households included children. Almost one-quarter (23 percent) of FDPIR households were single adults living alone. Compared with the general population of low-income households, more FDPIR households included one or more elderly people and fewer FDPIR households were single-parent, female-headed households. Roughly 40 percent of all FDPIR households included an elderly person compared with 16 percent of low-income households in the general population. Single-parent, female-headed households accounted for roughly 9 percent of all FDPIR households compared with 47 percent of low-income households in general. Researchers documented a strong tendency for households that were receiving Aid to Families with Dependent Children (AFDC) benefits to participate in the FSP rather than the FDPIR.

Usher et al. found that most FDPIR households had adequate food storage and preparation facilities. However some FDPIR households lacked at least one of five basic facilities: 20 percent did not have hot running water, 15 percent had no indoor running water, 9 percent did not have a refrigerator, 6 percent did not have a stove or other cooking facility, and 7 percent had no electricity. All of these conditions were much more frequent in the Western Region than in other regions. Three-quarters of the households that lacked running water and 90 percent or more of the households without refrigerators or electricity lived in the Western Region.[157]

Importance of the FDPIR in the Food Supply on Reservations

Many American Indian families may depend on the monthly FDPIR food packages as their primary source

[156]Even when offered, some families are not able to use fresh or frozen foods because they do not have refrigerators (Ballew et al., 1997).

[157]Refers to one of FNS's seven regions. The Western Region includes Alaska, Arizona, California, Hawaii, Idaho, Nevada, Oregon, and Washington (and ITOs operating in those States). In 1989, about 30 percent of all local FDPIR programs were located in the Western Region.

of food. In its 1990 review of food assistance programs on four Indian reservations, the General Accounting Office (GAO) noted that, for many Indians, the food assistance programs "constitute their primary and long-term food supply because of persistent unemployment on the reservations" (GAO, 1990).

In 1993, numerous tribal officials from reservations in the West and Northwest testified at a Senate hearing on "Barriers to Participation in Food Stamp and Other Nutrition Programs of the Department of Agriculture by People Residing on Indian Lands." The officials indicated that the American Indian residents on their reservations relied on the FDPIR as their primary source of food (U.S. Senate Committee on Indian Affairs and Senate Committee on Nutrition and Forestry, 1993).

In his testimony at the joint hearing, Mr. John Yellow Bird Steele, President of the Oglala Sioux Tribal council, testified that "the USDA food distribution programs, all of them, are very much needed on Pine Ridge Reservation. They are viewed not as subsistence. They are viewed as a primary source of food." Similar views were expressed by virtually every person who testified at this hearing, representing Indian tribes, reservations and trust lands, and organizations that served American Indians.

Wolfe and Sanjur (1988) studied the diets and food and nutrient intakes of 107 women attending food distributions on the Navajo reservation. They found that commodity foods contributed 43 percent of total energy intake and close to 50 percent of all nutrients examined, except vitamins A and C. Although mean nutrient intakes were found to be below the RDA for energy, calcium, iron, vitamin A, vitamin C, and phosphorus, the pattern of vitamin and mineral intakes was similar to that of women in the general population. Moreover, the percentages of energy derived from fat, carbohydrates, and protein in the diets of these low-income Navajo women were closer to those recommended in the *Dietary Guidelines* than were the percentages in the diets of women in the Nation as a whole. The authors concluded that:

> The relative adequacy of the women's diet, despite their very low income levels, was associated with substantial use of foods provided by the Food Distribution Program. Except for vitamins A and C, commodity foods were the source of approximately 50 percent of nutrient intakes. Thus, this program appeared to make an important nutritional contribution to the contemporary Navajo diet.

Research has provided some evidence that the importance of the FDPIR as a component of the nutrition safety net has increased on some reservations in recent years. Davis et al. (2002) found that FDPIR caseloads increased on the Northern Cheyenne reservation in Montana, while enrollment in the FSP declined.[158] The authors report that many factors contributed to this shift. One was lack of transportation (access to a vehicle and/or money for gas) to shop off the reservation, where prices are lower. In addition, work requirements were seen as a disincentive because of high unemployment rates and a perception that finding even a minimum wage job would result in a loss of benefits for the household.

Characteristics of the Diets of American Indians

A number of reports and journal articles have assessed the quality of the diets of American Indians, with no regard to presence or absence of FDPIR (although, as noted in the 1993 Senate hearings, one can safely assume that FDPIR foods play an important role in the diets of most American Indians living on or near reservations). Several conclusions appear repeatedly in the literature. Most of the studies summarized here are based on data that were collected before the changes in FDPIR food packages. However, findings from the few more recent studies that are available are consistent with findings from earlier research.

The general finding is that the high prevalence of protein, calorie, and vitamin and/or mineral deficiencies reported by researchers during the 1960s has been significantly reduced (Van Duzen et al., 1976). Inadequate intake of key nutrients remains a problem, especially for vulnerable age groups, such as children, women of child-bearing age, and the elderly (Ballew et al., 1997). However, concerns about nutrient intakes of American Indians largely reflect those of the overall population. For example, many are concerned that the diets of many American Indians living on or near reservations are too low in variety (number and types of different foods consumed), fruits, and vegetables and too high in fat (relative to food energy), highly sweetened and salted foods, and heavily sweetened drinks (Cole et al., 2001; deGonzague et al., 1999; Harnack et al., 1999;

[158] For the Nation as a whole, participation in the FDPIR has not increased. Since FY 1999, average monthly participation has declined from 129,000 participants per month to 110,000 participants per month (FY 2002). (USDA/FNS, 2003c).

Story et al., 1998a, 1998b; Ballew et al., 1997; Vaughan et al., 1997; Campos-Outcalt et al., 1995; Brown and Brenton, 1994; Jackson, 1993; Teufel and Dufour, 1990; Wolfe and Sanjur, 1988).

In addition, several researchers (Vaughan et al., 1997; Calloway and Gibbs, 1976) observed preferences among American Indians for fried foods, including fry bread, fried potatoes, and fried meats. These foods are typically fried in lard, commodity shortening, or butter rather than vegetable oils (Wolfe and Sanjur, 1988). In addition, commodity cheese has been a significant source of fat and sodium for some groups of American Indians (Vaughan et al., 1997; Wolfe and Sanjur, 1988).

Researchers at USDA's Center for Nutrition Policy and Promotion (CNPP) studied the diets of the small subsample of American Indians (including Alaska Natives) included in the 1994-96 Continuing Survey of Food Intakes by Individuals (CSFII). Although the sample was small (n=107), results indicate that American Indians' overall scores on the Healthy Eating Index (HEI) were not significantly different from the rest of the U.S. population (Basiotis et al., 1999). In addition, the prevalence of food insecurity/food insufficiency and hunger among American Indians was similar to that of other minority groups in the U.S. population (Basiotis et al., 1999).

In recent years, research has focused increasingly on traditional foods and traditional food resources (such as cultivating small home gardens, harvesting wild foods, and hunting rabbits, deer, and other game) as a means of improving the diets, food security, and/or self-sufficiency of American Indians (Lopez et al., 2002; Grant et al., 2000; deGonzague et al., 1999). Lopez and his colleagues (2002) recommended that FDPIR programs be allowed to purchase locally, with an emphasis on healthful traditional foods, up to 10 percent of the foods they distribute.

