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IS THERE AN INTRAHOUSEHOLD FLYPAPER EFFECT ? EVIDENCE FROM A SCHOOL FEEDING PROGRAM

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ABSTRACT

Are public transfers targeted toward children largely neutralized by the household, as the theory of altruism implies, or is there an intrahousehold “flypaper effect” whereby such transfers “stick” to the child? This paper studies the impact of a school feeding program on child caloric intake in the Philippines. Because children are interviewed on school days and nonschool days, and because some schools offer a feeding program and others do not, the dietary impact of the program is identified under mild restrictions. The empirical results confirm an intrahousehold flypaper effect; indeed, they indicate virtually no intrahousehold reallocation of calories in response to the feeding program. In poorer households, however, children’s gains from the program appear to be “taxed” more heavily.

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1. INTRODUCTION

Do government transfers to a specific household member, say a child, "stick" to that individual? Or, as the theory of altruism (Becker, 1974, 1981) implies, are such policies largely neutralized by reallocations of resources away from the child toward other members of the household? The existence of an intrahousehold "flypaper effect" (IFE), to borrow a term from the public finance literature,[1] would seem to be an essential justification for much of public policy targeted toward children. Becker (1981), for one, attributes the failure of compensatory education programs for minority children to the absence of a strong IFE. Yet, despite its importance, there is remarkably little direct evidence on the magnitude of the IFE. In this paper, I provide such evidence by studying the impact of a school feeding program on child caloric intake in the Philippines.

To state the null hypothesis of a zero IFE more precisely: if parental altruism is "operative," then an infra-marginal government transfer to a child, whether in cash or in kind, should not affect child consumption, holding total household resources constant.[2]

Supplementary child feeding programs in general, and the program that I analyze in particular, can provide powerful evidence on the IFE. Transfers between parents and their young children generally flow in one direction, and, since parents usually provide much

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[1]The "flypaper effect" refers to the empirical phenomenon that government grants to localities increase local spending by more than an equivalent increase in local income (see, Hines and Thaler, 1995, for a review).

[2]Infra-marginal means that the transfer does not exceed what the child already receives from his parents. Otherwise, the transfer would lead to a corner solution, in the case of one-sided altruism, or a switch in transfer regimes from child to parent in the case of two-sided altruism.

more food to their children on a given day than would a typical supplementary feeding program, these programs are largely inframarginal. Moreover, at least at this early stage in their relationship, strategic or bargaining considerations do not play a significant role in resource allocation between parent and child.[3] Nor are asymmetric information and other transaction costs likely to be important, as they might be between members of extended families who do not live together (see Altonji, Hayashi, and Kotlikoff 1992). In sum, it is hard to imagine a more congenial setting for the altruistic model.

Looking at supplementary feeding programs also obviates some thorny measurement issues. Gauging the impact of cash transfers calls for a broad measure of *individual* consumption, which is hard to capture in household survey data.[4] Since programs targeted to children invariably provide in-kind transfers (e.g., health-care, education, meals), their impact can be assessed by focusing on the consumption of a single good, which one has a better hope of measuring. Of course, measuring the consumption

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[3]An example of such considerations is given by Cox and Jakubson (1995), who argue that an increase in public transfers to the child generation could result in *increased* private transfers from their parents, since parents might have to bribe their children more to take care of them.

[4]This problem is not as severe for non-coresident extended families, in which case one can, in principle, examine the total expenditures of the "child" household. Still, choice of coresidence is endogenous and may be influenced by government transfer policy (see Rosenzweig and Wolpin, 1994). Since their data sets do not have information on household expenditures, Cox and Jakubson (1995) and Rosenzweig and Wolpin (1994) study crowd-out effects of public transfer programs using data on financial transfers from parents to adult children. A problem with looking at parental financial transfers is that in-kind transfers may also be important (coresidence being one example). Moreover, child income need not equal current consumption, and, while altruism implies that public transfers should not affect the time-path of consumption, the theory has nothing to say about the time-path of parental financial transfers. These difficulties motivate Altonji, Hayashi, and Kotlikoff (1992) to test for altruism using data on consumption, albeit food consumption, rather than financial transfers. Although their study does not explicitly test the hypothesis of zero IFE, their rejection of altruism would suggest a nonzero IFE within non-coresident extended families.

of some goods, particularly those that are home produced such as health and education, can present severe conceptual difficulties. Measuring an individual's daily caloric intake, on the other hand, is relatively straightforward.

A particular advantage of my data is that they provide an exogenous source of variation in program participation. Comparing the average caloric intake of children who participated in school feeding programs with that of nonparticipants may yield a biased estimate of the dietary impact of the program; hungrier children, for example, could be more likely to take up the program when it is offered to them. My sample includes children going to schools offering a program and those in schools without a program. In each case, some of the children were, arbitrarily, interviewed on school days and others on nonschool days. Comparing the average caloric intake of children interviewed on school days with that of children interviewed on nonschool days *in schools providing a program* identifies the average impact of the program *plus* the effect of attending school (which may require more energy expenditure than staying at home). The same comparison for the children in schools that do not offer a program isolates the effect of attending school. The difference between these estimates is the impact of the program.

Besides identifying the dietary impact of school feeding programs under only mild restrictions, there is another advantage to my empirical strategy. Testing the zero IFE hypothesis would normally require estimating two consumption effects, that of an increase in the public transfer and that of an equivalent (exogenous) increase in household income. However, as I will show, comparing the caloric intake of children within a given school, all of whom have access to the same feeding program yet are sampled on different days, already "controls" for the income effect of the program. Therefore, it is not necessary to find an exogenous source of variation in household income.

The next section of the paper explicates the conceptual framework underlying my test of the zero IFE hypothesis. In section 3, I review previous studies of the dietary impact of supplementary feeding programs, both in the U.S. and in developing countries, showing how these fail to provide reliable evidence on the IFE. In section 4, I describe the data and the school feeding programs as well as my estimation strategy. Section 5 contains my empirical analysis, with the conclusions and implications in section 6.

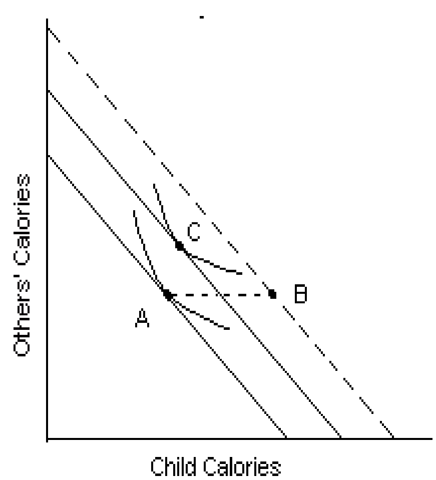
2. A FRAMEWORK FOR TESTING THE ZERO IFE HYPOTHESIS

THE CONCEPTUAL EXPERIMENT

Consider a school feeding program offering every child a free meal every school day. A random sample of children is chosen from a school and interviewed about their diet the previous day. The day of interview is arbitrary, so some children will recall their diet for a school day and others for a nonschool day (a weekend or school break). Because all the children have access to the same program over the course of the school year, the fact that some received a school meal the previous day does not make them "wealthier" than the children who did not receive the meal the previous day. Thus, a comparison of the average caloric intake of children on school days with that of children on nonschool days provides an estimate of the IFE, ignoring, for the time being, any other differences in caloric intake between school days and nonschool days.

Figure 1 illustrates this conceptual experiment in the two-good case, though the argument carries over to general preferences. With the price of child calories relative to other household members' calories assumed to be one, the innermost budget line shows household resources devoted to calories in the absence of a feeding program. On the days that the program is offered (school days), the intrahousehold allocation of calories would be at point B if parents fail to adjust their behavior in any way. Under this "inertia" hypothesis, as the child's school opens and the feeding program becomes available, the intrahousehold allocation of calories moves from A to B.

