

## DISCUSSION

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In view of the impossibility of doing even remote justice to six such complex and diverse papers in the short time remaining, my one hope was to find some common threads among them on which to focus attention. The one thing that seemed to stand out was that this appears to be open season on the poor statistically as well as programmatically. The approach in the Smeeding paper on imputing in-kind income, no matter how it is done, would of course virtually wipe out poverty, as we know it, statistically. Citro-Manser, in their zeal to reconstruct families, would even resurrect the dead if they could thereby push a family above the poverty limit. Both Budd-Yuscavage- and Whiteman reject the notion that a farm or nonfarm proprietor could possibly be losing money. In the first instance, they would have the IRS audit farmers into the black. For the nonfarm group, the sorry shape of their businesses can be implicitly attributed to all of the money they are drawing out on the side. Even many of the poor themselves, according to Goudreau, Oberheu, and Vaughan, deny their status by refusing to report receipt of AFDC benefits. Only Mollie Orshansky continues to fight the good battle but I sense that even her resolve is diminishing to some extent.

### Budd-Yuscavage paper

In the remaining time, I will attempt a few comments on as many of the papers as possible. Whether or not the painstaking and ingenious efforts in the Budd-Yuscavage paper to reconcile the CPS and IRS farm income series really paid off, one must admire the courage of the authors for attempting so prodigious a task. In one year, for example, one of the series was only one-tenth the size of the other. One has to be a bit skeptical, however, of adjustments of the magnitude needed to close the gap between the series. The largest--and the one on which the whole exercise hinges--was an upward adjustment of 40 percent in IRS-statistics on Income (SOI) estimates to correct for underreporting revealed in tax audits. I am not quarreling with the size of the adjustment (it could, in fact, be too small) but rather to the fact that they did not feel a similar adjustment for underreporting was appropriate for the CPS series, although the latter estimates are also markedly below those produced for the National Income Accounts. They appeared to believe that the two series are essentially independent. However, CPS interviewers are instructed to encourage respondents to refer to records, such as those involved in tax returns. Even if this is not highly successful, the annual CPS income inquiry comes at the same time that most people are preparing their tax returns, so that it seems quite likely that some of the same factors would influence both reports.

Another point they make later in the paper also seems to contradict the notion of independence of the series. The authors do not

accept the contention that one of the reasons for the higher CPS estimates is that they are sometimes reported in gross rather than net terms, at least insofar as depreciation and other fixed expenses are concerned. However, it is difficult to accept the idea that CPS respondents give offhand answers (without consulting records) or even report typical or average rather than actual income (as the authors believe) and still take account of so esoteric an item as depreciation, which one would think would only come up when preparing something as detailed as a tax return.

On the whole, I think the authors have undertaken a rather thankless and perhaps impossible task. We might look for even greater heroics if they take on--as they threaten to do--the even more divergent Department of Agriculture series.

### Smeeding paper

In the Smeeding paper, an effort is also made to reconcile two different approaches, which raises about as many questions as it answers. I thought the paper provided a very useful description of the two main approaches for imputing in-kind income--the Pure Microsimulation method and the so-called Survey method-- and of some of their implications. Although the author did not come down clearly in favor of one or the other, one might almost intuitively favor the survey method, whereby identification of recipients of in-kind income is made through questioning of survey respondents and only the monetary value of such receipts is imputed. This would appear to perpetrate the least violence to internal relationships within the micro-units and introduce the least noise in the case of cross-tabulations and multivariate analysis.

Of course, in order to make the most effective use of the Survey approach, a quantum improvement in survey reporting of reciprocity would be necessary. I understand the ISDP-SIPP experimental work has made important strides in this direction. If this progress continues--as we have reason to hope--the matter of imputation of reciprocity would become a minor or nonexistent issue.

Aside from mention of the fact that it is a difficult matter, the author intentionally omits a discussion of the valuation of in-kind benefits. Although it is recognized that the paper could not possibly cover all aspects of the problem, one could gain the impression--which I'm sure was not intended--that valuation is considered a somewhat secondary issue. For reasons given above, as reciprocity reporting is improved, valuation could of course become the whole crux of the matter. I think it would have been useful for the author to have pointed this out, to have mentioned briefly the main problem areas, and to have cited references containing a thorough discussion of these.