Research has provided some evidence that traditional diets may reduce metabolic risk factors for diabetes and cardiovascular disease—for example, blood levels of glucose, lipids (fats), and insulin) (Murphy et al., 1995; Gittelsohn, et al., 1998; Swinburn et al., 1991; McMurry et al., 1991). In addition, a study that followed a group of Pima Indians over a 6-year period, found that, among women, individuals who consumed an "Anglo" diet were more likely to develop diabetes than those who consumed a traditional diet or a mixed diet (Williams et al., 2001).

Specific Nutrition and Health Concerns Among American Indians

The increasing prevalence of obesity, particularly among children, is a major health concern for the entire U.S. population. However, the problem is particularly troubling in the American Indian population because of the high prevalence of other health problems for which obesity is a serious risk factor. These include, but are not limited to, diabetes, coronary heart disease, and hypertension. The particular histories and geographic and economic situations of most Indian reservations include numerous factors that encourage patterns of diet, food consumption, and inactivity that are highly conducive to adiposity and the onset of obesity (Story et al., 1998a; Vaughan et al., 1997; Campos-Outcalt et al., 1995).

Story et al. (1998a) note similarities between the observed emergence of obesity and associated health problems among American Indians and patterns that have been observed in developing countries. As a result of relatively rapid shifts to high-fat diets and sedentary lifestyles, American Indians as well as several populations and minority groups in Africa, Asia, and Latin America have begun to manifest an increased prevalence of type 2 diabetes, which is linked to obesity. Popkin (1994) describes this phenomenon as the "nutrition transition" that causes both under- and overnutrition to occur and coexist in low-income countries.

Brown and Brenton (1994) describe the rapid emergence of diabetes among American Indians since the 1940s. Burrows and her associates (2000) describe an increase of 29 percent over 7 years (from 1990 to 1997) in the prevalence of American Indians and Alaskan Natives with diagnosed diabetes. Over the same period, the increase observed for the general U.S. population was 14 percent (Burrows et al., 2000). According to the Centers for Disease Control and Prevention (1998), the age-adjusted prevalence of physician-diagnosed diabetes among American Indians and Alaskan Natives is 2.8 times greater than the prevalence among non-Hispanic Whites.

Members of the Pima tribe are reported to have the highest known diabetes rate of any population in the world. However, Campos-Outcalt et al. (1995) found the prevalence of diabetes among the Pasqua Yaqui tribe in Tucson, AZ, to be as high as that of the Pima. Lopez et al. (2002) reported similar statistics for the Tohono O'odham Nation (formerly known as the Papago

Nation). One of every two Pimas over age 35 has diabetes, compared with 1 in 25 in the overall U.S. population (Brown and Brenton, 1994). The rate of gestational diabetes among American Indians is also among the highest in the world (Brown and Brenton, 1994).

Summary

None of the literature examined for this review specifically evaluated the influence of FDPIR on nutrition and health outcomes of participants. The available literature provides largely descriptive information about the program and the individuals it serves. Anecdotal evidence suggests that the FDPIR supplies a substantial part of the dietary intake of many American Indians living on or near reservations.

Available data on food and nutrient consumption patterns of American Indians indicate that American Indians consume diets that are high in fat and limited in variety. These shortcomings are not significantly different from those observed in the population as a whole. However, the increased prevalence of nutrition-related health problems among American Indians—namely obesity, diabetes, hypertension, and related health conditions—calls for a heightened level of concern.

Recent work completed under the auspices of USDA's Economic Research Service's small grants program (Davis et al., 2002; Lopez et al., 2002; Grant et al., 2000) has contributed to a better understanding of the role of food assistance programs in the lives of American Indians. These exploratory studies should continue and researchers should begin to explore the impact of the FDPIR (and other food and nutrition assistance programs) on the nutrition and health characteristics of FDPIR participants.

A rigorous evaluation of the health- and nutrition-related impacts of the FDPIR may be difficult to implement. The penetration of the program (as well as the alternative FSP) on Indian reservations is likely to make identifying an appropriate control/comparison group difficult. Still, a better understanding of the program's impact on participants' lives is important because (1) this population is at such high nutritional risk and (2) their dependence on the FDPIR makes them uniquely vulnerable to program effects, both positive and negative. At a minimum, studies of the contribution of FDPIR foods to American Indians' diets should be updated to reflect currently available food packages.

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Chapter 14

WIC Farmers' Market Nutrition Program

The WIC Farmers' Market Nutrition Program (FMNP) provides low-income women and their children with coupons that can be used to buy fresh fruits and vegetables from authorized farmers and farmers' markets. The program primarily serves women, infants over 4 months of age, and children up to the age of 5 who are certified to receive program benefits from the Special Supplemental Program for Women, Infants, and Children (WIC) or are on a waiting list. States may choose to provide FMNP coupons to other groups of participants, most often low-income elderly or children over the age of 5. Costs for additional participants must be met by State matching funds.[159]

Very little research has been conducted on the FMNP, and the available literature offers no firm conclusions about the impact of the program on nutrition-related outcomes.

Program Overview

The FMNP is intended to encourage WIC participants to eat more fresh, unprepared, locally grown fruits and vegetables and to help small farmers by promoting farmers' markets.[160] The program began in 1989 as a pilot program in 10 States and was formally authorized in 1992, with mandated set-aside funding within the WIC program appropriation (*Nutrition Week*, 1991).

State participation occurs through an annual application process. Each year, States interested in participating in the program submit to their Food and Nutrition Service (FNS) regional office an application that outlines how the FMNP will operate in that State for the following year. Federal funds cover up to 70 percent of the cost of the program, and States are required to cover at least 30 percent.[161] The State's share of funds can come from State, local, or private sources.

States decide individually how they will determine which WIC participants will also receive FMNP coupons. Some States select coupon recipients based on the State priority system for allocating WIC benefits (for example, limiting participation to pregnant and breastfeeding women). Other States distribute coupons on a first-come, first-served basis. WIC participants can receive farmers' market coupons totaling $10 to $20 per year—in $1 or $2 denominations—at the beginning of the fruit- and vegetable-growing season (usually in June). States may limit the FMNP to specific fruits and vegetables that are grown locally.

Farmers' markets or individual farmers who wish to participate in the FMNP apply to the State agency that administers the program, or in some cases, to a subagency that handles regulatory or other issues related to farmers' markets. Criteria used to approve farmers' markets or individual farmers include membership in the local Farmers' Market Association, evidence of nondiscrimination, hours of market operation, market location, and the amount and proportion of sales involving fresh, unprocessed fruits and vegetables covered by the FMNP. Farmers can redeem coupons for cash either via the farmers' market manager or through a central processing office. In some States, the FMNP coupons are negotiable checks that the farmer can deposit in a bank. Coupons are good only for the growing season in which they are issued.

In FY 2003, the FMNP operated in 36 States, Guam, the District of Columbia, Puerto Rico, and 5 Indian Tribal Organizations. More than 13,000 farmers and 1,911 farmers' markets were authorized to participate in the program. In FY 2002, 2.1million participants redeemed FMNP coupons. In FY 2002, $15 million of the Federal appropriation for the WIC program was

[159]In January 2001, USDA's Commodity Credit Corporation (CCC) launched the Senior Farmers' Market Nutrition Program (SFMNP) as a pilot program. The SFMNP is essentially the same as the WIC version of the program but is targeted toward low-income elderly. Total costs for the program were about $13 million in its first year of operation ($2 million of the initial appropriation of $15 million were carried over to FY 2002) (USDA/FNS, 2003a). In FY 2003, the SFMNP was operating in 35 States, the District of Columbia, Puerto Rico, and 3 Indian Tribal Organizations. A total of $17 million in funding was available, including the FY 2003 appropriation ($15 million) and unspent funds from FY 2002 (approximately $2 million) (USDA, FNS, 2003a).

