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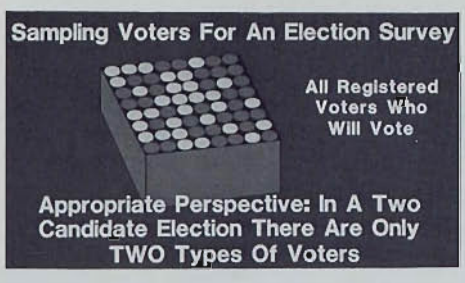
LETTERS *From You...*



From: Ron Fecso
Head, Nonsampling Errors Research Section
Survey Research Branch
Research and Applications Division, NASS/USDA
Re: Dillman's "Sample Survey Technologies"

In this age of information, it is sometimes distressing to see how little data users know about the process which generates one of their most important decisionmaking tools—data. Don Dillman and the editors have my appreciation for providing a "user friendly" introduction to the "engineering" of the process which generates data.

There are several points in the article which do need correction or clarification. At first I thought that the opening paragraph was a trick which would lead into a discussion of specification error or inappropriate comparisons. Even the most confident model-based sampler would feel uncomfortable with inferences for Oregon from a sample of Washington voters, regardless of sample size, likewise for Salem from Spokane. I assume these were processing errors. Next, specification error, processing error and incorrect estimators should be added to the list of errors to avoid.

Dillman states that in large national research organizations it is usually the case "that all error components are treated carefully." Having been on the subcommittee on Measurement of Quality of Establishment Surveys and being familiar with the methodology of several private survey firms, I believe the statement is overly optimistic. One should refer to the Office of Management and Budget, Statistical Policy Working Paper 15, "Quality in Establishment Surveys," for insight into the relatively infrequent use of error measurement and control procedures in the Federal surveys reviewed in the report. The pressures of increased data demands, tightening budgets and the scarcity of newly trained survey researchers are affecting the large Federal and Private survey groups in ways similar to those linked with "small organizations." These caveats addressed, I hope the readers took note of the important comments made in the article.

The quality control gurus tell industrial leaders that they must awaken to their problems, face them openly and make long-range plans for their removal for improvement to take place. Is Dillman's statement that "there is no discipline of survey research" a problem we're willing to face? Will we continue to use response rates, number of completed interviewers, etc., to imply quality, or will we begin actually measuring and reporting total survey error? Will universities recognize the need for trained survey researchers before the value of the information age to our society is gone?

There are some bright spots. A few universities have excellent survey research programs. The Census Bureau has extensive in-house training for newly hired statisticians. Also, a plug is due for one of my interests, the reemergence of the Graduate School of USDA in the area of survey sampling, an area in which it excelled under the leadership of Deming during the 1940s. Thanks for help in this effort is extended to Tom Jabine, Fritz Scheuren, Phil Kott and others. We must all explore ways in which we can help realize Dillman's call "to make basic training in all aspects of survey methodology a part of university curricula..." and for organizations to develop survey researchers with longstanding ties to particular surveys.

In conclusion, I found intriguing the call for a more flexible, decentralized and expanded national data collection system. I wonder whether large organizations will recognize the threat of competition which many may have thought impossible. Let's hope the competition materializes and results in improved accuracy and relevance of our surveys. Yet, if "there is no discipline of survey research," who will protect the untrained and unwary secondary data analysts from an avalanche of cheaper and quicker data which lacks the accuracy to be relevant?

From: The Editor
Fesco is correct regarding the first paragraph. It was an error on our part. Sorry. We appreciate alert readers. This slip reminds me of Louis Bean, the person who predicted Dewey's defeat and Truman's victory. His technique relied heavily on using information from selected precincts/states as a basis for anticipating outcomes for the nation. See his book "How America Votes" for an illustration of his approach.

