

DISCUSSION

Paul L. Menchik, Michigan State University

Before making my substantive comments, I would like to commend the authors for their valuable efforts. Very few researchers are studying the distribution of wealth -- many more study income distribution -- and I would encourage them to keep up the good work!

As the authors point out, there are three methods for estimation of wealth distribution:

1. The survey method.
2. The income capitalization approach.
3. The estate multiplier method.

These two papers employ all three of these with the Kennickell/Woodburn (KW) paper employing 1 and 2 and the Johnson/Woodburn (JW) paper using method 3. Research using the estate tax sample is particularly valuable because it is often the case that surveys under-include wealth-holders at the top of the wealth distribution where so much of the wealth is.

One of the important issues in method 3 is use of the appropriate mortality rates, since there is ample evidence that the wealthy have lower death rates than the average. Ideally, the research should seek a mortality rate associated with each level of wealth for estate tax filers. JW use the Metropolitan Life Tables for those with large life insurance policies based upon the firm's experience. One might ask why these rates? Evidence from the Survey of Consumer Finances shows wealth and life insurance value do not correlate very highly -- the top 1/2 percent in wealth have a much smaller percent of life insurance than of net worth.

Second, I wonder why JW use 5-year mortality intervals for their mortality rates. Certainly, 75- and 79-year olds do face different mortality risks and ignoring this injects avoidable error into the estimates.

An alternative strategy would be to use mortality differentials derived from panel data surveys in which people are grouped by wealth and their exit from the panel by death is noted. (My own work with two colleagues shows a continuous decline in death risk with initial wealth [1].)

There are a number of issues related to missing wealth or missing people that should be mentioned. Suppose a couple holds \$1,200,000 in jointly held stock, with all the stock passing to the surviving spouse at the death of the predeceasing spouse. It is plausible that the JW estimate of household wealth would be correct if the estate of the predeceasing spouse was registered with the estate tax authorities. However, since there is no tax liability, what makes us think the estate tax return would ever be filed? Failure to file, I believe, would distort the household wealth imputation.

Asset composition at death may be affected by tax strategies, e.g., the concentrated holding of appreciated assets at death (to take advantage of the basis step-up). These strategies should make the observed asset composition differ from that gleaned from the survey approach. Consequently, since all assets do not appreciate equally, the wealth distribution estimated from method 3 might, with perfect data, differ from the distribution gleaned from method 1. Use of testamentary trusts should lead to missing wealth. Suppose the husband bequeaths \$600,000 to a family trust for

his wife's benefit (then the children's benefit upon her death), and leaves the rest to his wife. On her death, the accumulated wealth in the trust would not be counted in the JW approach even though the wealth in trust offers her no less an income stream than if it was bequeathed directly to her.

It would be interesting to see how the wealth distributions of single as opposed to married households compare across the JW and KW studies. One approach would be for the authors use the KW data for only singles to get percentile cutoffs and amount within each wealth interval. Then they could compare the wealth held among singles in the estate data (at the higher percentiles of course) to compare distributions.

There are some difficulties worth mentioning with the income capitalization approach. Wealthholders with a heavy concentration of assets in certain securities, e. g., municipal bonds, would not be correctly included with other wealthholders. Consider also the case where grandma only owns a big house but has little cash income, perhaps relying solely upon social security. Given the lack of taxable income she might be a high wealthholder but be "too poor to file" an income tax return. Hence, there would be both missing wealth and missing people when using the capitalization approach. I am very impressed by the imputation procedure used in the KW paper. One question I have though is to what extent, if any, is the increase in wealth concentration between 1983 and 1989 attributable to the difference in imputation procedures between the two years?

As a researcher, I am a firm believer in giving users maximal choice. The authors in previous work and Kennickell in 1991, explained the complex procedures used to fill in missing values.

They reveal that it requires 100,000 lines of computer code and two months of processing time to fill in the blanks. It would be most unfortunate if the custodians of the SCF study were only to release survey results with the values imputed by their method. We are told that for each value a corresponding variable will indicate whether this value was the actual value reported or obtained through the imputation method. Thus, we are told that it should be possible to reconstruct the data set as originally obtained from the surveys. The release of the originally obtained responses (as done with previous such surveys) would allow data users a chance to make their own decisions regarding how to deal with missing data and not merely accept the very complex decisions made for them.

Perhaps, we should leave this subject with a healthy respect for the problems inherent in estimating household wealth. On the one hand, in a free society in which we depend on voluntary disclosure from the living, survey data will always be fraught with problems of both unit and item nonresponse. On the other hand, at death, disclosure of wealth data becomes mandatory, but subject to the composition and missing data issues discussed above. It is best to look at wealth from both sides -- for now.

REFERENCE

- [1] Jianakoplos, Nancy, Paul L. Menchik and F. Owen Irvine, "Using Panel Data to Assess the Bias in Cross-Sectional Inferences of Life-Cycle Changes in the Level and Composition of Household Wealth," in *Measurement of Saving, Investment and Wealth*, Robert Lipsey and Helen Tice, editors, University of Chicago Press for NBER, 1989.