[160]When Congress formally approved the FMNP, USDA officials were concerned that, because farmers' markets are also accepted in the Food Stamp Program (FSP), the FMNP might represent duplicate coverage. At the time, USDA favored strengthening efforts to bring farmers' markets into the FSP rather than continuing to promote the FMNP. Some Congressional representatives, on the other hand, expressed support for the FMNP because the FMNP had the additional purpose of increasing consumer awareness of farmers' markets and thus enhancing marketing opportunities for small farmers (House Committee on Agriculture, 1992).

[161]If approved by FNS, Indian Tribal Organizations that operate the FMNP may be approved for a lower matching rate, but not less than 10 percent.

earmarked for the FMNP. This figure increased to $20 million for FY 2001 and $25 million for FY 2003 (U.S. Department of Agriculture (USDA)/FNS, 2003b).

Research Review

Very little research has been conducted on the FMNP. Three studies have attempted some form of impact analysis. All three studies focused on consumption of fresh fruits and vegetables as the principal outcome of interest. Results were mixed, and design limitations make it impossible to draw meaningful conclusions about the program's effects.

The largest evaluation of FMNP was a USDA-funded study reported in Galfond et al. (1991). This study, conducted when the program was in its demonstration phase (the 1990 growing season), included an impact evaluation based on a cross-sectional sample of WIC participants drawn from several States. A telephone survey was completed with 1,503 women who received FMNP coupons during the 1990 growing season ("recipients"), 96 women who did not receive 1990 coupons but had received them in a prior year, and 1,126 women who never received FMNP coupons ("nonrecipients"). The women were randomly selected from lists of WIC participants in four States (Iowa, Massachusetts, Pennsylvania, and Vermont) and from WIC participant lists from a few WIC clinics in Texas and Washington.

Respondents were asked to report the kinds of fruits and vegetables they had eaten the previous day and how many servings of each they had eaten. Also, for specific fruits and vegetables, respondents were asked to report how many servings they ate in a typical week. The analysis, based on bivariate comparisons, found that recipients reported eating significantly more servings of fruits and vegetables than nonrecipients. Recipients were also significantly more likely than nonrecipients to report that their fruit and vegetable consumption was greater than it had been the previous year.

The study had several limitations, which the authors duly noted. First, the sampling method may have led to bias with respect to prior access to farmers' market because clinics offering FMNP tended to be those with a farmers' market nearby. Thus, WIC participants in areas with farmers' markets could have been more likely to eat fresh fruits and vegetables than even without the FMNP than WIC participants in areas without farmers' markets. Also, the authors found that recipients differed from nonrecipients at statistically significant levels with respect to several background characteristics: Recipients were more likely to be Black or Hispanic and to be college graduates than were WIC recipients. The analysis did not control for these differences.

Participant satisfaction with the FMNP was high. At least half of the recipients indicated that they would continue to shop at farmers' markets even after they stopped receiving coupons, although the study did not include any followup to ascertain whether this actually happened. Ninety-two percent were "very" or "somewhat" satisfied with the program, and 80 percent identified some benefit they had derived from the program. A survey of participating farmers also found enthusiastic support for the program.

An interesting theoretical exploration based on Galfond et al. is provided by Just and Weninger (1997). Traditionally, Just and Weninger point out, economists would say that a WIC participant who chooses not to buy fresh fruits or vegetables does so because she perceives the benefit she will derive from those fruits and vegetables to be lower than the price being asked for them. Coupons can induce her to make the purchase because they lower the effective price to a level equal to or lower than the benefit she perceives. In this view, coupons represent a net loss to society in the amount of the difference between market price of the fruit or vegetable and the price that the consumer would be willing to pay in the absence of the coupon.

The FMNP experience, Just and Weninger argue, illustrates an economic effect of combining food program coupons with nutrition information—that is, to change the consumer's perceptions about the benefit she is likely to derive from the same fruits and vegetables. In the FMNP, the recipient receives not only coupons but also information about the nutritional benefits of the fruits and vegetables that a participant can buy with her coupons. Galfond et al. observed that coupon recipients reported greater future intentions to shop at farmers' markets than nonrecipients. Just and Weninger reason that this difference in intentions reflects a change in recipients' perceptions of the economic value of the fruits and vegetables they can get at the markets. This change in perceived value results in higher demand for the fruits and vegetables, resulting in a higher equilibrium price and inducing farmers to bring more fruits and vegetables to market.

Just and Weninger go on to estimate this economic benefit by deriving equations to describe the supply and demand of fruits and vegetables and to quantify benefits to consumers and to society. Using data reported by Galfond et al., they estimate that the net gain to society of the FMNP amounts to about 21 percent of the value of coupons redeemed. Without nutrition information (that is, as an ordinary subsidy), they estimate a net loss to society equal to 7 to 18 percent of the value of coupons redeemed.

The second study to estimate FMNP impacts was a small study reported by Anliker et al. (1992). The study, conducted in 1989, during the program's pilot phase, included interviews with randomly selected participants in nine WIC programs in Connecticut. Six of the WIC programs distributed FMNP coupons. Participants from these programs constituted the treatment group, which contained 411 respondents. Participants from the three WIC programs that did not distribute FMNP coupons were designated as the control group, which contained 78 respondents.

At the time that subjects were recruited into the study—that is, before the treatment group had a chance to use their FMNP coupons—they were asked how often they ate fruits and vegetables or drank juices. A followup survey, completed approximately 2 months later, asked the same food consumption questions plus questions about use of farmers' markets and FMNP coupons. The final analysis sample (including respondents who completed both pretest and post-test surveys) include 172 FMNP participants and 44 nonparticipants. The authors report that individuals who responded to the followup survey and those who did not differed significantly on several characteristics.

The analysis of program impacts used analysis of covariance and controlled for baseline responses on frequency of fruit, vegetable, and juice consumption. The authors found no statistically significant relationship between receiving FMNP coupons and the reported frequency of fruit and vegetable consumption. Nonetheless, the authors conclude that the "…Farmers' Market Project has been generally successful in meeting its objectives." This conclusion appears to be based principally on the finding that, "more than three-fourths of the participants who received Farmers' Market coupons went to the farmers' markets and used their coupons to purchase fresh, locally grown produce."

The third and most limited study was conducted by the National Association of Farmers' Market Nutrition Programs (NAFMNP), an advocacy group in favor of strengthening and expanding the FMNP. The study included FMNP participants' only and collected information on participants' perceptions about the program's impact on their behavior (NAFMNP, 1996; *Nutrition Week*, 1995). The NAFMNP developed a set of questions for assessing the program from the point of view of farmers and recipients, and USDA distributed these questions for States to use. States that conducted surveys provided the data to NAFMNP. The survey and sampling procedures apparently differed from State to State but are not described in the publications. Questionnaires also differed from State to State, but study organizers were able to aggregate responses for questions that were asked across States.

Results are based on data from 24 States and 2 Indian Tribal Organizations collected in 1995. Responses were obtained from 2,670 farmers (representing 33.2 percent of participating farmers) and 24,812 recipients (representing about 3 percent of FMNP coupon recipients) who participated during the 1995 growing season. The data showed that FMNP participants generally had positive impressions of the program's impact on their consumption of fresh produce: 71 percent of coupon recipients said that, because of the FMNP, they ate more fresh produce than usual during the summer. In addition, 77 percent said they planned to eat more fresh produce year-round, and 66 percent said they would continue to shop at farmers' markets even if they did not receive additional coupons.