Figure 1 Intrahousehold allocation of calories: Zero IFE hypothesis



Now consider what happens when altruistic parents adjust allocations in response to the program. Since the program is inframarginal with respect to the child's total daily calorie intake, it should not affect the allocation of calories across days. The outermost (dashed) budget line in Figure 1 represents household resources if the child was offered the program *every day*. Suppose, for the sake of exposition, that school is in session for half the year. So, the budget line midway between the innermost and outermost takes into account the income gain from the program "smoothed" over the entire year. Under the null hypothesis of a zero IFE, the household optimum is at point C on school days as well as on nonschool days.[5]

Notice that the test just outlined has no power against the alternative hypothesis that spouses bargain with each other over allocations to their children. Put another way, whether or not point C is the outcome of parents maximizing a joint utility function or of a bargaining process between spouses with conflicting preferences over child allocations, that outcome should not be affected by the provision of a free school meal. While it is true that the *existence* of the school feeding program might alter spousal threat points due to an income effect and thereby affect intrahousehold allocations under certain assumptions (see, Lundberg and Pollack, 1993), the conceptual experiment in Figure 1 controls for the income effect of the program.

[5]The same analysis applies if the feeding program provides a subsidized, rather than a free, meal, as long as there is a purchase limit. In particular, children cannot be allowed to purchase more calories than they would have received for the day in the absence of the program (i.e., the subsidy must be inframarginal).

IMPERFECT INTERTEMPORAL SUBSTITUTION: A CAVEAT

Implicit in the discussion thus far has been the assumption that the in-kind transfer is a perfect substitute for private consumption. In the case of a feeding program, this assumption might seem plausible, since one calorie could be viewed as just as good as any another. There is, however, an intertemporal dimension; at the margin, one calorie now is not necessarily as good as one calorie later in the day.[6]

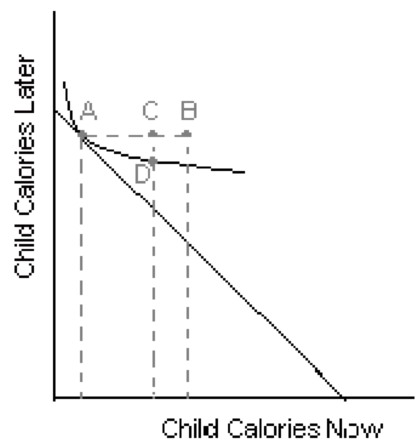
To understand the implications of imperfect intertemporal substitutability (IIS) of calories, turn to Figure 2 which illustrates the intertemporal choice. Altruistic parents are assumed to care about the overall utility their child derives from food each day. On a day with no feeding program, the child is at point A. On a day with the program and in the absence of any adjustments by parents, the child would move to point B. Parents can adjust by eliminating the calories that they would have provided for that meal, moving the child to C. But, in this example, the in-kind transfer is *not* inframarginal with respect to *current* calories; the feeding program provides more calories now than would have been supplied by parents. Thus, parents can only adjust further by reducing calories provided in a meal later (or earlier) that day.[7] However, at the margin, a calorie later is not worth the

[6]There is a possible taste or quality dimension as well, so that a publicly provided calorie may yield different utility at the margin than a privately provided calorie. I return to this issue in the concluding section.

[7]Note that the in-kind transfer, in this example, is still inframarginal with respect to total child calories that day. Hence, the program should not affect the allocation of child calories *across* days even if the interday substitutability of calories is imperfect. Put another way, since calories are presumably less substitutable across days than within a given day, adjusting child calories the next day would be a less efficient method of "taxing" the child's gain from the feeding program than adjusting calories in a later meal on the same day.

same to the child as a calorie now. So, any reductions in later calories beyond point D would make the child worse off with the program. Since altruistic parents would not make the child worse off, total daily calories (calories now + calories later) must increase on the days the program is offered. The extent of this increase depends on the difference between the calories provided in the feeding program and the calories provided by parents, for the same meal, in the absence of the program, as well as on the degree of IIS.

Figure 2   Intertemporal allocation of calories



The upshot of this discussion is that a finding that total calories are significantly higher on the days of the feeding program does not necessarily reject a zero IFE; it may only reject the maintained hypothesis of perfect intertemporal substitutability of calories. This is problematic because the policy implications are very different under the two alternatives. While the existence of an IFE suggests that targeted transfers can improve child welfare compared to untargeted transfers, this is not the case under the IIS hypothesis (the child consumes more total calories in Figure 2 but has the same utility level with and without the program).

There is one way to distinguish between the two hypotheses empirically. As is evident from Figure 2, the magnitude of the increase in current calories due to the program (segment AC) is greater than the increase in total calories (AC-CD), barring, that is, the implausible case of perfectly imperfect intertemporal substitution in calories (CD=0). Essentially, under the IIS hypothesis, there should be a reduction in calories consumed later in the day in response to the program.

3. A REVIEW OF THE EVIDENCE ON THE IFE FROM SUPPLEMENTARY FEEDING PROGRAMS

I now turn to a review of the previous literature on the dietary impacts of supplementary feeding programs in both the U.S. and in developing countries. A careful review of this literature, in light of the previous discussion, is important because supplementary feeding programs are widely viewed as being informative about the nature of parental altruism (for example, Becker, 1981, cites such a program in this context). Yet, as I will show, existing studies, at best, provide meager evidence on the IFE.

U.S. SCHOOL FEEDING PROGRAMS

There are three published studies evaluating the nutritional impact of the National School Lunch Program (NSLP) and/or the School Breakfast Program (SBP). Two of the studies (Akin, Guilkey, and Popkin, 1983; Devaney and Fraker, 1989) examine the total daily caloric intake of children, while the third study (Long, 1991) considers the impact of the two feeding programs on weekly household food expenditure. Since total spending on food does not measure individual allocations, the latter study is not directly relevant to the IFE.

Neither of the caloric intake studies have much to say about the IFE either. First, neither study measures the caloric content of the meals provided at school, so one can only speculate on the degree of substitution of government meals for private meals.

Second, Akin, Guilkey, and Popkin do not deal with the endogeneity of individual participation in the NSLP, although Devaney and Fraker are attentive to this issue, at least in regards to the SBP. Third, neither study controls for the endogeneity of the decision by schools to adopt the program, which is more serious for the SBP given that it only covers about a third of U.S. schools (Long, 1991). Schools serving parents who are more concerned about their children's nutrition may be more likely to adopt a school feeding program. Fourth, children who did not participate in the feeding program on the reference day may or may not have been eligible.[8] Comparing the caloric intake of eligibles (point C in figure 1) and noneligibles (point A) confounds the IFE and the income effect of the program, though for many U.S. families such income effects may be negligible.

Notwithstanding these criticisms, it is worth considering the findings of the two dietary studies, which are somewhat contradictory (albeit differing in data sets and empirical specifications). For younger children, Devaney and Fraker find no dietary impact of either the NSLP or the SBP, but find that the Special Milk Program (with participation treated as exogenous) does significantly raise total daily caloric intake of the child. Akin, Guilkey, and Popkin obtain a statistically significant effect of the NSLP on intake, ranging between 126 and 240 calories. If we are willing to assume that NSLP participation is exogenous and that income effects are negligible, we might conclude from the modest dietary effects in the latter study that, while some substitution for privately

[8]According to Devaney and Fraker: "The nutrient intake equations...are estimated on the subsample of all students who eat any breakfast, whether or not they have access to the SBP" (p. 936).

provided food is taking place, altruism does not entirely neutralize the NSLP. However, overall, it is hard to draw any definitive conclusions about the IFE from the U.S. school feeding literature.