From: Don A. Dillman
Washington State University
Re: The Author Responds

This survey researcher is certainly among those who would *not* attempt to make inferences about voters in one geographic area from a sample survey of voters taken from another area. I, too, regret the editing error and the obvious confusion it created for readers. The appropriate corrections are to replace "Oregon's" with *"Washington's"* (page 12, line 8) and "Salem's" with *"Spokane's"* (page 12, line 12).

My identification of four types of survey error that need to be overcome follows that of Robert Groves in "Research on Survey Data Quality", *Public Opinion Quarterly* 51(4, Part II):S156-S172). Because of space limitations and for thematic reasons, I decided to limit the article to errors of data collection, and thus felt it appropriate to omit discussion of potential data processing and analysis errors. Mr. Fescoe has usefully pointed out that these additional sources of error (e.g., specification and incorrect estimators) also stand in the way of obtaining accurate reports from sample surveys.

My statement that large national research organizations treat all error components carefully was purposely qualified to those organizations, "which do surveys that are designed over a long period of preparation and are peer reviewed...(and) have individuals with different survey training in the organization so that all error components are treated carefully." I agree with Mr. Fescoe's assertion that increased data demands, tightening budgets, and the scarcity of newly trained survey researchers are affecting such organizations as well as the small ones.

The increased cost of surveying, which funders seem less will-

ing to accept, could be added to Mr. Fescoe's list. Nonetheless, I believe the pressures, both internal and external, for quality surveying are substantially less at state and local levels and likely to remain that way. The reasons are multiple—smaller budgets which make sponsors more tolerant of error, the lack of well trained survey methodologists to conduct or monitor such surveys, and the lack of checks and balances that stems from having fewer methodologists (from whatever discipline) involved in the conduct and reporting of results.

Mr. Fescoe's challenge, "will universities recognize the need for trained survey researchers before the value of the information age to our society is gone?" is indeed provocative. To the best of my knowledge, there is no department of research methods (let alone survey research) in any U.S. university. Such departments do exist in other countries, e.g., the Department of Research Methods at the Free University of Amsterdam in The Netherlands.

Whether through formal creation of university departments (or institutes) or less formal means, the need for concerted research aimed at improving survey quality has never been greater. Recent cognitive research shows dramatic effects from changing question order or even the order in which categories are presented to respondents. These issues, coupled with the response rate challenge now facing all modes of research, have pushed the survey research frontiers onto multiple fronts, none of which can or is likely to be effectively addressed by a single discipline.

Indeed, the problem of survey error is complicated by the different perspectives and resultant terminology scientists from different disciplines use to refer to survey error. For some, error is synonymous with sampling and is measured in statistical terms. For others, such concepts as reliability and validity connote error. We as of yet have no "Gross Survey Product" that combines multiple indicators of survey quality in a way that has general acceptance. Learning one another's languages is an important first step in developing such indicators.

These challenges come at precisely the time when we are developing the striking new survey capabilities at state and local levels in response to the evolving information age. The quality of societal guidance is at stake.

Rural America

From: Eldon D. Smith
University of Kentucky

Re: Knutson and Fisher's "Fragmentation Moving Toward Consensus" and Deavers' "Rural America...Do We Care? (Second Quarter 1989 *CHOICES)*

Consensus, Knutson and Fisher correctly recognize, is essential to any constructive attack on public policy. But consensus about what? Destroying the myths that have trapped us is surely important as a first step, but myth destruction does little unless it is replaced by a consensus on a valid view of what needs doing and what the consequences will be on the part of those who have power to do something about it. And the evidence is rapidly accumulating that simply admonishing rural communities to educate their youth, keep them healthy and create jobs through business development does not reduce illiteracy, infant mortality, or chronic joblessness in rural areas. The ERS studies by Daberkow, Coltrane, Larson and others in Kentucky and Georgia put pretty deep cracks in the myth that employment growth equates to economic development. And our more recent surveys in that same general region of Kentucky indicate that, in fact, there were more unutilized labor resources after an increase of employment of over 125 percent than before. Schooling lags behind about as much as it did before, still about 45 percent dropout rates and abysmal scholastic achievement as measured by standardized tests. And the numbers which showed the strategic position of human capital have been available to politicians and educators for 30 years.