Summary

The limited available research permits no firm conclusion about the impact of the FMNP on participants' consumption of fresh produce or on any associated nutrition-related effects. Of the two studies that used quasi-experimental designs to examine FMNP impacts, one found a positive impact on participants' consumption of fresh fruits and vegetables and the other found no significant effect. Both studies had severe methodological limitations, however—likely selection bias in the first case and possible selection bias combined with a very small sample size in the second—and both report on a very early time period in the program's history.

The small dollar value of the FMNP benefit—no more than $20 per year—suggests that any impact on nutrition and health status is likely to be so small that it would be extremely costly to measure. Research might better be directed toward effects on participants' awareness and use of farmers' markets.

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Chapter 15

Special Milk Program

The Special Milk Program (SMP) operates in schools and child care institutions that do not participate in other Federal meals programs—the National School Lunch Program (NSLP), the School Breakfast Program (SBP), or the Child and Adult Care Food Program (CACFP). Schools that do participate in these programs may also participate in the SMP to provide milk to children in prekindergarten or kindergarten programs who do not receive meals. Participating institutions provide milk to children and receive a Federal subsidy for each half pint of milk served. Children from households with incomes at or below 130 percent of the Federal poverty level may receive milk free of charge.

Research on the SMP is sparse and has generally been conducted as an adjunct to research on the NSLP and SBP. Available data indicate that the SMP contributes to increased nutrient intake, particularly among children from low-income families and elementary school children.

Program Overview

The earliest version of the SMP began in 1940. A federally subsidized program distributed milk to children at 15 elementary schools in low-income areas of Chicago. Children were charged 1 cent per half pint of milk. Those who could not pay received milk for free, with the cost paid by private donations. During the next decade, similar programs were introduced in low-income areas of other large cities. The SMP began operating nationally in 1955 when legislation provided funds to the Commodity Credit Corporation (CCC) to be used for school milk programs.[162] In 1956, eligibility for institutional participation was expanded beyond schools to include other nonprofit institutions that cared for children. The SMP came under the supervision of the U.S. Department of Agriculture (USDA), Food and Nutrition Service (FNS) when it was incorporated into the Child Nutrition Act of 1966. The SMP was permanently authorized in 1971.

From the beginning, schools were allowed to charge children for milk, but the price could not exceed the cost to the school after the Federal subsidy. In FY 1975, children eligible for free lunch under the NSLP were made automatically eligible for free milk under the SMP. This free-milk provision significantly increased the program's administrative complexity because school administrators now had to track milk served to free-milk-eligible children separately from milk served to other children.

For 25 years, the SMP was open to all schools and child care institutions, regardless of whether they participated in other food assistance and nutrition programs (FANPs). In FY 1982, the Omnibus Budget Reconciliation Act (OBRA) limited participation to schools and institutions that did not participate in other FANPs and to private schools with annual tuition of less than $1,500. These restrictions were eased in 1987 when the $1,500 tuition restriction on private school participation was lifted. In FY 1988, eligibility was reinstated for schools and institutions that participate in other FANPs. In these institutions, SMP participation is limited to half-day prekindergarten or kindergarten students who do not receive meals.

Since the late 1960s, when the SMP served around 3 billion half pints of milk per year, the size of the program has declined dramatically. At the same time, other child-focused FANPs, particularly the SBP and the CACFP, have grown substantially. In 1980, even before the OBRA changes, the SMP was providing just 1.8 billion half-pints of milk, or somewhat more than half of the amount served in the late 1960s. By 1990, this number had fallen to 181 million.

Today, relative to total Federal expenditures for food and nutrition assistance, the SMP is the third smallest FANP, overall, and the second smallest FANP to provide direct food assistance benefits (only the Senior Farmers' Market Nutrition Program and the Team Nutrition Initiative had lower total costs in FY 2002). In FY 2002, the SMP provided approximately 113 million half pints of milk at a cost of $16 million (USDA/FNS, 2003). Participating institutions were reimbursed for the net price of purchased milk for milk served to students free of charge and $0.135 per pint for milk served to paying students. In FY 2000,

[162]The CCC is a Government-owned and -operated entity that was created to stabilize, support, and protect farm income and prices (U.S. Department of Agriculture (USDA), Farm Service Agency (FSA), 2003).

the latest year for which information is available, almost 7,000 schools and residential child care institutions, 1,100 summer camps, and 500 nonresidential child care institutions participated in the SMP (USDA/FNS, 2002).

Research Review

Research on the SMP is extremely limited. Only two studies that assessed program impact were identified. Both of these studies are based on data that are more than 20 years old, reflecting a time when the program was about 15 times as large as it is today. The only other relevant research identified was a study that examined the relationship between milk consumption and lactose intolerance among SMP participants.

Of the two studies that assessed SMP impacts, the most recent and comprehensive is the National Evaluation of School Nutrition Programs (NESNP), reported by Wellisch et al. in 1983. This study took place after OBRA limited the SMP to schools that did not participate in any other federally sponsored meals program.

Data were collected on a nationally representative sample of 6,556 students (and their families) in grades 1-12 in 276 public schools in 90 school districts. Data collection included in-home interviews of parents regarding family composition, economic status, and food expenditures; in-person interviews of students in school, including a 24-hour dietary recall; and mail surveys of State, district, and school foodservice administrators, with telephone followup.

Although the study looked at a wide variety of outcomes, the SMP analysis focused solely on dietary intake. Participants in the SMP were compared with children who did not participate, using multivariate regression to estimate program effects. The researchers cautioned that results should be interpreted with care because of potential selection bias.

With that caveat, the study reported that the SMP significantly increased students' intakes of food energy, calcium, riboflavin, protein, magnesium, and vitamin B_6. Differences were more pronounced for below-median-income students and for elementary students. Among secondary students, impacts for energy were related to family income-the higher the family income, the larger the difference between participants and nonparticipants.

Wellisch and colleagues constructed an index of nutritional quality (INQ) to measure relative nutrient density.[163] They reported positive impacts on INQs for calcium and magnesium, indicating that SMP participants consumed more of these nutrients per kilocalorie than nonparticipants. Significant negative effects were reported for INQs for niacin, iron, and vitamin C. These results are largely consistent with the nutrient content of milk; milk is rich in calcium and magnesium and provides little to no niacin, iron, or vitamin C.

A study by Robinson (1975) is the only other study to address SMP impacts. This study is even older than the NESNP study, and the data are much more limited. The study used data collected in March and April 1975, after the free-milk provision had been implemented and before the SMP was restricted to schools in which no other Federally funded meal service program was operating. The objectives of the study were to assess the impact of the free-milk provision on the SMP, assess the impact of the SMP and its free-milk provision on the NSLP, determine the sources and amounts of milk and food that children consumed, determine which children used the SMP, determine when during the day children preferred to have milk available and whether schools were meeting these preferences, and determine the extent of milk waste associated with all USDA programs.

Schools were selected through a two-stage sampling process. In the first stage, approximately 4,000 schools were randomly selected from the Office of Education’s database of public and private schools. In the second stage, schools were stratified according to various configurations of participation in the SMP and the NSLP. From these strata, a subsample of 768 schools was selected for the study. Data were collected by means of a school administrator questionnaire, a foodservice supervisor questionnaire, and student questionnaires administered to randomly selected classes or students within each school. A milk waste study was also conducted in schools that participated in the NSLP, the SBP, or the SMP.