PRESCHOOL SUPPLEMENTARY FEEDING PROGRAMS IN DEVELOPING COUNTRIES

Although the literature on supplementary feeding programs in developing countries is replete with concern over the extent to which the food provided supplants children's normal diet, there is a surprising dearth of quantitative evidence on the phenomenon. In a much cited article, Beaton and Ghassemi (1982) review about 200 studies of preschool feeding programs, yet they find only eight evaluations that provide data on food substitution.[9] Five of the eight are "take-home" rather than "on-site" programs. In take-home programs, the mother goes to a feeding center to pick up food *intended* for her preschool child to prepare at home. Since take-home programs are not effectively targeted toward children, they cannot provide reliable evidence on the IFE.

[9] A more up-to-date review by Figa-Talamanca (1985), which also covers school feeding program evaluations in developing countries, does not augment this total. However, in a recent experimental study of a school breakfast program in Peru (Jacoby, Cueto, and Pollitt, 1996), children in 10 randomly selected schools were given a 600 calorie morning snack. Calorie intake data were collected for equal subsamples of children in the treatment and control schools, 116 children in all. The average difference in total daily intake between the two groups was 451 kcal. (s.e.=113). While this comparison does not control for the income effect of the program, it is strongly suggestive of a substantial IFE (pre-intervention intakes were the same across groups, but it is not clear from the study whether the intervention was announced prior to this measurement and thus whether the baseline diet captures the income effect). More specifically, though the supplement appears to have completely displaced the usual (small) morning snack for the treatment group, it had no significant effect on calories later in the day. One caveat in interpreting these findings, however, is that since the calorie survey was conducted just two weeks after the introduction of the intervention, parents may not have been habituated to the program yet.

That leaves the three on-site programs, in which children are taken by their mothers to feeding centers where they are served prepared foods. For each of these programs, information is available on the previous day's caloric intake of the children who show up at the feeding center on the survey date. Dietary data is also available for a "control" group of children. For one program (India Poshak), this control group consists of children who do not participate in the program but are thought to have similar characteristics as the participants (Beaton and Ghassemi, p. 870). Comparing the average caloric intake of this control group with that of the "treatment" group corresponds to a comparison of points A and C in figure 1, and thus confounds the IFE and the income effect. In a poor country, the income effect of a feeding program is unlikely to be negligible.

In the two other on-site programs evaluations (both in Anderson et al. 1981), the control group consists of children who came to the feeding center on the survey date, but who did not show up on the previous day (the reference day for the diet recall survey). In this case, a comparison of average intakes between treatment and control groups does correspond to the experiment in Figure 1. However, the evaluation methodology in Anderson et al. (1981) treats the decision to attend the feeding center on a given day as exogenous; in other words, the treatment and control groups are implicitly assumed to be identical along unobserved dimensions that may affect caloric intake (not to mention observed dimensions, which are not controlled for either).[10]

Despite the potential for bias, I summarize the estimates of the IFE from the two programs in Table 1. In general, the IFE is significantly different from zero. The exception is for older preschoolers in the Tamil Nadu program, where virtually the entire food supplement is “taxed away” by the household. By contrast, the estimate of the IFE for infants in the same program is not significantly different from one, meaning that we cannot reject the hypothesis that none of their extra calories are taxed away. Anderson et al. (1981) speculate that “the older child consumes more of the adult diet

[10]On the bright side, Anderson, et al. (1981) report that attendance at the feeding centers was not significantly related to the incidence of diarrhea, so that the difference in caloric intake between treatment and control groups does not appear to be due to greater illness among the latter group. However, there are large and unexplained inconsistencies between feeding center attendance rates reported in the study and the samples reported in Table 1.

Table 1 Substitution in two supplementary feeding programs

	(1) Mean caloric intake of supplemented group	(2) Mean caloric intake of nonsupplemented group	(3) (1)-(2)	(4) Calories in supplement	(5) (3)/(4)
Costa Rica					
Overall	1,495 [61]	1,033 [105]	462	737	0.63 (0.08)
1-3 year olds	1,325 [24]	917 [36]	408	737	0.55 (0.11)
3-5 year olds	1,403 [65][a]	1,063 [68]	340	737	0.46 (0.09)
Tamil Nadu					
Overall	972 [193]	811 [60]	161	340	0.47 (0.13)
1-3 year olds	937 [79]	667 [26]	270	340	0.79 (0.18)
3-5 year olds	990 [110]	935 [33]	55	340	0.16 (0.18)

Source: Anderson et al. 1981.

Notes: Sample sizes in square brackets and standard error of estimates in parentheses, the latter calculated using sample sizes and standard deviations provided in source. The nonsupplemented group consists of children who did not visit the feeding center the previous day.

[a] Includes 28 children not in overall figures, who ate lunch only rather than both breakfast and lunch.

for which the [supplement] is substituted" (p. 159). However, this age differentiated pattern does not appear in the Costa Rica program. Given their findings, Anderson, et al. suggest that, to mitigate substitution, feeding programs ought not to serve children at regular meal times.

In sum, though the dietary impacts of child feeding programs have been analyzed in a variety of contexts, I would argue that no study to date identifies the IFE under a plausible set of assumptions. Nonetheless, taken at face value, the evidence on the IFE that does exist poses a challenge to the altruism model. It would appear as though families may not always neutralize the impact of supplemental feeding programs.

4. DATA AND METHODS

THE CEBU LONGITUDINAL HEALTH AND NUTRITION SURVEY

The data for this study come from the 1994-95 follow-up to the Cebu Longitudinal Health and Nutrition Survey (CLHNS), carried out in the Metropolitan Cebu area on the island of Cebu, Philippines. Metro Cebu includes Cebu City, the second largest city in the Philippines , and surrounding urban and rural communities. The initial sample consists of 3,384 children ranging in age from six to twelve, 3,220 of whom were enrolled in a total of 190 schools. About two-thirds of the children have been followed since birth over the twelve year course of the CLHNS and the rest are their oldest younger siblings. Since the original sample was drawn at random from all pregnancies in the Metro Cebu area over a one year period (1983-84), the sample used here is reasonably representative of that population.[11] Overall, while the population is not desperately poor, nutritional status is quite low; almost half the children in the sample are stunted and more than a third are underweight (i.e., their height-for-age or weight-for-age, respectively, is at least two standard deviations below the mean for a healthy population).

The 1994-95 follow-up survey collects detailed information on school attendance, school feeding programs and related topics from school administrators, mothers and the children themselves. In addition, a dietary recall survey was administered to each child in the sample, referring to all food consumed the previous day.[12] Children were asked not only about the quantities of each food consumed, but also about where each dish was prepared, including whether it was part of a school feeding program. The level of detail is sufficiently fine that one can ascertain the caloric content of a food item provided by a feeding program even if the school meal was supplemented by food purchased from a private shop or brought from home.

Since 31 of the 190 schools contribute only a single child to the sample, I drop them from the within-school analysis, leaving a sample of 3,189 children in 159 schools. These

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[11]Sample attrition over the 12 years of the survey has been around 29 percent, mainly due to permanent outmigration from the Metro Cebu area.