### Citro-Manser paper

This paper attempted to cover a wide range

of complex issues--from the whole morass of conceptual theology, to data availability, and methodological approaches--which were possibly too broad for a single, brief presentation. As a result, I had some difficulty in grasping the theme of the paper and the connection between its major parts.

One key aspect which was intentionally omitted was the issue of feasibility and reliability of the data. This matter, of course, could (and undoubtedly will) be the subject of many future papers, but I felt its omission with hardly any mention could imply (as I noted for a parallel issue in the previous paper) that it was regarded as of somewhat secondary importance or could be dealt with as a separable problem. However, it is not really possible to discuss concepts (or methodology) without reference to the issue of feasibility or reliability. No matter how ideal a set of concepts may be theoretically, it is obviously of little value if not reasonably measurable in a survey setting. I think it would have been useful for the authors to have pointed this out and perhaps to have identified those conceptual issues which would be most troublesome from this standpoint.

On a more specific point, I think the authors may have been a little hard on the CPS for its inability to reconstruct families, as though this were its major limitation. I think most analysts will agree that non-response and underreporting are the crucial problems in the CPS income data and that the issue of family composition is of considerably less significance. At one time--and I assume they are still doing this--CPS interviewers, as a check, were instructed to ask for families with little or no reported income, how they managed to cover their living expenses. Responses would range from this matter of loss of a main earner, to use of savings or other assets, or--and this was the main objective--to belated reporting of some income source. I had always felt that if this inquiry were systematized, and the results integrated into the statistics, that we would have a better description of the status of many families than would be provided by reconstruction. If the main earner departed in mid-year and the residual family was now in abject poverty, reconstruction might make it appear that we have a typical lower middle income family, which would not be a very apt description.

#### Whiteman paper

Here I commend the author for recognizing that he had two very different series (on non-farm self employment income) and not attempting to reconcile them. My main quarrel with the paper--and it is really with the data--is the period of reference. The comparison between the conventional net income measure and the alternative measure of the amounts drawn out of the business for living expenses

was made essentially for the first quarter of the calendar year. I realize this was a brief experiment and timing may have been dictated by operational considerations. Nevertheless, the first quarter is probably the least typical period for making the comparison.

For a large proportion of the nonfarm self-employed, the first quarter would be a seasonal low point in terms of net income, having just followed the seasonal high in the period preceding Christmas. The amounts drawn out for living expenses probably do not vary nearly as much seasonally, as they presumably reflect actual needs. Thus, the differences between the two are probably at their maximum during this period, and the comparison could be misleading in terms of describing the typical relationship.

Fortunately, there will be an opportunity for what I believe would be a more valid comparison. In the annual round-up, at the final ISDP-SIPP interview with the panel, information is collected on net income of the nonfarm self-employed during the preceding calendar year. A comparison of these data with the cumulative amounts drawn out during the year should provide a better indication of the relationship between the two concepts and the extent to which they are or are not interchangeable. The annual data are also not subject to the uncertainty of the methodological difference in the first-quarter experiment--whereby the net income data were collected by mail--or the inordinately high nonresponse rate in the mail returns.

#### Goudreau-Oberheu-Vaughan paper

Time only permits me to say that I am strongly in favor of such experiments, whereby cases are drawn from record files and subjected to survey interviews in order to assess reporting differences. In fact, such an approach extended to in-kind benefits and carried out on an adequate scale could assist materially in improving the imputation procedures discussed in the Smeeding paper.

One omission I noted in the paper was the lack of any discussion on the implications of the findings--and perhaps there were none because of the limited nature of the study--for improvements in survey measurements of AFDC income. Presumably, this is a principal reason for the conduct of studies of this kind.

I regret that time does not permit (and for this she should perhaps be thankful) discussion of the Orshansky remarks. She depicted the state of the arts at the time the poverty criteria were developed and the fact that they recognized most of the limitations (for which there were then no solutions) which have since been the basis for most criticisms of the criteria. Hopefully, we can all look forward to improvements in all of the matters discussed in all of these provocative papers, as the SIPP program emerges and progresses.