More than 20,000 student questionnaires were collected. Results indicated that students who attended schools that participated in the SMP drank more milk than students who attended schools that did not participate in any Federal meal service program. The author noted, however, that the increased milk consumption may have resulted, at least in part, from the NSLP-90 percent of SMP schools also participated in the NSLP.

[163] INQ = Recommended Dietary Allowance for nutrient/Recommended Energy Allowance, based on energy and nutrients consumed over 24 hours.

Robinson avoided drawing conclusions based on SMP-only schools on the grounds that these schools may have contained nonrepresentative groups of students (in these schools, students who were eligible for free SMP milk constituted only 3 percent of enrollment). Nevertheless, the report mentioned that the few free-milk-eligible children at SMP-only schools reported drinking 77 percent more milk at school than corresponding ineligible children. For these same children, away-from-school consumption was 7 percent less, and overall milk consumption was 12 percent more than for ineligible children.

Finally, a study by Paige and Graham (1974) looked at the incidence of lactose intolerance among SMP participants. Although dated and not specifically focused on the impact of the SMP, the findings may be of interest to researchers interested in the SMP. The authors gathered data on the amount of SMP milk consumed by 320 African-American children and 125 White children in the grades 1-5. The study was conducted in two schools in the lowest socioeconomic census tracts in Stamford, CT. Two separate measurements were taken. The analysis compared rates of milk consumption among the sampled children with race-specific rates of lactose intolerance reported in medical literature.

The authors report that 36 percent of the African-American children drank less than 50 percent of their SMP milk (a half pint) compared with 18 percent of the White children (a statistically significant difference). The children's pattern of milk rejection, along with patterns of milk rejection among adults of various races cited in other studies, was plotted against a distribution of lactose-intolerance rates among African-Americans and Whites. Patterns of milk rejection among the SMP children appeared to follow known patterns of lactose intolerance. The authors concluded that some SMP participants would benefit more from alternative sources of protein and calcium than from the milk provided by the SMP.

Summary

The available information on the nutrition-related impacts of the SMP is very limited and is of questionable relevance to today's program. The strongest and most recent study was conducted in the early 1980s, when the program was roughly 15 times as large as it is now. That study suggested that the SMP increased children's intakes of food energy and several nutrients. An earlier, though substantially weaker, study suggested that the program increased children's consumption of milk. In both cases, however, researchers put substantial and appropriate caveats around their findings.

Given the size of the SMP today, relative to other child-oriented FANPs, it is not clear that an updated study of the program's impacts should be viewed as a priority. If feasible, however, studies of the NSLP, SBP, and/or CACFP might incorporate a study on the SMP, as was done in the NESNP.

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Chapter 16

Team Nutrition Initiative and Nutrition Education and Training Program

This chapter describes two programs: the Team Nutrition (TN) Initiative and the Nutrition Education and Training (NET) Program. Both programs, which are implemented primarily in schools, differ from other food assistance and nutrition programs (FANPs) in three important ways. First, the primary focus of each program is educational in nature—to promote healthful eating patterns. Neither program provides food or enhances food purchasing power. Second, neither program considers income in targeting benefits. That is, both programs are intended to serve all children, unlike other programs that offer greater benefits to low-income children, as the National School Lunch Program (NSLP) and School Breakfast Program (SBP) do, or that are limited to children with specific nutritional risks, as the Special Supplemental Program for Women, Infants, and Children (WIC) does.[164] Finally, target audiences for both TN and NET services extend beyond children to include teachers, school foodservice workers, parents, and community members, all of whom may influence children's food choices.

After the Senior Farmers' Market Nutrition Program, which began in FY 2002, TN is the youngest FANP. It was created in 1995 as part of the comprehensive School Meals Initiative (discussed below). The NET program has been authorized for more than 25 years but has not received funding since FY 1998.

The following sections describe the TN Initiative and the NET program, in turn, and then summarize relevant research for each program. The TN Initiative is described first because it is the program that is currently active. Relatively little research has been done on either program.

Overview of the Team Nutrition Initiative

The Team Nutrition (TN) Initiative is a cornerstone of USDA's School Meals Initiative for Healthy Children (SMI). SMI was launched in 1995, with the explicit goal of improving school meals by providing schools with educational and technical resources that could be used to (1) encourage children to eat healthy meals and (2) assist foodservice staff in preparing nutritious and appealing meals.

The impetus for SMI can be traced to findings from the first *School Nutrition Dietary Assessment Study* (SNDA-I). This national study of the NSLP and SBP found that NSLP meals were higher in fat, saturated fat, and sodium and lower in carbohydrates than recommended by the *Dietary Guidelines for Americans* (DGA) and the National Research Council (Burghardt et al., 1993; U.S. Departments of Agriculture and Health and Human Services, 1990; National Research Council, 1989). At the time the SNDA-I data were collected, program regulations did not require that NSLP meals be consistent with these recommendations, and meals generally satisfied the nutrition standards that were in effect—providing one-third of students' daily requirements for food energy and key nutrients.

Since the SNDA-I study revealed that school meals were not consistent with accepted guidelines for healthful eating, USDA has been working on many fronts to enhance the nutritional quality of school meals. In 1995, USDA formalized its commitment to implementing the *Dietary Guidelines* in school meals by launching SMI. Key components of SMI include changes in the nutrition standards defined for school meals and an expansive change in the procedures used to plan and evaluate school menus (SMI nutrition standards and menu planning options are discussed in detail in chapter 5). The new nutrition standards maintain the traditional goal of satisfying some portion of students' needs for energy and key nutrients (one-third for the NSLP and one-fourth for the SBP), but also specify goals for fat and saturated fat content that are consistent with *Dietary Guidelines* recommendations.

To ensure that these changes in program policy and operations would be implemented successfully and accepted by program participants, USDA also included in SMI a comprehensive plan for providing technical assistance, educational resources, and training for school foodservice personnel as well as other stakeholders in the school meals programs—children, parents, teachers, and administrators. This plan is the TN Initiative.

[164]The Team Nutrition Initiative also provides nutrition education materials to other FANP programs, such as the WIC Program and the FSP.

TN provides support to schools via three different behavior-oriented strategies:

- Training and technical assistance for school foodservice professionals to assist them in preparing and serving meals that meet the new nutrition standards without sacrificing taste or attractiveness.
- Nutrition education for children and parents to build the motivation for adopting healthful eating habits and regular physical activity and the skills required to do so successfully.
- Involvement of school administrators and other school and community partners in building support for recommended healthful eating and physical activity patterns (USDA/FNS, 2002a).

Schools formally enroll as “Team Nutrition Schools” and make a commitment to do the following:

- Support USDA’s Team Nutrition goal and values.
- Demonstrate a commitment to help students meet the *Dietary Guidelines for Americans*.
- Designate a Team Nutrition School Leader who will establish a school team.
- Distribute Team Nutrition materials to teachers, students, and parents.
- Involve teachers, students, parents, foodservice personnel, and the community in interactive and entertaining nutrition education activities.
- Share successful strategies and programs with other schools (USDA/FNS, 2004).

However, FNS has no way of tracking the extent to which enrolled TN schools actually engage in the above activities.