[12]There is a large nutrition literature on the accuracy of caloric intake data from 24-hour recall surveys, though few studies specifically assess the responses of children. Nelson, et al. (1989) estimate from multi-day diet records that the responses of British adults are about twice as accurate as those of children (ages 5-17), in the sense that it would take about twice as many days of data collection from children to obtain a correlation of at least 0.9 between the observed and true caloric intake. Of course, to affect my results, inaccuracies must vary systematically by program participation status, which is unlikely (but see below).

children belong to 2,090 households, a feature of the data that I do not exploit,[13] though intrahousehold correlation does necessitate standard error corrections.[14]

Interviews were spread fairly evenly across days over the course of a one-year period, so that about half of the sample (1,542 children) turned out to have attended school on the day preceding the interview date. Three-quarters of the nonattenders were interviewed during the school break or on a Sunday or Monday, meaning that the reference day for the dietary recall survey was not a school day. Some of the remaining nonattendance resulted from school closings due to national and local holidays and other activities, the exact dates of which vary by school and are not recorded in the data. The rest of the nonattendance is due to absenteeism, though this appears to be low in the sample—roughly one out of twenty school days on average are missed.[15] An empirical question that I address below is whether absenteeism is systematically related to access to or take-up of the school feeding program.

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[13]In principle, sibling data would allow estimation of cross-sibling effects of participation in the school feeding program, which should also be zero under the null hypothesis. However, identifying such effects requires having enough sibling pairs that were interviewed on the same day and in which one sibling attends school and the other does not, for exogenous reasons. Given that most sibling pairs attend the same school in my sample, this is an extremely unlikely occurrence.

[14]Robust standard errors reported in Table 2 below are only approximate because sampling within households was not random.

[15]There are two sources of information on absenteeism: the child schooling questionnaire administered to mothers and the attendance records collected directly from schools for almost every child in the sample. Mothers were asked how many days in the past month the child missed school when it was in session, and their average response was 1.19 days. Restricting attention to households interviewed in a month other than April, May or June, so that the one month reference period did not encompass the school break, only raises the average days missed to 1.35. School attendance records cover one full semester for most children, and two semesters for some, and show that around three percent of school days are missed in the sample.

THE SCHOOL FEEDING PROGRAMS

About 15 percent of those who attended school on the reference day participated in some kind of school feeding program (226 children). The main feeding program in Cebu is sponsored by the relief agency CARE. The diet recall survey, which categorizes seven possible meals (breakfast, lunch, dinner, and four snacks), indicates that all food from school feeding programs is consumed as part of a morning or afternoon snack (sometimes both). On the reference day, most participants reported receiving a single morning snack (165 children), and most of the rest (52) a single afternoon snack. All but 13 of the 226 participants ate the CARE snack, a sweetened porridge made of bulgur wheat, with the rest consuming assorted other foods (chiefly rice, vegetables, or milk) from "indigenous" feeding programs.[16] These snacks are far from being nutritionally inconsequential for most of the participants, as Figure 3 shows. On average, the feeding programs provide 303 kcals., or about 20 percent of daily calories, for the participants.

Program characteristics vary across schools, according to school administrator reports. Some schools operate multiple programs, some charge a nominal fee for the food,[17] some provide the program fewer than five days a week, some provide the program

[16]Other information gathered from children and school administrators confirms the predominance of the CARE program. All but seven of the 226 participants report that they *usually* obtain a bulgur snack from the CARE feeding program. According to information from the administrators' reports merged with the child data, 212 of the participating children are covered by a CARE program.

[17]Fees are charged to defray the cost of preparing the food. Almost all the schools charge for the indigenous programs, but fewer than half of the children covered by the CARE program are charged for it. Fees never exceed half a peso, which is a very small amount even by Filipino standards. Moreover, the portions received are unrelated to fees; in the case of CARE bulgur, 96 percent of the time children reported consuming no more than the standard portion of one cup.

only to children in lower grades, and some screen recipients according to the severity of malnutrition. This variability across schools, in part, dictates a within school analysis of dietary impacts.

Before turning to this analysis, however, it is worth looking at the raw data. Figures 4 and 5 plot the distributions of calories from snacks (morning + afternoon) and total daily calories, respectively, for participants and nonparticipants who attended school on the reference day (1,542 children in all). Figure 4 shows that snacks play an important role in the Filipino child's diet. Even among children who did not participate in a feeding program on the reference day, 96 percent had either or both a morning or an afternoon snack. Together these snacks amounted to 309 kcals., on

Figure 3 Daily calories from feeding program: Participants

Figure 4 Snack calories: Participants versus nonparticipants

Figure 5 Daily calories: Participants versus nonparticipants

average, which is 27 percent of total daily calories.[18] This fact suggests that, even if calories are not perfectly intertemporally substitutable (see section 2), providing about 300 kcal. in a feeding program would represent an inframarginal transfer, at least for the average child.

One might conclude from these figures that participation in school feeding programs has quite a dramatic impact on caloric intake, both for snacks alone and in total, and, hence, that the IFE is far from zero. Of course, such a conclusion would be premature, as I have not yet controlled for a host of potential differences between participants and nonparticipants.[19]

EMPIRICAL SPECIFICATION

A within-school analysis of the dietary impact of school feeding programs is necessary for two reasons. First, variation in feeding program characteristics, such as those discussed above, may induce variation in income across households. To replicate the conceptual experiment in Figure 1, it is necessary to compare the caloric intake of

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[18]Understandably, midday snacks are somewhat less important for the 1,647 children who did not attend school on the reference day. These children consume an average of only 220 kcals. from morning and afternoon snacks, and about 20 percent ate neither snack.

[19]A possible explanation for the apparently positive impact of program participation on caloric intake is based on measurement error. To whit, suppose that children tend to understate the quantities consumed in any given meal, but that the degree of understating is increasing in the size of the meal. On days with the program, children are provided an extra meal and consume less during their other meals. In this case, even though their total caloric intake is unchanged, measured intake rises because children are eating more meals. Yet, this explanation is belied by the fact that most children who attend school consume midday snacks in the absence of a feeding program.

children with access to an identical feeding program.[20] Second, the characteristics or presence of the school feeding program may be related to school-level unobservables. As mentioned earlier, schools with more nutritionally concerned parents may adopt better feeding programs, or, alternatively, program sponsors may supply schools with meals based on unobservable characteristics of the students (see Rosenzweig and Wolpin, 1986). Similarly, more nutritionally concerned parents may choose to send their children to schools with better nutrition programs.

To gain intuition for my empirical strategy, I begin with a simplified model. Suppose that: (a) school attendance is exogenous; (b) all children who attend school take up the feeding program; (c) all children who take up the feeding program obtain (i.e., consume) the same number of calories from it. Later, I will relax these assumptions.

Let D_{is}^{A} be an indicator variable for whether child i enrolled in school s attended school the previous day, and let D_{s}^{P} be an indicator for whether school s offers a feeding program. The equation for total daily calories, C_{is}^{T}, is

$$C_{is}^{T} = \alpha_p D_s^P D_{is}^A + \alpha_A D_{is}^A + \delta_s + u_i, \tag{1}$$

where δ_s is a school fixed effect and u_i is a child-specific disturbance reflecting unobserved determinants of caloric intake. Here, α_p is the average dietary impact of the feeding program, which only matters for children attending schools offering the program, and α_A is

[20]Feeding programs also vary within schools, by grade (age) and by nutritional status of the child, both of which can be controlled for in a regression context.

the average dietary impact of attending school; children may have greater energy requirements on school days than on nonschool days. I assume, plausibly, that α_A does not vary according to whether a school offers the program. An analogous equation can be specified for snack calories, or any other meal for that matter.

