

DISCUSSION
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The opportunity to comment on these last two papers is tantalizing as they offer the promise of so much more in the way of insight into the income distribution than what we have been able to obtain from existing annual, recall measures. Lane and Linninger offer an invitation to study the monthly data of the ISOP. Their reports of income stability and income changes are fascinating. I hope they will forgive me for concentrating on gaps in their report, which is clearly a model for others to come soon. First it strikes me as peculiar that they offer no insight on the difference in the quality of earnings reported according to recall period. They have three measures of earnings in January, 1979. One comes from persons who were interviewed in February; a second, from those who were interviewed in March; and a third from those who were interviewed in April. What difference does it make? Are there implications for the collection of income data on an annual basis?

Second, in commenting on Table 3 they allude to the possibility that large earnings changes derive from a change in the number of bi-weekly pay periods experienced by earners. Looking at earlier work by Feldman and Coder (1980) shows that in 1977 27% of earners were paid on a bi-weekly basis. It would have been useful for the authors to help us evaluate the variance in earnings change associated with this problem, and indicate how a monthly series of panel data could avoid appearances of variability when the rate of earnings received is constant.

In the same Table the authors deleted families with no earnings in month 1. This makes the table noncomparable to Table 2, and is unfortunate. Some of those with no earnings in month 1 will have earnings at a later point in time.

The statistics that are of greatest interest are those presented in Table 2. They should be compared to statistics that are available from administrative records which can give identical information. I find the numbers suspect as it is hard for me to believe that the Food Stamp program exhibited a net rate of growth of 8.1% over this period. Is it possible that some recall problem leads to bias in the estimates? Is it possible that problems in matching data from the several waves cause an overstatement of the turnover in the program? Answers to these questions are vital, if the data are to be used in analyzing programs of income maintenance.

It would be extremely useful to augment the information presented with another statistic--the ratio of intra-personal variation in the income source to the interpersonal variation. Such a statistic can be computed for the

earnings data from the three monthly reports.

The Czaika-Citro paper is clearly a landmark in the understanding of income distribution. I would like to reinterpret their conceptual framework to offer another explanation for what is going on, and follow that interpretation with comments on the relevance of the difference they find between the CPS revision and the CPS replication.

If we consider the population, the universe, three dimensions encompass the problem that C-C elucidate in their paper: (1) the quality of income measured for individuals, (2) the measurement of consumption costs for individuals, and (3) the aggregation of individuals to form households. Let me explain. Legally income is the property of individuals who are bound to various institutions by explicit and implicit contracts. No matter how those individuals are organized into households, the quality of income measurement will affect the count of persons in poverty and the descriptive statistics of income distribution. At any one point in time the number of individuals whose income should be tallied is fixed, and that number will change as persons age into adult status and die.

Estimating consumption costs for individuals is an area of great debate within the profession, but there is substantial agreement that what is required is to establish a set of equivalency parameters through the estimation of consumer demand equations in a utility-based, multi-commodity choice framework. The difficulties that arise from this approach are that we have little evidence on the economies of scale that may be associated with consumption, and the most obvious candidate for such economies is the consumption of consumer durables and housing. Nonetheless, as Quellbauer (1975) points out it is possible to treat the problem of consumption cost for the individual as if the household in which s/he is embedded alters the price vector for purchasing desired commodities.

The aggregation problem at an instant of time is simply to add the instantaneous rates of income accumulation and the instantaneous cost of consumption across the individuals who elect to form a household calculate the ratio of the first sum to the second and derive an instantaneous estimate of the poverty rate. While it is no problem to aggregate instantaneous rates of receipt of income over time to derive a cumulative annual income experience for one individual, it is conceptually not obvious whether one would wish to measure the poverty experience of the individual as the average of the instantaneous rates (my

preference) or the ratio of the cumulative income to the cumulative cost of living associated with the (several) unit(s) in which the individual may have been embedded over the year.

Still looking at the individual, one should remember that individuals receive transfers from others who live in the same unit. This holds true especially for those who have no receipt of cash income from outside the unit. Thus one aspect of rigorous measurement of income at the individual level is the estimation of intra-household transfers. This is a formidable task, but Morgan and I demonstrated that results of value could come out of an assignment of such transfers (See Morgan et al., 1962).

When viewed in this way, one can see that the value of disaggregating households to individuals is greater clarity in the aggregation of income experience over time, a clear understanding of the costs of consumption associated with the units to which the individual relates and an understanding that the zero cash receipts of some adults bear no relation to their well-being inside of household units.

The alternative that C-C present us with is an alternative of dealing only with household units. This requires an understanding of the complex changes in household organization over time, in addition to births and deaths of individuals; it also entails a need for an arbitrary assignment of continuity to household units that may prejudice the type of statistic that is generated. The advantage of the household approach is that intra-household transfers need not be estimated.

Lastly, one should point out that in addition to the population measurement problems any actualization of a measurement program will impose a design effect on the statistics generated. In the case of ISDP this design effect has two principal parts: (1) non-interview of sampled individuals (over time) and (2) sample bias associated with a failure to follow the sample individuals over time.

Implications for the C-C Approach

As C-C observe (p.5) the amount of income that is missed in CPS because of a failure to report income experience of deceased persons is small. Most likely it is even less than they suggest since a large part of asset-related income transfers to other individuals; and will be reported (correctly) by them for part of the year. There is a potential for full reporting using taxable income reports, if the CPS were to ask questions about person who died during the year.

It is interesting to note that the BLS procedure for reconstructing consumer units as they existed during the year produced a larger difference in mean consumer unit income than the effect identified by C-C:

	1979	1950
CPS replication (3 mos.)		(annual)
Number	80806	32672
Mean	\$18719	\$ 3826
CPS revision		
Number (200's)	80259	31539
Mean	\$18999	\$ 4237

It is hard to judge whether the larger discrepancy between the BLS adjustment and CPS mean income has to do with the longer period for which BLS tried to obtain household years of experience, or with a difference in the construction of household units between the 1950's and the present. This certainly bears further exploration.

Looking at Table 1 and the categories listed on page 27, one can see that groups (b) and (c), the top two rows, are sample design effects. A more realistic picture of the impact of the CPS measurement procedure on the level and distribution of income could be obtained by imputing family composition changes to those units and incorporating them into the remaining categories. One might undertake to obtain some understanding of the effect of following movers by retabulating Table 1 and classifying all households who moved into group b to show what biases are implied by the household sampling principle of CPS. That is, since CPS does not follow any individuals, they would appear as attrition from one wave to the next, and income data are correspondingly biased.

C-C also should have reported on the role that proxy respondents play in these statistics. It is likely that households which split into two sampled households are represented by more informants than households which do not. Since we know that proxy reports of income are biased, one would expect an increase aggregate report of income associated with family splitting, because of a reduction in response error. This source of change should be isolated from the change in income associated with the conceptual structure by which CPS aggregates income experience.

References

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- J. Morgan, M. David, W. Cohen, and H. Brazer. 1961. INCOME AND WELFARE IN THE UNITED STATES (New York: McGraw-Hill)