TN was explicitly designed on a theoretical framework—social learning theory—that explains how people make behavior choices. As such, the focus of the program is to promote behavior change, not simply to impart information (with the hope that this might lead to behavior change). TN employs six “reinforcing communication channels to reach children where they live, learn, and play, as well as the adults who care for them and can influence their behavior” (USDA/FNS, 2002b). The communication channels are foodservice initiatives, classroom activities, schoolwide events, home activities, community programs and events, and media events and coverage. TN has established, and continues to build, relationships with national organizations that agree to work on large, visible TN projects. Examples include the American Culinary Federation Chef and Child Foundation, which sponsors cooking and taste-testing activities in local schools, conducted by professional chefs.

In FY 2002, TN was funded at $10 million (French, 2002). This level of funding has been relatively constant since FY 1996. Schools in all 50 States, the District of Columbia, and the U.S. territories have enrolled as TN schools. All State child nutrition agencies actively participate in the program and are eligible to apply for competitive grants to fund TN activities.

Research Review of the Team Nutrition Initiative

When TN was launched in 1995, plans were included for a pilot project that would test the effectiveness of the program in influencing children’s food choices and provide information on implementation issues that could be used to guide future policies and technical assistance. The evaluation of this pilot project is the only formal evaluation of TN conducted to date.

Team Nutrition Pilot Implementation Project

Seven school districts were competitively selected to serve as pilot sites for the TN Pilot Implementation Project (USDA/FNS, 1998). Schools were selected based on a demonstrated capacity to meet the requirements inherent in both TN implementation and the associated evaluation. Four of the seven districts were selected to participate in an indepth outcome evaluation; the other three districts participated in a limited process study.

The TN pilot was designed to test optimal implementation of the initiative. Participating districts were required to have designated TN coordinators. In addition, staff in each district received orientation, training, and materials that, in regular practice, a TN school would not receive. Although these practices may have given the pilot sites some advantages over a typical school, sites also dealt with many issues that put them in a less favorable situation than schools might typically encounter. These issues included a number of constraints associated with the evaluation, such as condensed implementation schedules and limited read-in time, recordkeeping and other burdens associated with data collection, and restrictions on use of local media outlets to avoid contamination of comparison schools. In addition, pilot schools were not immune to routine challenges faced by all schools, including competing for limited classroom time and resistance to change on the part of school foodservice staff. On balance, results from the evaluation probably provide a fairly realistic picture of what TN can accomplish.

Research Design

Participating school districts (communities) nominated at least two matched pairs of elementary schools. Schools were matched on size (total enrollment), proportion of students eligible for free and reduced-price meals, racial/ethnic composition, extent of existing nutrition education activities, and characteristics of the foodservice program. Across all four pilot sites, 12 pairs of schools were nominated. One of the schools in each pair was randomly assigned to the TN group and the other to a comparison group, resulting in 12 treatment schools and 12 comparison schools.[165] The comparison schools agreed not to implement any TN activities and to delay any non-TN nutrition education activities.

Districts selected three elementary grades in which to implement grade-specific nutrition education curricula (different versions for Pre-K and K, grades 1 and 2, and grades 3 and 5) in the TN school. The impact evaluation focused solely on *fourth graders* because of limited resources and the belief that children in this age range could reliably complete study surveys, including food frequency items. A total of 144 fourth-grade classrooms participated in the evaluation.

In addition to implementing the classroom curricula, pilot districts agreed to train teachers and foodservice personnel to implement menu changes that would promote compliance with the DGAs, and to conduct a number of different core TN activities, including, at a minimum:

- Two cafeteriawide events.
- Three parent-contact activities.
- Two chef activities.
- One districtwide community event.
- One districtwide media event.

Because some activities could be structured to meet more than one requirement, school districts were required to conduct at least five different activities.

The TN pilot was implemented and evaluated in two phases: spring 1996 (Phase I) and fall 1996 (Phase II). The same classrooms participated in each phase. Each phase included a pre-post design in both treatment and comparison schools. Followup data were collected the following school year for Phase I students (who were then in fifth grade) to assess long-term retention of any favorable impacts.

The evaluation assessed the impact of TN in three key areas: skill-based nutrition knowledge, nutrition-related motivation and attitude, and food consumption behaviors. Survey items and observational measures were chosen to assess changes associated with specific TN messages to eat more fruits, vegetables, and grains; to eat less fat; to eat a balanced diet; and to increase the variety of foods eaten. Knowledge, attitudes, and self-reported behaviors were measured using self-administered questionnaires. A total of 1,509 students in Phase I and 1,441 students in Phase II completed both pre- and post-tests (response rates ranged from 86 to 91 percent). Data on students' food choices and consumption behaviors were obtained through cafeteria observations carried out by trained field staff. More than 3,000 meals were assessed at each measurement point, representing response rates of 79-85 percent. The evaluation also included measure of food consumption behaviors that relied on student self-reports and parents' perceptions.

Research Findings

Because the research design included multiple data points for each subject, a repeated measures approach was used in the analysis. The direction and amount of change was assessed for treatment and comparison groups, and the net difference was attributed to the impact of TN. Regression analysis was employed, using a mixed models approach to control for clustering of the study sample. Data were generally aggregated across districts. Data from meal observations were analyzed within district and by phase, however, because of lack of comparability in menus.

Results showed that TN had small, but consistently positive and statistically significant, impacts on two of three measures of skill-based knowledge and on three different measures of nutrition-related attitudes and motivation. For skill-based knowledge, significant and positive impacts were noted for students' ability to (1) identify healthier choices and (2) apply knowledge of

[165]Because some aspects of the TN intervention specifically involve the entire school or surrounding community, randomly assigning either classrooms or students to treatment and control groups was not feasible.

the Food Guide Pyramid. Students' ability to apply a "balanced diet" concept also increased, relative to pretest scores, but differences were not statistically significant. Nutrition attitude and motivation measures included a general attitude score as well as separate scores for perceived consequences of increased consumption of fruits, vegetables, and grains, and a "cognitive rules" scale, which asked students about their willingness to make healthier food choices and their understanding about what that required. The relative size of the impacts was small (generally an increase of less than one correct answer). This did not seem to be attributable to a ceiling effect. The authors suggest that the impacts reflect the short implementation period used for the evaluation and speculate that greater effects could be achieved with a more protracted period of intervention.

Followup data showed that significant TN effects were maintained over time, although the size of the impact decreased for three of the five measures that were significant during Phase I. Estimated impacts were equivalent or greater at followup, compared with Phase I, only for the general attitudes measure and for perceived consequences of increased consumption of fruits, vegetables, and grains.

Effects on observed food selection and consumption behaviors in the cafeteria were modest. The only significant effects that were noted consistently in all districts were a slight increase in the amount of grain foods eaten and a small increase in the diversity of foods eaten (the number of different food groups included and total number of items). Changes in the selection and consumption of fruits, vegetables, and low-fat milk were in the expected direction, overall, but were not statistically significant or consistent across districts.

Analysis of three different measures of self-reported eating behaviors showed that TN had small but statistically significant effects on students' self-reported behaviors. The specific behaviors examined were use of low-fat foods, consumption of fruits and vegetables, and dietary variety (the number of food groups included in meals and snacks eaten the previous day). Three different multivariate models were used to assess TN's impact on self-reported eating behaviors: a uniform treatment model (treatment defined as a binary variable), a discrete component model (treats TN treatment as several discrete components and estimates effects for each), and a level of exposure model (treats TN treatment as a continuous variable, ranging from zero to six, based on the number of channels to which the student was exposed). Results for all three models were largely congruent and demonstrated that TN had a small but positive and statistically significant impact on students' self-reported eating behaviors (all three measures). However, none of these impacts persisted over time.