For the reasons discussed above, $E\left[\delta_s | D_s^P\right] \neq 0$, in general, and ordinary least squares estimates of (1) will be biased. A consistent estimator of the dietary impact of the program is

$$\begin{aligned}\hat{\alpha}_p = &\left\{E\left[C_{is}^T | D_s^P = 1, D_{is}^A = 1\right] - E\left[C_{is}^T | D_s^P = 1, D_{is}^A = 0\right]\right\} \\ &- \left\{E\left[C_{is}^T | D_s^P = 0, D_{is}^A = 1\right] - E\left[C_{is}^T | D_s^P = 0, D_{is}^A = 0\right]\right\},\end{aligned} \qquad \textbf{(2)}$$

which is simply a "differences-in-differences" estimator. The first term in curly brackets is the difference in mean caloric intake between attenders and nonattenders in schools offering the program, while the second term in curly brackets is the same mean difference in schools not offering the program. This estimator can also be implemented by including a set of school dummy variables in (1) and estimating the resulting equation by OLS.[21]

Next, I relax assumptions (b) and (c) by including the actual calories *consumed* from the program, C_{is}^P, on the right-hand side, instead of $D_s^P \times D_{is}^A$, to get

$$C_{is}^T = \alpha_p C_{is}^P + \alpha_A D_{is}^A + \beta' X_i + \delta_s + u_i, \qquad \textbf{(3)}$$

[21]In this simplified model, an equivalent approach would be to include D_s^P separately in (1). However, this procedure is inadequate if, as in the more general specification below, programs differ across schools.

where X_i is a vector of additional control variables to be discussed later. There are good reasons to believe that C_{is}^{P} is endogenous. Not every child will be inclined to accept a bowl of bland bulgur when it is offered, at least not every day; indeed, my data indicate a take-up rate far below one.[22] Since preferences for the program are plausibly related to the unobservables in (3) and since take-up is not universal in the sample, C_{is}^{P} and u_i are likely be correlated. In addition, hungrier children might get bigger portions and children with small appetites may waste theirs, so the number of calories consumed *conditional* on participation in the feeding program may also be correlated with u_i.[23] Finally, C_{is}^{P} may be measured with error, although, because the dependent variable C_{is}^{T} is partly composed of C_{is}^{P}, the consequences of (classical) measurement error differ from the standard case. In fact, it is easy to show that the bias in the OLS estimate of α_p is proportional to $1-\alpha_p$ (instead of $-\alpha_p$). Therefore, measurement error bias is positive under the null hypothesis $\alpha_p = 0$, but approaches zero as the true value of α_p approaches unity.

In thinking about an instrumental variable for C_{is}^{P}, recall that equation (3) contains school dummy variables and that I continue to treat school attendance as exogenous. Consider the "leave-out" school mean $\overline{C}_{(i)s}^{P} = n_s(\overline{C}_s^{P} - C_{is}^{P})/(n_s - 1)$, where $\overline{C}_s^{P}$ is the mean of C_{is}^{P} in school s, and n_s is the number of sample children in that school. My

[22]The survey asks each child whether they were *offered* food from a school feeding program on the reference day and, if so, whether they took it; these numbers imply a take-up rate of about two-thirds.

[23]As mentioned earlier, most school administrators report targeting the programs to malnourished children. I have good measures of nutritional status in my data, namely weight-for-age and height-for-age, which I can include in the regression. However, targeting based partly on unobservables that are correlated with the error term in (3) could be another source of endogeneity bias.

instrument for C_{is}^{P} is the interaction $\overline{C}_{(i)s}^{P} \times D_{is}^{A}$. Intuitively, this instrument is just like $D_{s}^{P} \times D_{is}^{A}$ in (1), but with $\overline{C}_{(i)s}^{P}$ capturing between-school variation in program characteristics. Because many of the schools contribute only a few children to the sample, I use leave-out means instead of conventional means to avoid any correlation between my instrument and u_i.

Finally, I relax assumption (a). School attendance may be endogenous if absenteeism is correlated with appetite due to illness, or if truants tend to have poorer diets. Errors in reported attendance will also induce a correlation between D_{is}^{A} and u_i. My instrument for D_{is}^{A} is an indicator variable, denoted by D_{is}^{B}, for whether the child was interviewed on a Sunday or Monday, or during April or May, which is the school break. I also replace the instrument for C_{is}^{P} in the previous paragraph with $\overline{C}_{(i)s}^{P} \times D_{is}^{B}$.

5. EMPIRICAL ANALYSIS

IMPACT OF SCHOOL FEEDING PROGRAMS ON SNACK CALORIES

I begin by examining how participation in school feeding programs affects caloric intake during the meals in which they are provided; that is, during morning and afternoon snacks. Twelve percent of the sample consumed neither a morning nor an afternoon snack on the reference day, so the dependent variable (combined calories from both snacks) is censored at zero for these children, most of whom are nonattenders. To avoid possible bias due to aggregating calories from the two snacks, separated as they are by several hours, I also estimate the impact of calories from the morning feeding programs alone on morning snack calories. Recall that almost two-thirds of the children who participated in a feeding program on the reference day obtained only a morning snack. Censoring of the dependent variable is more serious here, with 30 percent of the sample not consuming a morning snack on the reference day (83 percent of these are nonattenders).

The regression results for combined snack calories and morning snack calories are reported under the first two major headings in Table 2, and I will discuss them together. But before delving into the details, the overall picture is this: School feeding programs do significantly increase caloric intake during the meals in which they are provided; on average, intakes increase by 90-100 percent of the program rations!

I report several estimates of equation (3) in Table 2, under successively less restrictive assumptions. At first, I ignore censoring of the dependent variable and estimate linear models. Specification (1) includes no regressors other than C_{is}^{P}, D_{is}^{A}, and school dummies. Although they are jointly significant, adding child characteristics (age, sex, weight-for-age, height-for-age), mother's schooling, total household nondurable expenditures, and household composition variables, in specification (2) hardly affects $\hat{\alpha}_p$.[24] Evidently, within schools, these variables are not highly correlated with participation in school feeding programs or school attendance. Summary statistics and coefficient estimates for the child and household characteristics are reported in the Appendix.

The remaining linear specifications in Table 2 relax the exogeneity of C_{is}^{P}, but this does not significantly affect $\hat{\alpha}_p$ in either the combined snack or morning snack regressions, nor whether I treat D_{is}^{A} as exogenous (specification (3)) or as endogenous (specification (4)). However, one question raised by my identification strategy is whether the date of interview can indeed be excluded from the calorie regressions. After all, caloric intake might vary by day of the week or month of the year. Although I cannot test these hypotheses jointly without losing identification (D_{is}^{B} is a linear

[24]One can view equation (3) as an Engel curve for calories, hence the inclusion of household expenditures. Child weight-for-age and perhaps height-for-age could be endogenous, if they reflect, say, parental favoritism toward (or against) the child in caloric intake. Household expenditure could also be endogenous due to heterogeneity in tastes or measurement error. However, given the lack of influence of these variables on my main results, correcting for these potential endogeneity problems is not worth the loss of efficiency.