I am a cynic; I have little faith in appeals to either pure ethics of "Do unto others...", nor in appeals to human compassion. Deavers' plaintive negative answer to his rhetorical "Do we care?" about rural poverty, mostly casts the issue in terms of a council of despair, i.e., people don't care and won't do anything. While nodding acknowledgement of a "public goods" aspect of poverty, it fails to come to grips with the main issue, "Why should people (who are more comfortably situated) care?"

The barely perceptible nod to public goods aspects of the problem, should be, instead, an acknowledgement that it is basically the core of the problem, not a peripheral aspect. It is this, I would contend, rather than appeal to abstract ethics, which provides a basis for hope and a potential ground for public action in rural development. It is the public goods component which is the real underpinning of the Equal Economic Opportunity Act, statutory provisions for mandatory public education through age 16 in most states, Headstart and a variety of public welfare programs. Do-gooder appeals to compassion and abstract ethics have, as the Great Society episode shows, transient appeal, at best.

Lawrence Haynes and Mary Jean Bowman in *Resources and People in Eastern Kentucky* (U. of Kentucky Press, 1955) were on the right track in focusing on "costs and returns to public account," which appeals to the selfish motives of the power-broker elites and middle-class people who call the tune politically. Productivity losses and how they impinge on the costs of goods and services to us all are also real and potentially measureable. Both, if measured, raise the fundamental issue, "Can we afford *not* to address rural development problems?" Businessmen and other taxpayers can understand that to have upwards of $400 per capita unemployment insurance and income supplement costs in a region with four hundred thousand or so people is a not inconsequential fiscal burden even in an era of trillion dollar federal budgets.

But the social sciences have failed almost completely to provide the public with information on these fundamental consequences of adhering to the present, largely ineffective, job creation and physical infrastructure-centered strategies. (I also had tunnel vision in this respect.) And the present wave of interest in education will go the way of other efforts if the view is accepted that we must simply accept the value systems with respect to education of the families of the impoverished and the power brokers of their political establishment. All of the evidence, I would contend, suggests that valuation of personal investments in their own fund of human capital, and the institutional structures which supply education is at the heart of the problem. Building consensus based on dysfunctional valuations will simply not get the job done—institutional change and changes in values regarding education based on enlightenment regarding their consequences is the central problem. As Mark Blaug (The Methodology of Economics) succinctly puts it, "Decisionmakers do not try to get what they want; *they learn to want* by appraising what they (appear likely to) get." (p. 51, italics and parentheses added.)

The central points are simple. First, both Deavers' and Knutson and Fisher's remarks unfortunately and unnecessarily circumscribe the alternatives. With improved understanding of the

immense cost of doing little or nothing to develop stagnating rural (and urban) areas, the public for quite tangible reasons very possibly *will come to care*. But on us is the burden of proof!

Second, if the institutional structure of rural public education continues to be one which simply reflects existing, uninformed private demand for enhanced education and articulates the interests of those with a vested interest in cheap, unskilled and unschooled labor, not much will come of the current upsurge of interest. Institutional alternatives which with variable efficiency articulate the very large public goods component of educational productivity must be understood and addressed. Not just any consensus will do!

NOTE: *The Third Quarter 1989 issue of* CHOICES *included a letter re Knutson and Fisher's Rural Development article. It was from David L. Debertin, who is also from the University of Kentucky.*

The Editor

From: Ron Knutson and Dennis Fisher

Texas A&M University

Re: The Authors Respond

David L. Debertin seems to have missed a major point in our article. Our argument was that most rural areas have become dependent upon a wide variety of industries in addition to agricultural and natural resource based industries. Over a long period of time, the waning importance of agriculture and natural resource based industries has been a major cause of the lagging rural economy. In many areas these industries have declined in relative importance to the point that policies to shore them up may have little impact on the local rural economy. Thus, one cannot rely on farm policy to rescue rural America; otherwise, the record farm program spending levels in recent years would already have done the job.

