

DISCUSSION

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The staff of the ISDP should be congratulated for bringing the early fruits of their program to us for discussion. This makes the task of the discussant and the audience a challenging one, as our criticisms can lead to an improved vehicle for the study of the distribution of incomes and well-being in the US. In reviewing the six papers presented, we need to ask, "How well do the methodologies presented address the policy needs for which ISDP was created?" Secondly, we need to ask, "Are there additional tools that ought to be brought to bear on the data to make the information more valuable for policy-making?" I will address both questions, proceeding first to state the policy objectives of ISDP, as I understand them; second to discuss the "DATA" papers - #3, 2, 5 and 1 in that order; and finally to comment on the analytical innovations proposed in papers 4 and 6.

Coder enumerates some of the variables that are collected by ISDP, but did not have the time to discuss the ends for which ISDP was designed. The papers by Lininger and Ycas and Lininger in the previous session reviewed those ends carefully. I will paraphrase those ends by asserting that the objective for a major new survey on households is to measure the well-being of persons in those households and to understand the contribution that Federal income maintenance policies or changes in those policies will have on the well-being of persons. A derivative of this objective is that we need to know who benefits and who fails to benefit from Federal income maintenance programs. We need to know how the dynamics of change--from expected aging in the life cycle, to the hazards of disablement, to the dissolution of households, and to the policy-induced impacts of inflation and deflation--affect the well-being of persons.

From the perspective of this basic objective, the DATA papers indicate some major accomplishments. Household non-response and panel attrition are acceptable. Monthly reports on income give measures of intra-annual variance that have been lacking; they portray a much more meaningful picture of the cash flow to households. Secondly, the detailed questioning in the income area has yielded better coverage of the recipients of Federal transfer payments and interest which have been notoriously under-reported in the CPS. Thirdly the Coder paper documents the value of self-reporting for most items of income information, where proxies are far less likely to be able to supply correct information. This finding, combined with the fact that self-response and proxy response took the same amount of interviewing time makes a strong case for self-reporting. The few items where less information is disclosed by self-reporting may reflect the defensive behavior of persons who have evaded income taxes (Farm sales and expenses) or evaded the termination of Unemployment compensation. In instances where evasion of authority is likely to cause

understatement both self-reports and proxy reports should be obtained to provide some redundancy in the data collection.

On the negative side the DATA papers share in revealing a number of disturbing problems about the ISDP: high rates of item non-response, matching difficulties, and their concomitant -- missing observations in statistics employing the panel features of ISDP. Non-response is a greater problem than the CPS, particularly when the person is considered the unit of analysis. Coder fails to report the proportion of households in which one or more persons are non-interview, by type of interview, which is the datum that one needs to compare the enormous losses in the Feldman, Nelson, Coder analysis of 1978 data with performance in the data collection of the following year. (Kalton indicates person non-person response was 1 1/2-2 times the 1979 level in 1978). Knowledge of those rates is also important to assess the combined effect of non-interview and item non-response on reporting self-reported data (the optimal method given the comparisons of collection methods just commented upon).

Table 1 of Feldman, et al. demonstrates the seriousness of the missing data problem in statistics based on the panel. If jobs occur with the same frequency among persons who were lost from attrition and non matches, then only about 40% of the sample yielded job data that were complete. Such a low rate of complete data requires some explicit modelling of the selection bias in order to give a better picture of how we are to interpret the data available. Additional comparisons of annual earnings extrapolated from persons with 2 and 3 quarters of data would be most helpful.

A second criticism is that Feldman et al. have lost the forest for the trees. There is no reason to believe that the W-2 report includes all earnings for all persons. The existence of the "underground economy" alleged to be 25% of GNP (Feige, 1980) suggests that some persons will have receipts not summarized in formal records. The larger amount of earnings reported in the quarterly estimate from persons who did not use records may be an advantage, and may reflect better short-term reporting of otherwise "hidden" receipts through the ISDP than the annual data available heretofore. Comparison of wage payments with aggregates on a monthly or quarterly basis would be a most helpful perspective (Cf. Coder Table 7).

It would clearly be desirable to replace Feldman, Table 4 with an analysis of variance on the ratio of annual earnings derived from quarterly data to 12-month retrospective recall. This would indicate the joint effects of method of payment, frequency of payment, method of reporting and the use of records which are reported individually in Table 4.