Table 2 Impact of school feeding programs on caloric intake

Specification	Morning + Afternoon Snack			Morning Snack Only			Total Daily Calories		
	$\hat{\alpha}_P$	$\hat{\alpha}_A$	p=value	$\hat{\alpha}_P$	$\hat{\alpha}_A$	p=value	$\hat{\alpha}_P$	$\hat{\alpha}_A$	p=value
All Schools (N=3,189)									
(1) OLS, $\beta = 0$	0.985	88.3		0.892	84.2		1.059	79.9	
	(0.069)	(10.1)		(0.039)	(6.5)		(0.140)	(23.9)	
(2) OLS	0.983	84.7	0.000[a]	0.897	82.8	0.000[a]	1.104	61.1	0.000[a]
	(0.068)	(10.0)		(0.038)	(6.5)		(0.134)	(21.9)	
(3) 2SLS (IV1)[b]	1.019	83.1	0.662[c]	0.912	82.4	0.757[c]	1.358	49.5	0.443[c]
	(0.200)	(12.8)		(0.161)	(8.1)		(0.452)	(28.7)	
(4) 2SLS (IV2)[d]	0.977	85.0	0.635[c]	0.911	100.0	0.751[c]	1.153	38.9	0.739[c]
	(0.198)	(16.9)		(0.144)	(10.7)		(0.460)	(39.0)	
(5) 2SLS (IV2),	1.053	118.3	0.148[e]	0.931	128.0	0.266[e]	1.082	65.6	0.616[e]
Day of interview dummies	(0.202)	(26.9)		(0.148)	(16.2)		0.464)	(66.0)	
(6) 2SLS (IV2),	1.106	53.0	0.024[f]	1.004	82.8	0.146[f]	1.108	37.7	0.658[f]
Month of interview dummies	(0.198)	(18.4)		(0.145)	(11.2)		(0.458)	(41.0)	
Schools with $n_s \geq 20$ (N=2,439)									
(7) Tobit[g]	0.961	104.8		0.920	130.2				
	(0.051)	(10.9)		(0.049)	(8.5)				
(8) Two-Stage Tobit (IV2)[h]	0.855	115.8	0.659[c]	0.891	160.7	0.923[c]			
	(0.243)	(22.4)		(0.225)	(16.2)				
(9) Censored LAD[i]	0.929	96.1		0.921	122.7				
	(0.068)	(14.8)		(0.063)	(16.4)				

Notes: Huber/White standard errors, corrected for intrahousehold correlation, in parentheses. Models include school dummy variables and X={child's age, sex, weight-for-age, height-for-age, mother's schooling, total household nondurable expenditures, and ten household composition variables} (see Appendix).

[a] H_0: exclusion of X_i ($\beta = 0$).

[b] IV1 = $\{\overline{C}^P_{(i)s} \times D^A_{is}\}$. F-tests for IV1 in first stage: F(1,3012)=173.8 for combined snack; F(1,3012)=193.7 for morning snack.

[c] H_0: exogeneity of C^P_{is}.

[d] IV2 = $\{\overline{C}^P_{(i)s} \times D^B_{is}, D^B_{is}\}$. F-tests for IV2 in C^P_{is} first stage: F(2,3012)=145.1 for combined snack , F(2,3012)=153.5 for morning snack;. F-tests for IV2 in D^A_{is} first stage: F(2,3012)=1228 for combined snack; F(2,3012)=1226 for morning snack.

[e] H_0: exclusion of day of interview dummies.

[f] H_0: exclusion of month of interview dummies.

[g] Conventional standard errors. OLS estimates on this sample: 0.966 (0.070), 86.4 (10.7) for combined snack; 0.898 (0.040), 83.8 (7.1) for morning snack.

[h] Conventional standard errors. 2SLS (IV2) estimates on this sample: 0.885 (0.221), 99.3 (20.1) for combined snack; 0.871 (0.165), 107.3 (12.4) for morning snack.

[i] Bootstrapped standard errors corrected for intrahousehold correlation using two-stage resampling (500 replications).

combination of month and day of interview dummies), I can test them individually. Thus, specification (5) includes day of interview dummies, while specification (6) includes month of interview dummies. Only in the case of specification (6) for the combined snack are the exclusion restrictions rejected.[25] Even so, there is no significant change in $\hat{\alpha}_p$.

As noted, there are numerous zeros in the disaggregated caloric intake data, so I now turn to nonlinear models that deal with censored dependent variables. An immediate concern in nonlinear models is the incidental parameters problem. With many schools contributing only a few children to the sample, estimates of at least some of the school fixed effects are inconsistent. Though not a problem in linear models, in the nonlinear case the inconsistency of the fixed effects is transmitted to the other parameters (see Heckman and MaCurdy, 1980). To avoid this problem, I drop schools contributing fewer than 20 children to the sample,[26] which leaves just 34 schools and 2,439 children. OLS and 2SLS estimates on this smaller sample are similar to the full sample results (see notes to Table 2).

The implications of censoring are explored in the last three specifications of Table 2. Tobit and two-stage tobit (Smith and Blundell, 1986) estimates are quite close to the corresponding OLS and 2SLS estimates based on the same sample, and, once again, the

[25]This rejection seems anomalous in that α_A drops substantially relative to the other specifications, and, at the same time, the only month dummies that are individually significant are those for April and May, the months of the school break. Moreover, the month dummies are jointly insignificant in the corresponding specification for morning snacks, as well as for total daily calories.

[26]Hotz and Miller (1988) perform a Monte Carlo study for a tobit model showing that the small sample bias is largely negligible with as few as 10 observations per school (time periods per household in their application). Based on their findings, 20 observations per school is quite conservative.

exogeneity of C_{is}^{P} cannot be rejected. While the assumption that u_i is normal allows one to deal with endogenous covariates, the tobit model is not robust to heteroscedasticity or intrahousehold correlation.[27] Therefore, I also present censored least absolute deviation (CLAD) estimates (Powell, 1984), which are robust to these deviations from the i.i.d. assumption. The CLAD estimates are in close agreement with the tobit results. Since the CLAD estimator cannot accommodate endogenous covariates, I cannot assess the robustness of the two-stage tobit.

In and of itself, the finding that $\hat{\alpha}_p$ for snack calories is far from zero (and close to one) sheds no light on the IFE. We should find that $\hat{\alpha}_p$ equals one if children never get a snack unless provided by a feeding programs, since parents are presumably unable to take school meals out of the mouths of their children. But, as we have seen, snacks make up a substantial portion of the diet of children who do not participate in feeding programs. Moreover, children who do participate continue to receive about the same amount of calories from home as do nonparticipants. So, the evidence suggests that parents are not withdrawing snack calories from their children in response to the programs. Keep in mind, however, that parents probably have little or no control over when during the school day their children consume food brought from home or when they spend their pocket money, which is why we must also look at total daily calories.

[27]Given that the tobit model is not consistent under intrahousehold correlation, I do not correct the tobit variance-covariance matrix for clustering by household.

IMPACT OF SCHOOL FEEDING PROGRAMS ON TOTAL DAILY CALORIES

The total calorie regression results are presented under the last major heading in Table 2. Following the same sequence of specifications as before, I find no significant differences in the estimates of α_p—all the point estimates are around unity. Standard errors, however, are more than double those of the combined snack regressions, and about triple those of the morning snack regression.[28] Nevertheless, the joint null of zero IFE and perfect intertemporal substitutability of calories within the day is rejected (p-values: 0.000 based on spec. (2) and 0.012 based on spec. (4)).

What about the IIS hypothesis, under which total calories increase in response to the program, but not by as much as snack calories? Recall from section II that IIS is "binding" only if the program provides more *snack* calories than parents would have supplied to their child in its absence. As discussed earlier, this does not appear to be the case in my sample, at least not on average. Perhaps it is not surprising then to find no support for the IIS hypothesis in Table 2; that is, school feeding programs increase total daily calories by about the same amount—in fact, by more (but not significantly so)—than they increase snack calories. One qualification, however, is that the large 2SLS standard errors preclude the possibility of detecting "small" deviations of the two estimates of α_p from each other. This is a symptom of a more general problem, which I turn to next.

[28]Since both sets of regressions contain the same explanatory variables and use the same instruments, the difference in standard errors must be due to the fact that a larger portion of the variance in total daily calories is unexplained by the regressors.

HOW MUCH INERTIA DO HOUSEHOLDS EXHIBIT?