Debertin indicates as an example that agriculture is very important to rural North Dakota. As evidence, he states that "cash receipts from sales of crops and livestock in many recent years has exceeded the total value added for all manufacturing in the state." Debertin further indicates that much of the manufacturing is linked to agriculture in the form of inputs or outputs. These arguments coupled with relatively high income and employment multipliers are often used to demonstrate the importance of agriculture.

When applied to the state's economy, Debertin's reasoning may be correct. When applied to the nation's rural areas in general, the reasoning is faulty for the following reasons:

- A small proportion of gross farm cash receipts stays in rural areas as income to farmers or farm workers.
- Second, most value added from manufacturing and marketing of agricultural commodities or inputs occurs in metropolitan areas.
- Even in a state as apparently dependent upon agriculture as North Dakota, passive income sources accounted for 40.5 percent of personal income in nonmetro counties in 1983. Maybe retirement income is the major driving force behind many of North Dakota's nonmetropolitan counties? In other words, a major force driving North Dakota's rural economies is that its people are getting old! That phenomenon has become so widespread throughout rural America that unearned income has become a significant factor in economic development.

All this is not to say that agriculture is unimportant to the state of North Dakota or to individual counties in North Dakota, but it does say that many rural counties that were once dependent on agriculture are now dependent on other sources of income—perhaps retirement, tourism, manufacturing and/or mining. Because of this diversity a strong farm policy will not rescue rural America nor will it rescue rural North Dakota! However, the comprehensive rural development policy developed by the workshop participants allows for selecting activities and programs to match local conditions. It would not be single-sector specific but would meet the needs of all rural people, including farmers.

We concur with Debertin that farm organizations and major agribusiness firms have not expressed interest in a comprehensive rural development policy. One must wonder if it will be a surprise to the leadership of these two groups when they recognize that their markets are in rural communities; their constituents live in rural America; their children go to rural schools; and, in the event of an emergency, their families would be taken to a rural hospital—if it has survived the rural medical crisis!

While Eldon D. Smith recognized that consensus is essential to enacting a national rural development policy, he went on to impose his own judgments regarding what that policy should involve. Our approach was quite different. Participants at the four national rural development policy workshops discussed the diversity of rural America and suggested policy options that would allow state and local areas to construct a mix of rural development programs and projects that would address the needs unique to their area. The participants' emphasis on a comprehensive approach involving a mixture of infrastructure and human capital avoids perpetuating the problems with the current unbalanced program. For example, investing in a mixture of human resource, infrastructure and business development activities increases the probability of a return to public and private investments in rural development. This approach avoids the low return to an infrastructure investment because complementary investments in human capital and/or business development were not made.

Smith correctly argues that economic development has not solved the rural poverty problem in much of the South. However, it should not be surprising to see that recruiting low-wage, low-skill manufacturing firms results in low-wage, low-skill jobs and firms which subsequently move out to take advantage of lower wages elsewhere—whether at home or abroad. Reliance on an unskilled labor force makes the South particularly vulnerable and emphasizes the need for human resource development programming to undergird economic development efforts in rural communities.

Yet, it should be clarified that the rural poverty problem in the United States and particularly in the South cannot be solved using an economic development approach alone. A substantial proportion of the poor are not able to enter the labor force, and therefore are not helped by job-oriented programs, although family members may be helped. This speaks to the diversity of conditions in rural America and the need to tailor packages to suit local conditions.

To argue that the view of society toward education in the South mitigates against a consensus merely indicates that public policy education and consensus building are needed to make the leadership aware of the situation, the alternatives and the possible consequences. We were particularly impressed by the willingness and ability of Southern leadership to recognize their challenges. However, it also became very clear that federal leadership and support would be required to deal with rural education problems. Education programs must be designed for both children and their parents if we are to change the view of education.