The discrete components model was not successful in identifying the most influential component(s) of the program because of change in or omission of various program components over the course of the demonstration. The level of exposure model reinforced results of previous research, indicating that the impact of TN varied depending on the number of channels to which a student was exposed.

Overview of the Nutrition Education and Training Program

The beginnings of the NET program can be traced to the 1969 White House Conference on Food, Nutrition, and Health (Maretzki, 1979). The White House conference emphasized the importance of good nutrition during childhood and the need for children, parents, and school foodservice personnel to understand the relationship between good nutrition and health. The conference stimulated interest in school-based nutrition education and the possibility of collaboration between school foodservice staff and educational staff.

The NET program was established in 1977, 8 years after the White House Conference, under P.L. 95 166, the National School Lunch Act and Child Nutrition Amendments. NET was envisioned as a means of using the school meals programs and school cafeterias as learning laboratories for helping children develop a better understanding of the principles of healthy eating. The intent of the program was to teach children the value of a nutritionally sound diet, develop nutrition education curricula and materials, and train teachers and school foodservice personnel (Maretzki, 1979).

Major goals of the program, as stated in the enabling legislation, include the following:

- The instruction of students, preschool through grade 12, in the nutritional value of foods and the relationships between food and health.
- The training of school foodservice personnel in nutrition, foodservice management, and the use of the school cafeteria as an environment for learning about food and nutrition.

- The in-service education of teachers and other school staff in nutrition education and in the use of the cafeteria as a learning laboratory.

- The identification, development, and dissemination of nutrition education resources and curricula.

The program is administered at the State level. FNS awards NET grants to States, and States appoint a State Coordinator to administer the funds. The Coordinator must assess the State's nutrition education and training needs, develop a State plan for meeting those needs, and implement the program according to the plan. NET resources may be used to develop curricula and materials, implement nutrition education programs for children, and conduct in-service training for foodservice and classroom personnel.

States have considerable autonomy in allocating NET program funds. States have used their NET funds in vastly different ways that reflect not only the results of their needs assessments, but also the status of their school-based nutrition education, training, and resources at the time NET began. Some States have spent significant resources in curriculum development, while others adopted or adapted existing materials and focused on dissemination. Still others encouraged local school districts to develop projects that suited their own needs.

States are required to submit annual reports that provide information on program dissemination, including the number of individuals who participated in NET program activities and the number of NET publications that were distributed. States must also describe their key accomplishments and outcomes. Reported outcomes are descriptive in nature, such as the number and type of workshops that were held, rather than measures of program impacts.

Program Funding

NET has had a roller-coaster funding history. Funding for FY 1978 and FY 1979 was authorized at $0.50 per child enrolled in schools and institutions participating in the NSLP—roughly $26.2 million per year. A minimum level of $75,000 was established for individual State grants.

This initial level of funding was not maintained for long (USDA/FNS, 2002c). By FY 1981, only 3 years after the program started, funding had been reduced to $15 million, a 42-percent decrease. In FY 1982, funding was further decreased to $5 million, only about 19 percent of initial funding. This level of funding was maintained through FY 1990 and was accompanied by a decrease in the number of students, educators, and school foodservice personnel served by the program (Kalina et al., 1989).

In 1989, growing public concern over children's nutritional well-being and specific concerns about the nutritional quality of school meals contributed to a resurgence of interest in NET, particularly as a means for providing training for foodservice personnel (Nelsen, 1992). P.L. 101-147 (November 10, 1989) reauthorized NET for 5 years (FY 1990-94). During this interval, NET authorization levels grew from $10 million to $25 million. In 1994, P.L. 103-448 authorized the NET program permanently, with annual funding of $10 million for FY 1996 and each year thereafter. Funding was increased to $7.5 million in FY 1991 and then to $10 million in FY 1992. Annual funding continued to be approximately $10 million through FY 1996.

In August 1996, P.L. 104-193 changed NET funding from mandatory to discretionary for FY 1999-2002. However, since 1996, NET has received funding only once, in FY 1998 ($3.75 million).[166] The curtailment of NET funding that began in FY 1997 coincided with the beginning of TN, which has been funded at about $10 million annually since FY 1996.

Research Review of the Nutrition Education and Training Program

The NET program was developed at a time when most nutrition education programs were based, expressly or implicitly, on the KABINS model: the assumption that an increase in **K**nowledge will affect **A**ttitudes, which in turn will affect **B**ehavior and ultimately **N**utritional **S**tatus (Contento et al., 1992). Today, nutrition educators realize that promoting behavior change, particularly among children, is a more complicated process. These understandings have contributed to the theory-based underpinnings of the TN Initiative, which makes ample use of social learning theory and social marketing to more directly target behavior change.

Given the underlying assumptions of the NET program model, it is not surprising that most studies of NET focused exclusively on impact on nutrition knowledge. Research has shown that change in knowledge is easily achieved, even by short-term programs (Contento et

[166]In FY 1997, NET operated with $3.75 million that was reprogrammed from TN funds.

al., 1992). In the context of this review, research that assessed knowledge gain or change in attitude, without some assessment of eating behavior, was considered insufficient. This research is well summarized elsewhere (Contento, 1992; Lytle, 1994). The following sections describe the limited available research on the effects of NET interventions that measured impacts on eating behaviors. Impacts on nutrition knowledge and/or nutrition-related attitudes assessed in this research are also described.

National Nutrition Education and Training Program Evaluation

The only national study of NET was completed during the very early stages of the program, between 1979 and 1980 (St. Pierre and Rezmovic, 1982). At that point, it was plausible to expect program impacts in only a few States that had been able to begin implementation almost immediately after funds became available. Moreover, because of the diversity of States' goals, only State-specific impact evaluations were deemed appropriate.

Consequently, impact assessment in the National Nutrition Education and Training Program Evaluation focused on program activities and outcomes in two States: Georgia and Nebraska (St. Pierre and Rezmovic, 1982). (The study also collected descriptive data at the national level, including an analysis of State plans.) The programs in Georgia and Nebraska were firmly established and were widely respected by NET staff at the regional and national levels.

The evaluation of the Nebraska NET program focused on assessing how well the program was implemented, as well as its impact on children's nutrition-related knowledge, attitudes, preferences, and eating habits. Nebraska offered a statewide curriculum that was experience-oriented in the primary grades, but knowledge-oriented in grades 4 through 6. Twenty schools were selected from 98 volunteers and were randomly assigned to treatment and control groups. A pre- and post-test design was used, with the pretest conducted immediately after the 10-week treatment concluded.[167] No followup measures were collected.

At pretest, treatment and control groups were equivalent on all outcome measures (St. Pierre et al., 1981). At post-test, NET participants in all grades had statistically superior gains in nutrition knowledge. In addition, NET participants in grades 1 through 3 were also found to be more willing than nonparticipants to try new foods in the school cafeteria and more likely to have made improvements in food preferences (based on self-report). NET participants in grades 4 through 6 were more willing than nonparticipants to try previously rejected foods. Results showed no consistent effects on nutrition-related attitudes, self-reported eating behaviors, or plate waste.

In Georgia, the State chose to follow a decentralized model, in which schools were free to use different nutrition education curricula and materials. The evaluation included 1,400 students in grades 1 through 8. Results showed that NET had strong positive effects on nutrition knowledge but limited effects on attitudes and self-reported eating behaviors (St. Pierre and Glotzer, 1981).