An alternative to testing whether the IFE is different from zero ($\alpha_p = 0$) is to test the hypothesis of complete inertia ($\alpha_p = 1$), in which parents make no calorie adjustments on the day the program is provided. Obviously, this hypothesis fares extremely well based on the results in Table 2. However, the salient question is how much power does this test of inertia have against the alternative that parents "partially" adjust child calories in response to the program (α_p (0,1)); the IIS hypothesis being one form of partial adjustment. Clearly, the 2SLS standard error of 0.46 (specification (4) for total calories) could conceal substantial adjustments.

The only way to increase power is to impose restrictions so as to improve the precision of the estimates. One approach is to impose the exogeneity of C_{is}^{P} and D_{is}^{A}, since these restrictions cannot be rejected in the total calorie regression, nor in the snack regressions, where precision (and thus the power of the exogeneity test) is much higher.[29] The OLS standard error of 0.134 does allow us to reject inertia against a much wider range of alternatives than the 2SLS standard error. Based on an inverse power function calculation (Andrews, 1989), for a one-sided test of $\alpha_p = 1$ at the 0.05 level, we reject with probability 0.95 if the true value of α_p is less than 0.559, and with probability 0.5 if the true value is less than 0.780. In other words, while it would be difficult for the test to

[29]Moreover, in the snack regression, endogeneity bias in α_p is arguably greater than in the total calorie regression because C_{is}^{P} is likely to be more strongly correlated with the unobserved taste for snack calories than with the unobserved taste for total daily calories. Notwithstanding the smaller standard error in the snack regressions, the Wu-Hausman test (at the 0.05 level) has low power to detect a discrepancy between the OLS and 2SLS estimates of α_p that is less than 0.364 in magnitude (see Andrews, 1989).

distinguish an intrahousehold tax rate of 22 percent or less from complete inertia, the test has high power against alternatives in which more than half of the calories provided by the program are taxed away.

An additional restriction worth considering is that all of the caloric adjustment to the feeding programs takes place during school meals; that is, during snacks and lunch. According to the diet recall data, all but 13 of the 3,189 children ate lunch on the reference day, and these lunches averaged 278 kcals., about the same for attenders and nonattenders.[30] If school meal calories (combined snack calories + lunch calories) are less noisy than total daily calories, then isolating the impact of school feeding programs on school meal calories will provide more power against the partial adjustment alternative.

An OLS regression of school meal calories on school feeding program calories, analogous to specification (2) in Table 2, produces $\hat{\alpha}_p$ = 0.979 (0.080), and its 2SLS counterpart (specification (4)) is $\hat{\alpha}_p$ = 1.000 (0.257). As before, neither estimate indicates that parents adjust calories in response to the program, nor that C_{is}^{P} is endogenous. More importantly, assuming that all adjustment does take place during school meals, these estimates provide considerably more power against partial adjustment alternatives than those based on total daily calories. Specifically, the OLS standard error implies that we reject $\alpha_p = 1$ at the 0.05 level with probability 0.95 if the true value of α_p is less than 0.737, and with probability 0.5 if the true value is less than 0.868. Thus, the test of inertia

[30]Most children (73 percent) who attended school on the reference day brought a lunch from home, and the rest purchased their lunch around school, mainly from street vendors.

has high power against alternatives in which only a quarter or more of the child's calories from the feeding program are taxed away by the household.

DOES THE IFE DIFFER ACROSS HOUSEHOLDS?

My results imply an inefficiency in the intrahousehold allocation of calories. Children's windfalls from the feeding program are insufficiently taxed to finance other household members' consumption. One might argue that, because these feeding programs provide only a modest transfer, the inefficiency is small. However, presumably, poorer households would be less tolerant of such misallocations, suggesting an inverse relationship between household income and the intrahousehold tax rate. Writing $\alpha_p(y_i)$ as a function of income y_i, the "poorer is more efficient" hypothesis implies that $\alpha_p'(y_i)>0$. Taking a linear approximation to $\alpha_p(y_i)$, the test boils down to whether an interaction term between C_{is}^P and y_i is significantly positive in equation (3).

I use per-capita nondurable household expenditures as a measure of income (recall that total expenditures and household composition variables are already included in X_i), which has a mean of 10.3, a standard deviation of 6.8, and a minimum of 0.8 thousand Pesos. Adding the interaction term to specification (2) for total calories yields

$$\hat{\alpha}_p = \underset{(0.240)}{0.673} + \underset{(0.0156)}{0.0358}\,y_i$$

which supports the poorer is more efficient hypothesis,[31] though even the poorest households in the sample have an $\hat{\alpha}_p$ far from zero. I do not present comparable estimates treating C_{is}^P and D_{is}^A as endogenous, as these proved extremely imprecise and endogeneity bias should in any case be no more of a problem here than it was in Table 2. I also do not correct for possible measurement errors in per-capita expenditures, since such errors would likely only bias the estimates in favor of the null, which I reject anyway.

As a robustness check I estimate the more flexible Box-Cox model

$$\alpha_p(y_i) = \tilde{\alpha}_p^0 + \tilde{\alpha}_p^1\left(\frac{y_i^\lambda - 1}{\lambda}\right) = \alpha_p^0 + \alpha_p^1 y_i^\lambda, \tag{4}$$

of which the above estimates are a special case (λ=1). The maximum likelihood estimate (MLE) of λ is -0.88, though it is highly imprecise and a likelihood ratio test cannot even reject the hypothesis that λ=1 (p-value=0.20). Still, the finding that $\alpha_p'(y_i)>0$ is robust to the choice of λ.[32] Given the lack of precision, it would be imprudent to say much more about the shape of $\alpha_p(y_i)$ based on these MLE results, except to note that they imply that $\hat{\alpha}_p>0.5$ for all but the poorest 7.5 percent of households in the sample and $\hat{\alpha}_p>0.25$ for all but the poorest 3.7 percent of households.

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[31]By contrast, an interaction between C_{is}^P and the age of the child, included in a separate specification, is not significant (p-value=0.63). Thus, I find no evidence that the IFE differs by the age of the child, as might be suggested by the Tamil Nadu results in Table 1.

[32]When λ = -0.88, $\hat{\alpha}_p^0$ = 1.551 (0.226) and $\hat{\alpha}_p^1$ = -3.34 (1.15), where the (robust) standard errors are conditional on λ and hence are understated. Though it is nonlinear, incidental parameters (the school dummy coefficients) do not create a problem in the Box-Cox model because it is linear conditional on λ.

6. CONCLUSIONS AND IMPLICATIONS

A striking implication of the theory of altruism is that an inframarginal transfer to a household member should not "stick". This paper provides the most direct test to date of the absence of an intrahousehold flypaper effect and finds no evidence to support it. My empirical results indicate virtually no intrahousehold reallocation of calories in response to school feeding programs. While policy-makers concerned with the welfare of children might draw comfort from these results, my evidence also suggests that children's gains from the programs are "taxed" more heavily in poorer households; that is, precisely in those households where the public interest in helping children is presumably the greatest.

The obvious question at this point is what do these results have to say about the theory of altruism. Altonji, Hayashi, and Kotlikoff (1992) attribute their rejection of income pooling within extended families to the absence of altruistic linkages at the margin. Altruistically motivated transfers exist, they argue, but do not operate to efficiently allocate consumption across extended family units. The same case is hard to make, however, when talking about the transfer of calories from parents to children, since it is only through a continuous flow of such transfers that children do not starve!

A possibility considered earlier is that the transfers provided by the feeding programs that I study are small enough for households to ignore, at least on average.[33] But one can take this "bounded rationality" argument only so far, since the transfer does amount to about a fifth of daily calories for the average participant. Moreover, most, if not all, of the poorer households in my sample exhibit the flypaper effect.