This "public good" nature of rural development is the primary argument for a comprehensive national rural development policy

and additional funding from the national level. To argue that rural development is primarily a state or local responsibility ignores the complexity of the problem and the national benefits to be gained from a developing rural economy.

PSEs

From: G. H. Peters

University of Oxford, United Kingdom
Re: Ballenger's PSEs

Because the concept is central to the GATT negotiations, which we are all watching with intense interest, it is hardly surprising that the theoretical underpinning of the producer subsidy equivalent is being subjected to increased scrutiny. Readers may have read the important article by Schwartz and Parker in the December 1988 *AJAE;* some may have already seen Tim Josling's exposition presented to the 1988 Argentina conference of the International Association; and if they delve further into the literature have noted my own comment in the 1988 *Oxford Agrarian Studies*. My attention has now been drawn to Nicole Ballenger's *CHOICES* First Quarter 1988 article in which she sets out estimates of PSEs for U.S. and Japanese beef. There are elements in those calculations which call for some comment.

The first difficulty is one of principle. In the table the beef producer prices are respectively $1,928 and $6,322 per ton. Values of policy transfers on price support are placed at $647 mil and $1,720 mil, which on production of 11,000,000 and 555,000 tons become $59 and $3,099 per ton respectively. This is not unexpected; Japan is particularly well known for the severity of its restrictions on beef imports. Ballenger states that one technique for calculating the price support element within PSEs relies on the difference between producer prices and a 'reference price,' or what we can regard as a 'world price'. The level used is not reported, though in this case it can presumably be inferred by subtracting price support per ton from the producer price. The results are $1,869 and $3,223. If the theory is what it appears to be one would not expect such an inexplicable difference to appear. Discrepancy could arise from transport costs or quality adjustments. If that is the case the matter should be clearly reported, otherwise there is a real danger that comparative PSEs will be constantly questioned. Failing such an explanation I can only describe myself as bemused, and frankly, somewhat suspicious.

A second feature of the calculation is also puzzling. This relates to the manner in which numerous types of expenditure are reduced to a 'common denominator', or their 'subsidy equivalent'. Ballenger very fairly states that when a mix of policies is involved there is a strong assumption that dollar for dollar effects are presumed equal. This can be debated at length with particular reference to input subsidies, as suggested in my own article.

There is, nevertheless, another point relating to the final presentation of PSEs. Let us make the key assumption, and use Japan as a hypothetical example. In that case the total policy transfers are $2,076 mil, of which $1,720 is price support. Now suppose that price support was to be eliminated and its total value switched to other instruments, with unchanged output. How would this affect the PSE? Obviously given an unaltered total the numerator of a percentage PSE would remain constant. Since the price support effects in Japan are mainly transmitted into producer prices the 'value of production' would nevertheless presumably drop from the $3,509 mil in the table, by $1,720 mil to $1,789 mil. If total support of $2,076 mil is expressed as a percentage we reach 116 percent. This compares with 59 percent in the table.

Certianly the example is hypothetical, but the principle is that adopted in quantification. Hence the size of the final percentage appears to depend not only on the level of *total* support, but on the way in which it is granted!

Readers who jump to the conclusion that I must be guilty of misunderstanding are asked to consult page 114 of the OECD 1987 publication, cited by Ballenger, in which the same point is made, though with little explanation or justification of the technique adopted. The OECD and USDA methodology, in this respect, is identical. While the problem does not affect the total or the PSE per unit of output for comparative purposes across commodities percentage values are central as an aid to inspection. The adopted convention—one might say the firmly agreed convention—appears defective.

There is an obvious avenue of escape—express the total PSE as a percentage of the value of production as it would have been in the absence of support, but on the post support output. It would then be a clear and unequivocal addition to a revenue base which would not itself vary if support *methods* were altered. There might be objections to this (see Schwartz and Parker) from those who wish to define a percentage PSE as a measure of 'income' loss which would follow from the elimination of support. This is a matter for theoretical debate. In the meantime what trade negotiators appear to need is clarity in presentation of material which is so vital to their deliberations.