My last comment on Feldman et al. relates to matching. Their difficulties with tracking jobs

clearly underscores the need for redundancy in the information used to match data collected at different points in time (David et al. 1974). A system of computer-generated questionnaires including data reported in the prior quarter would appear to be almost essential to assure that information reported by job at different moments of time can be accurately linked into a time-series.

Olson/Klein give some insight into the causes of the attrition over the panel. The primary finding appears to be that oversampling certain strata in SIE has a very high cost in sample attrition. It is not obvious why that should be the case; the CPS normally incorporates eight interviews into its panel, and many of the SIE respondents had only been interviewed once prior to the ISDP in 1978.

Olson/Klein have presented their data well, but the information collected appears faulty. Pre-coded answers lead the interviewer to a particularly superficial description of the refusal situation. The ISDP should have insisted on content analysis of uncoded interviewer comments on refusals.

Olson/Klein, Table 5 is the most tantalizing. It suggests that perhaps three-quarters of the refusals are interviewed at some prior time. Careful analysis should be undertaken to determine whether subsequent refusal can be predicted from reported characteristics. If so, we may obtain important clues as to how to overcome attrition. Persons known to have high propensities to refuse could be assigned to more skillful interviewers, they might be given special incentives, or they might be included in a modified sample.

Lane/Olson sketch how the ISDP data become relevant for policy-making. Unfortunately their methodology for dealing with missing data is badly conceived. Missing persons and missing households cannot be imputed at the identical values observed at other points in time. Their procedure systematically reduces (a) the ratio of all participants to monthly participants (p. 5) and (b) it tends to understate change in income in Tables 4 and 5. Lane/Olson would be well advised to repeat their analysis excluding imputed persons and households.

The two remaining papers offer an important perspective on the meaning of the financial data collected in the ISDP. Income, after all, is the means to an end -- securing a high quality of life. Lancaster/Vaughn tell us that policy makers ought to be knowledgeable about the psychological implications of an income situation. Ycas/Kasprzyk remind us that no income can compensate for some severe disabilities.

The Lancaster/Vaughn piece is helpful in reminding us of a number of important psychological aspects of income that we tend to forget in the budgeting and cost/effectiveness exercises used in making transfer policy:

(1) Many people have accommodated to low incomes and therefore report modest satisfactions (Table 4.1).

(2) Even in the highest income groups there are some who are not delighted.

(3) The normative content of income depends on the number of dependents supported. These findings would be much more understandable if we knew the within income group variance in the normative scores. (Are the differences in mean scores of neighboring groups significant?) Do motivational differences imply a broad range of norms for one income level?

Lancaster/Vaughn also need to be reminded that others' ideas of the normative value of income are important to the shaping of transfer policy: the majority dictates transfer levels for the minority. For this reason alone a more complete test of van Praeg's IEFs would be desirable -- and as a profession we should damn the OMB for inhibiting a more complete replication of the van Praeg methodology.

Finally I find it comforting that norms on level of living appear to be more closely correlated to the denotation of the scale item than in the case of income (Table 5.1). I would also predict less within group variance in normative assessment for each scale point than the within group variance one observes in income levels of those who give particular norms "delighted" for current income.

Ycas/Kasprzyk do well to reduce a large number of data items to 10 which discriminate degrees of work limitation. Their analysis is straightforward and a useful guide to future questionnaire construction. However, I have lost the forest for the trees. What policy objective is served by the discrimination? If we wish to determine disability as a criterion for making transfer payments 60 items and professional evaluation will be required. If we wish to know the normative judgment of the individual about his capacity to function then it is not obvious that we should limit the domain of interest to work limitation. If we are concerned about the well-being of families it is clear that a composite index of family needs is required. In summary I am perplexed as to the implication of this report for generating better policy-relevant data.

Returning to a view of the papers as a whole I would like to note a major omission. Concern about the well-being of persons in families implies that ISDP needs to make a major commitment in both data collection and analysis to the study of change in the household. What are the concomitants of an individual's move from one living arrangement to another? How is the distribution of well-being affected by family formation and dissolution? What shifts in income sources relate to these upheavals? I realize that analysis of ISDP is in its infancy and these are difficult questions pertaining to small fractions of the sample. However without

intensive exploration or questions related to change in household composition we will be unable to ascertain the adequacy of the questionnaire and sample design for locating persons whose change in living arrangements may be induced by economic causes or may produce serious economic consequences.

David, M. H. et al. (1974), Linkage and Retrieval of Microeconomic Data, Lexington, MA., D.C. Heath.

Feige, E. E. (1980), "The Underground Economy," American Economics Association Proceedings, September, 1980.