The authors appropriately point out several factors that limit the generalizability of their results. A major limitation is the quasi-experimental design, with the incumbent potential of nonequivalent NET and non-NET groups (on factors other than pretest measures). Another major limitation is the duration and content of the intervention. The authors questioned whether it was appropriate to expect changes in behavior from an intervention that was limited in time and essentially focused on knowledge dissemination (St. Pierre, 1982). Other factors that complicate interpretation of the study's findings are the exposure of some non-NET students to other (non-NET) nutrition education activities during the treatment period and the significant nutrition education that had been conducted in many classrooms before the NET intervention.

Other Studies of NET's Impact

The literature search identified three small, local studies that examined the impact of NET interventions on children's nutrition-related knowledge, attitudes, and/or eating behaviors.[168] One of the earliest studies examined the NET program in Tennessee. Tennessee's first NET Plan included a detailed evaluation plan (Banta and Cunningham, 1982). Assessment instruments were developed for students, parents, teachers, school administrators, and foodservice personnel. All instruments included self-reported measures of nutrition behaviors. A plate waste study was devised to measure student-level changes in food consumption.

[167] Nebraska's *Experience Nutrition* curriculum had 11 segments, designed to be used sequentially for grades K through 6 with an expected cumulative impact on children's behavior. However, the evaluation was only able to test for an immediate effect of the particular segment used in each classroom (St. Pierre et al., 1981).

[168] The literature search revealed several published studies that evaluated the impact of NET on the knowledge and attitudes of teachers and/or foodservice personnel. These results are summarized elsewhere (Olson, 1994).

Baseline data were collected in 36 elementary schools—2 treatment schools and 2 control schools in each of 9 so-called development districts. No details are available in the published literature on the school selection process or on the characteristics or comparability of treatment and control groups (Banta and Cunningham, 1982; Banta et al., 1984).

The first post-test was conducted after less than 1 year of NET interventions. At this point, fourth and sixth graders in treatment schools were more likely than those in control schools to report positive eating behaviors. In addition, first graders in treatment schools were more likely than those in control schools to report having eaten the school lunch. However, plate waste studies found no significant differences in food consumption treatment and control students at any grade level. At the last followup (year 3 of the study), significant differences were noted between NET and non-NET students for knowledge gain (at all grade levels) and for attitude scores (at four grade levels). Again, however, no significant differences were detected in self-reported eating behaviors or in plate waste (Banta et al., 1984).

Also in the early 1980s, Gillespie (1984) studied three NET interventions in New York State. Three schools that had received mini-grants from the New York State NET program (and used them for very different activities) were matched with control schools based on size (total enrollment), community socioeconomic indicators, staff interest in nutrition education, and type of food service.

The study included pre- and post-test assessments of students' nutrition knowledge and attitudes. In addition, after the intervention was over, parents were asked whether they observed changes in the foods their children ate, their children's interest in eating nutritious foods, or their children's understanding of nutrition. Similarly, teachers were asked whether they noticed changes in children's food choices or their attitudes toward nutrition.

After controlling for differences in baseline scores, Gillespie found no significant improvement in nutrition knowledge or attitudes among NET students. Both groups of students showed significant gains in knowledge and attitude measures at post-test, and the difference in relative size of the gains made by each group was not significantly different.

With regard to parental reports of children's eating behaviors, Gillespie found that parents in NET schools that had the most intensive intervention were more likely than their non-NET counterparts to report an improvement in the quality of foods chosen for snacks eaten away from home. Effects on reported quality of at-home snacks were inconsistent. No effects were detected for any of the other parent-reported measures or for the teacher-reported measures.

Shannon and Chen (1988) conducted a 3-year study of the NET program in Pennsylvania. The authors assessed the knowledge, attitudes, and self reported behaviors of children as they progressed through grades 3, 4, and 5. Districts that responded to an invitation to join the study were grouped by geographic region and ranked according to nutritional need and community socioeconomic status. The 12 neediest districts were offered participation in the study, but two declined.

Schools in the remaining 10 districts were then randomly assigned to treatment and control groups. Initial assignments were adjusted because of administrative contingencies. For example, one principal supervised three of the small schools and wanted all of them to be in the same group. The resulting baseline sample included 17 treatment schools with 879 students in 39 third-grade classrooms and 18 control schools with 828 students in 36 classrooms. Students in the treatment group received 9-12 weeks of nutrition education each year based on the *Nutrition in a Changing World* curriculum.

In the end, the treatment group had significantly greater knowledge gains than the control group, as well as significantly greater improvements in some attitude measures. However, no significant impacts were detected for eating behaviors. Eating behaviors for both treatment and control groups significantly improved over time. The authors concluded that "it is difficult to demonstrate that increased nutrition knowledge dramatically affects nutrition attitudes and eating behaviors."

Summary

Since it was established in 1977, the NET program has provided fluctuating support for nutrition education in school classrooms and cafeterias. Programs have been State-defined and have varied considerably across States. Most programs have aimed at improving children's knowledge and attitudes as a means of ultimately influencing their behavior because of assumptions that improved knowledge and/or attitudes will lead to behavior change, because of resource constraints, or because of questions about whether behavioral outcomes constitute an appropriate goal for school-based nutrition education.

Several studies provide compelling evidence that NET nutrition education activities can improve, at least for the short-term, children's nutrition knowledge and attitudes, but there is limited evidence that NET programs affect children's eating habits.

This finding holds true for most school-based nutrition education programs, including programs not sponsored by NET and not based on the KABINS model (Contento et al., 1992). In a comprehensive review of research on school-based nutrition education implemented in the 1980s and early 1990s (most of which was not specifically sponsored by NET), Contento and her colleagues found that, on average, these interventions provided 10-15 hours of instruction over a period of 3-15 weeks. The programs that were most successful, however, tended to include longer (more intensive) interventions. The year-long *Know Your Body* curriculum, for example, has been found to produce not only behavioral changes, but also measurable physiological improvements. The *Food ...Your Choice* curriculum, which includes activities for all grades that can be included in subjects already being taught, has induced elementary students to eat significantly more fruits, vegetables, protein foods, and vitamin-A-containing foods.

The relationship between extended intervention periods and behavioral change is supported by the results of the School Health Education Evaluation (Connell et al., 1985). This nationally recognized study found that 30 hours of classroom instruction were required to achieve "medium" effects for general health practices, 40 hours were required for changes in attitudes, and 50 classroom hours were required to achieve stable levels in knowledge, attitudes, and behavior across a variety of health areas.

In addition, research has shown that teacher training increases teachers' interest in teaching nutrition, as well as the time they devote to it. Adding parent participation to classroom instruction was found to enhance program impact, particularly in the earlier grades, and particularly if parents and children worked together. Contento and her colleagues (1992) conclude that, in most evaluations of nutrition education programs, "the effectiveness of nutrition education was not given a fair test."

The TN Initiative is well-conceived in building on the NET experience and in incorporating a multi-pronged, theory-driven focus of behavioral change. Results of the pilot implementation project, though preliminary and certainly not generalizable, are promising.

Future research should examine the impact of the TN Initiative in a larger number of schools where the program is firmly established. Examination of program impacts on nutrition-related behaviors should move beyond the self-administered questionnaires and cafeteria observations employed in the evaluation of the TN pilot project to include more sophisticated dietary assessment techniques that will provide information on food and nutrient intake both in and out of school. Given the multi-modal nature of the TN Initiative and the likelihood that students will receive varying "doses" of the program's intervention components, a process study that clearly documents how the program is implemented and, to the extent feasible, the amount of exposure to the program for each child, is also very important.

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