From: Nicole S. Ballenger

Economic Research Service and
National Center for Food and Agricultural Policy
Re: The Author Responds

I appreciated Dr. Peters' careful scrutiny of the beef PSEs published as examples in the *CHOICES* article. It is only with such scrutiny that calculation procedures will evolve so as to produce a consistent and useful set of empirical estimates.

Professor Peters is correct in suggesting that quality adjustments and transport costs underlie differences in the reference prices implied by the U.S. and Japanese beef PSEs. Details on producer and reference prices are reported in ERS's forthcoming report, "Estimates of Producer and Consumer Subsidy Equivalents: Government Intervention in Agriculture from 1982 to 1987," edited by Alan J. Webb, Michael Lopez, and Renata Penn. Revisions since the *CHOICES* article was published have resulted in slight changes in the estimates found in this report from those reported in *CHOICES.*

In the case of the U.S. PSE for beef, price support consists of tariffs and, after 1985, beef purchases associated with the dairy herd buyout program. For the 1985 calculation, price support was derived by multiplying the tariff rate ($/ton) by total production. Although a reference price is not required for this procedure, the implied reference price is, as Dr. Peters found, the producer price minus the tariff. The producer price was derived from the average value of beef production per ton of carcass weight, reflecting the many different qualities of beef marketed in the United States.

In the case of Japan, the procedure was very different. The price support component of the Japanese beef PSE consists of tariffs and 'state control' over trade. Deriving this component requires the calculation of a price wedge which captures the price

enhancing effects of both the tariff and implicit import quota, and the identification of a Japanese beef price and beef reference price on which to base the calculation. In the ERS PSE, the full price wedge (including the effect of the tariff and state control) is the difference between (1) the Japanese wholesale price for dairy beef in Tokyo and Osaka markets and (2) the wholesale price for U.S. choice steer beef adjusted for ocean freight costs between the United States and Japan (and converted to yen). The choice of the high-quality U.S. beef price as the starting point reflects the very high quality beef produced in Japan. The Japanese dairy beef price and U.S. choice steer beef price were compared because these two grades of beef are the closest substitutes in terms of quality that the analysts were able to identify. Other Japanese beef is of even higher quality, a fact which shows up in the average producer price reported in the PSE. Thus, in the case of the ERS calculations for Japanese beef, the reference price cannot be directly derived by subtracting the price wedge from the reported average producer price.

It is my understanding that negotiations continue at the OECD over what beef reference prices to use in calculating PSEs. These negotiations may result in changes in OECD and ERS procedures in the future. My article tried to simplify or demystify the PSE, but as Professor Peters has detected, the actual calculation procedures are often far from straightforward.

Professor Peters also questions the choice of denominator used in both the OECD and ERS PSEs. This denominator is the value of production including any direct government payments. ERS uses this denominator largely in order to have a series of numbers comparable to those generated at the OECD. Also, as the original PSE author, Tim Josling, points out, this denominator is one that is commonly understood and is based on reported price and quantity or value of production data. ERS's forthcoming publication and its previous documentation of PSE estimates also report PSEs on a dollar per ton basis. This second method avoids the pitfalls pointed out by Professor Peters (that is, that changing the policy mix can change the percent PSE markedly), but suffers in that it includes no correction for quality differences. Professor Peters' suggestion to "...express the total PSE as a percentage of the value of production as it would have been in the absence of (that country's) support, but on the post support output" is theoretically appealing so long as the appropriate reference (or free trade) prices could be identified for every commodity PSE for every country. As the above discussion on calculating beef reference prices suggests, this is a tall order for even the small country cases. It is somewhat ironic to note that PSE calculations appear to increasingly rely on price wedges and manufactured reference prices, although Josling introduced the PSE partly in order to escape the dependence of nominal protection coefficients on these kinds of price derivations and comparisons.

The Nexus...