One interpretation of a nonzero IFE is as a manifestation of "loss aversion" or of an instantaneous "endowment effect" (Kahneman, Knetsch, and Thaler, 1990; Tversky and Kahneman, 1991). That is, parents are loath to tax away their child's calories on the days of the feeding program because parental preferences for child consumption change as soon as the program calories are incorporated into the child's endowment; in other words, it is not that easy to take candy from a baby. An implication of the asymmetry of loss aversion is that if a program existed that could somehow *confiscate* calories from children at school, altruistic parents would respond by subsidizing their child's calories on the days the program operated. Unfortunately for economists, such programs are difficult to find in practice, making loss aversion hard to test in this context.

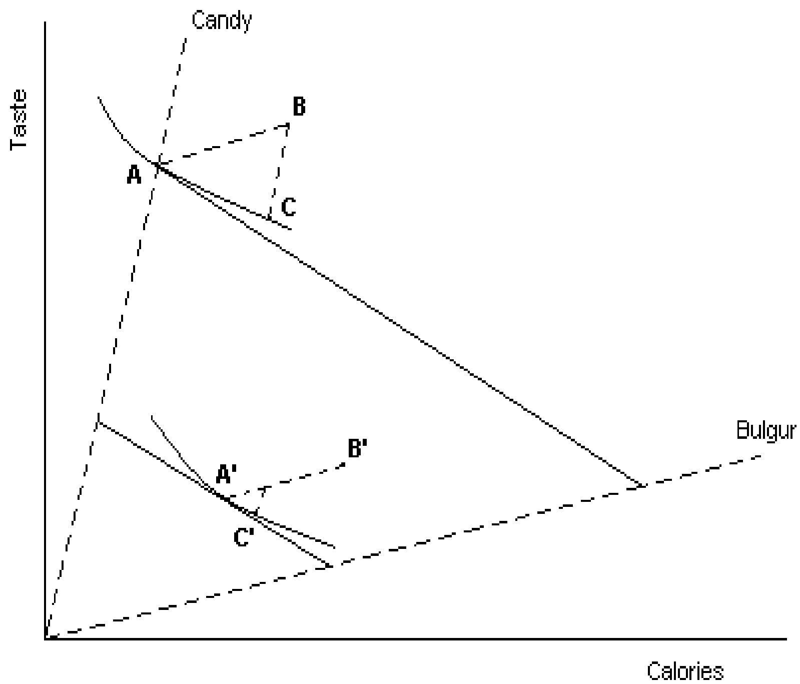
A different way to potentially reconcile my results with the theory of altruism is by recognizing the multidimensional nature of food. In the linear characteristics model

[33]Alternatively, if children do not participate in a feeding program every day, then parents may simply not be aware of when their children are obtaining snacks from the program. However, in my sample, about half of the 226 children who participated in a program on the reference day report that they obtain food from this program five days a week, and the average frequency for all 226 children is 3.8 days per week. To check whether the IFE is lower for children who participate in a feeding program less regularly, I include an interaction term between C_{is}^{P} and the frequency of participation in specification (2) of Table 2. This term does not attract a significant coefficient (p-value=0.86), lending no support to the "awareness" story.

(Gorman, 1980; Lancaster 1966) illustrated in Figure 6, snack foods are valued for their taste and calories. Suppose that only two snack food items are available in the market, candy and bulgur (or a perfect substitute for bulgur). Candy is presumed to have a much higher taste to calorie ratio than bulgur, as indicated by the respective slopes of the rays from the origin. The child at point A consumes only candy as a snack in the absence of the program, while the poorer child, on the lower budget line at point A , consumes both goods (snack calories are assumed inferior). Now, the program offers both children the relatively bland bulgur, moving them to B and B , respectively, without any parental adjustment. For the parents of the richer child, the only way to make their child indifferent to the program is to reduce his consumption of snack candy (ignore adjustments at other meals), which leads to only a slight reduction in calories (point C) relative to point B. Thus, for the rich child, we observe a large IFE in calories. Parents of the poor child, on the other hand, are less constrained in their ability to reduce calories. At first they eliminate the bulgur (or perfect substitute thereof) that they had provided their child, moving as far as possible down the "bulgur" ray, and then they reduce his candy consumption to reach point C . We still observe an IFE for the poor child, but it is not as large as for the rich child.[34]

[34]Another way to think about it is that the "bundling" of characteristics results in imperfect substitutability between candy and bulgur, so that the transfer of bulgur is not inframarginal.

Figure 6   Linear characteristics model



Although my data do not allow me to assess the empirical relevance of this story, since there is practically no variation in the nature of the snack provided, the fact that take-up of the school feeding programs in Cebu is not universal on a given day does suggest that children may occasionally get tired of eating bulgur (albeit sweetened) and prefer a tastier snack instead. In any case, under this explanation for the flypaper effect, children would be no better off on the days the program is offered, although their consumption on these days would be distorted in the direction favored by the policy-maker; that is, toward more nutritious snacks.

Clearly, more programs like the one examined here must be studied using similar methods to determine whether the Cebu experience is a fluke or whether it necessitates a paradigmatic shift; that is to say, a more general model of parent-child interaction. Indeed, given the relevance for policy, not to mention for our understanding of intrahousehold allocation, it is surprising that the caloric impacts of supplementary feeding programs have not been subject to more rigorous evaluation, particularly using randomized experiments. An interesting experiment suggested by the approach that I take in this paper would be to offer a program to the same child on alternative days, perhaps varying the characteristics of the good transferred as well. Repeated observations on individual calorie intakes would allow extremely precise estimates of the IFE. In addition, caloric intake data for other household members collected on the same days as those of the child would provide even more powerful evidence on how households reallocate resources in response to government programs.

Appendix

Table 3 Descriptive statistics and regression coefficients for other variables

Variable	Mean (Standard Deviation)	Coefficients from OLS regressions: Combined snack	Coefficients from OLS regressions: Total calories
Age of child	10.73	9.2	56.8
	(1.26)	(2.9)	(5.3)
Sex of child	0.487	-9.3	-92.6
	(0.500)	(9.6)	(19.0)
Weight-for-age Z-score	-1.62	6.7	137.2
	(0.82)	(8.5)	(20.1)
Height-for-age Z-score	-1.91	13.3	-1.8
	(0.99)	(6.3)	(14.1)
Total nondurable household expenditures	69.87	0.51	1.96
(1,000 pesos)	(44.58)	(0.13)	(0.31)
Mother's years of schooling	7.07	1.7	17.4
	(3.27)	(1.6)	(3.4)
Males age 0-5 in household	0.500	-13.1	-34.7
	(0.747)	(5.6)	(10.8)
Males age 6-11 in household	1.069	-11.9	-44.4
	(0.860)	(5.8)	(12.3)
Males age 12-17 in household	0.680	-12.5	-50.9
	(0.805)	(4.9)	(11.1)
Males age 18-55 in household	1.325	5.3	3.2
	(0.843)	(5.5)	(12.0)
Males age 56 and up in household	0.094	14.5	-31.3
	(0.293)	(17.1)	(34.0)
Females age 0-5 in household	0.457	-10.2	-36.0
	(0.698)	(6.1)	(14.5)
Females age 6-11 in household	1.018	-11.0	-41.9
	(0.852)	(6.0)	(12.4)
Females age 12-17 in household	0.672	-2.9	-18.6
	(0.793)	(5.5)	(11.3)
Females age 18-55 in household	1.346	-3.7	-8.5
	(0.782)	(6.1)	(13.8)
Females age 56 and up in household	0.121	-2.6	-27.6
	(0.344)	(12.1)	(28.2)
R^2		0.36	0.42

Notes: Standard errors in parentheses (see notes to Table 2).

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