From: David A. Atwood
Bamako, Mali (West Africa)
Re: Mellor's "Food, Poverty, Aid, Trade Nexus"
(First Quarter 1989 *CHOICES*)

John Mellor is a welcome addition to the pages of *CHOICES*. His clear vision of the relationship among agricultural incomes, rural employment, and overall growth in economies and trade in the developing world is a compelling one. It is also, in general, solidly supported by a wealth of empirical data. However, an important flaw in the development model discussed in Mellor's recent *CHOICES* article requires some attention from those concerned with the growth of income and employment in Sub-Saharan Africa. That flaw concerns the feasibility of using western food surpluses in the form of food aid for the creation of more rural infrastructure in Africa.

Mellor points to infrastructure development as one of three critical aspects of an employment- and agriculture-based development strategy. In a world where rural infrastructure is extremely costly relative to other development investments, and where a major set of resource transfers from the West (i.e., food aid) is used less efficiently than it might be, Mellor offers an elegant and attractive solution, to use food aid as a way to create the infrastructure which is in such critically short supply.

This use of food aid can work and has worked in South Asia, where large numbers of poor and undernourished people without land are more than willing to work on infrastructure projects for relatively low wages paid in food. However, this will not work in most parts of Sub-Saharan Africa, and in particular in those areas which are poorest in infrastructure. It will not work because Africa's rural poor are not as poor as those in Asia. With few exceptions, while land is becoming less abundant and land tenure is as a consequence becoming more restrictive, poor rural Africans still have access to land to farm. And it tends to be those rural areas least well endowed in infrastructure which have the lowest population densities and hence the fewest problems of access to land.

As an example, there are very few African irrigation projects which succeed in eliciting the labor-intensity they hope for from participating farmers. In the rainy season, farmers tend their upland non-irrigated fields as well as their irrigated ones, at the expense of the higher irrigated yields they would receive with greater labor inputs devoted to irrigation. In the dry season they generally prefer other available income-earning opportunities to the alternative of tending a dry season irrigated crop. So labor availability even for the utilization of irrigation infrastructure, let alone for its construction, is problematical in many areas because farmers' other employment or farming opportunities compete with full use of the irrigation infrastructure.

In addition, in much of West Africa and Southern Africa, the returns to migration (in the West African Sahel, to France or the Coast; in Southern Africa, to the mines) far exceed any conceivable rural "food wage". Such migration opportunities have a major impact on rural wage rates and willingness to participate in agriculture or other activities in the home area.

Food aid could be used for rural works programs in Africa under such circumstances, but only if the "food wage" were set high enough to elicit participation by people who already have access to farmland and to many off-farm employment opportunities. A competitively high food wage, however, would lead to substantial leakages from the program (i.e., sales rather than consumption by participants) and, hence, to competition between food aid and local production on the market, thus creating a disincentive for local food production. The main exception to this generalization is likely to occur during periods of drought and famine risk, when people threatened with destitution will be willing to work for low food wages. It is difficult to create a long-term program of infrastructure development on the basis of happenstance and disaster, though.

In a sense, Mellor's food aid/infrastructure component is "an idea whose time will come" if current stabilization, economic growth, and agricultural development efforts are unable to outpace population growth and its downward pressure on both rural wages and farmland availability. If this occurs, income-earning opportunities and access to land will both have shrunk consider-

ably, lowering available returns to labor, and hence the reservation wage of poor rural people. At some point in this process food-for-work wages will become more attractive to the poorest rural people than their shrinking alternative income-earning opportunities. One valuable service which IFPRI might render is to undertake a research program to identify such situations where they are beginning to exist to the extent required for broad scale food for work infrastructure programs to attract the labor they need.

From: John W. Mellor

IFPRI

Re: The Author Responds

Mr. Atwood's letter is too thoughtful and complex to brush off with a brisk reply. In addition, his case is grounded in correct observation of rural works projects in much of Africa. However, the letter does merit a try at a brief response designed to elicit the appropriate policy response to the problems he raises.

First, to meet him on his own ground of empirical observation, Kenya has been able to do labor-intensive rural works in a context of higher rural incomes and less immiserization than elsewhere in Africa (Mesfin Bezuneh, Brady J. Deaton, and George W. Norton. "Farm Level Impacts of Food-for-Work in Rural Kenya," Virginia Polytechnic Institute and State University, Virginia, 1985, mimeo.). Thus, Mr. Atwood's conclusions do not necessarily follow in Africa. However, the rural population densities in Kenya are high by African standards, decentralization of administration and political system is great and the factors that raise the rate of return to commercialization of agriculture are relatively favorable. To extend the Kenya example requires attention to these three factors.

Second, the case for rural infrastructure is essentially one of its necessity to reduce transaction costs to the level essential to the division of labor, specialization, trade, and commercialization so well described by Adam Smith as the essence of development. Those forces are just as critical in rural Africa as anywhere else. Thus, there is no point in arguing that in Africa infrastructure cannot be built. The need is to figure out what combination of macro policy, agricultural sectoral and subsectoral policy, and political change is necessary to successful provision of this critical element of rural development. Of course, emphasis needs to go first to the relatively higher population density areas within each country.

Mr. Atwood makes an important point about wage rates in rural Africa. Foreign aid induced Dutch disease has drawn labor into urban service sectors and effectively pushed up the opportunity cost of labor in rural areas. But average rural incomes are still low even by Asian standards even though the marginal product of labor is higher than in Asia (Mellor and Ranade, "Technological Change in a Low-Labor Productivity, Land Surplus Economy: The African Development Problem," IFPRI, Washington, DC, unpublished). However, the marginal propensity to spend on food is higher at given, low-income levels in Africa than in Asia (Hazell and Roell, "Rural Growth Linkages: Household Expenditure Patterns in Malaysia and Nigeria," IFPRI, Washington, DC, September 1983). In addition, the positive marginal product of labor in food production should result in a decline in food production where alternative employment is provided. The upshot of those three forces is that labor-intensive road construction financed in part by food aid should be as economically attractive as in Asia, while the food content in laborers consumption expenditures and in lost food output from labor diversion will be higher.

To summarize: rural commercialization and the incident roads are just as crucial to rural development in Africa as elsewhere; labor incomes are such as to justify labor-intensive techniques; and the implicit and explicit food content of road construction will be large. However, the degree of government decentralization, so necessary to road construction and even for maintenance is even less than in Asia; the improved technology essential to a high return to commercialization is less available; and the large areas of very low population density requires clearly defined priorities for geographic ordering of infrastructure investment. Clearly, rural development in Africa is difficult, but there is no reason to abdicate. It is reason for emphasis on training, on policy conditioning for decentralization, and on technical assistance in project design and implementation.

See Irwin and Hanson's discussion of "Farmland: A Good Investment?" on pages 22-23.

How The Land Affordability Model Works

- The model analyzes investment capability—not which farmers plan to buy land. Investment feasibility for 1990 is determined at the start of the year. We ask the question: "Based on first quarter land values, Federal Land Bank interest rates, USDA projected commodity prices, participation in Government commodity programs, and a 25 percent down-payment, could I make scheduled interest payments on a land investment out of earnings."
- Only highly specialized commercial crop farms with sales of $40,000 or more were analyzed. This lets us treat a whole-farm as one enterprise. The approximately 600 farm observations in the USDA data statistically represent about 40,000 corn/soybean and 20,000 wheat farms.
- The integrity of each farm's cost structure was maintained in the data. We assumed that costs per acre on land purchased would be the same as on land now farmed (constant economies of size). But 1990 results were based on 1988 per farm costs plus USDA projected changes in prices of fertilizer, insecticide, fuel, etc.
- Historically, the tendency is to make principal payments from earnings on previously owned land that is less leveraged. Thus we do not include principal repayment, which adds to wealth, in the cashflow mode.

— Gregory Hanson