

Inclusion in Asset Building: Research and Policy Symposium

Discussant Comments

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George Warren Brown School of Social Work

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My task as a discussant for this conference is as follows (I quote from the directions I received): "Summarize what we learned from the papers, what we didn't learn, and then raise questions for group discussion about what we still don't know, what is important to know, and what the next steps in research might be." This is a big assignment, and it is particularly challenging because I am not an expert in the research areas emphasized at this conference. I am a macroeconomist, and recently I have been thinking a lot about how saving and macroeconomic performance interact. The topics in this conference focus on the microeconomics of saving, in particular how saving and asset accumulation work in the most disadvantaged groups in our economy. Nonetheless, it will hopefully be useful for me to share the perspectives of an "outsider" to this research topic.

I begin with a discussion of the data. Robert Haveman and Edward Wolff analyze the trend and composition of U.S. household assets over the final two decades of the 20th century. Their interesting and provocative statistics show how many American families have very low levels of assets. They define the concept of "asset poverty" as the inability to meet basic needs with assets for more than a short period of time. Depending on the specific measurement method they employ, Haveman and Wolff find that somewhere between 25 and 50 percent of Americans are "asset poor." There are some surprises in these statistics. In particular, they show a trend toward lower asset values in the 1990s when the economy was strong. The overall trend, however, disguises a somewhat richer disaggregated structure. The erosion of asset positions in the 1990s is due to the experience of high-income groups. Lower income groups' asset poverty rates did improve during the strong economic conditions of the past decade. Understanding the source of these differences seems like an interesting research task.

I find these statistics interesting, but I have some doubts about their direct relevance to understanding the asset and saving situations of low to moderate income people in the U.S. The data come from Federal Reserve's Survey of Consumer Finance, which is a cross-section of the entire U.S. population. You could be "asset-poor" with a six-figure income if you spend nearly all of your earnings and do not have assets accumulated from past saving. This phenomenon may indicate the low saving behavior of the American people generally, but it does not necessarily tell us much about the lower income segment of society. Indeed, I do not believe it is all that surprising that a broad cross section of the American population has become more "asset poor" in the 1990s. A consumption boom and associated debt accumulation of households probably played a strong role in fueling good aggregate economic performance from 1992 to the end of the decade. Also, it is possible that greater income security in the latter part of the 1990s reduced household demand for precautionary saving and therefore reduced asset holdings. Clearly there are interesting research issues raised by these findings and there is a lot of attention in public policy circles to both the microeconomic and macroeconomic problems that may arise in a high-debt, low-saving economy. But I believe these problems are largely different from those that are the main focus of this conference.

To address issues of more direct relevance to those emphasized here, it might be useful to use Haveman and Wolffs' methods to study the asset holdings of low-income households. I have little doubt that "asset poverty" rates will be very high in this group, almost certainly higher than rates for the entire population (even if middle and upper class Americans are consuming at very high rates). This kind of analysis could certainly be done with the data sets compiled by a

number of the researchers here. For example, Jeanne Hogarth and Jinkook Lee could certainly do an analysis of net worth among low-income people with their data.

While the asset holdings of low-income households deserves more investigation, I am sure that such an investigation will support the hypothesis underlying most of the discussion at this conference: low-income people have very low asset levels. Low asset levels create serious problems that Michael Sherraden has emphasized in his research for about two decades now. Thus, the main question becomes: what can be done to raise asset levels for lower income people?

Low-income people can accumulate assets through Individual Development Account (IDA) programs. Clearly, knowledge of how the IDA programs work in practice is very relevant for the institutional design of the IDA programs. If program managers know what works to encourage low-income saving they can design incentives and education to help participants adopt successful strategies. Careful research on how IDAs work may also lower the high administrative costs of these programs.

Several papers here examine how this process works. The theoretical summaries in the papers by Amanda Moore et al. and Mark Schreiner et al. are interesting, especially for someone who has studied saving choices from a simple utility maximization perspective. Moore et al. write: "individuals are frequently tempted by immediate consumption ... saving (i.e., postponing consumption) requires self-control that is difficult and costly." Of course, this observation is not consistent with the analytical approach most economists use to analyze consumption and saving decisions. The standard "life-cycle" consumption model implies that individuals choose how they want to allocate their consumption over different time periods subject to the constraint that they pay for their consumption eventually. If people do not have adequate funds at a point in time to finance their consumption plan, they may borrow against future income, as long as their long-term plans make a provision for paying off loans with interest. There is no sense in which individuals have to constrain themselves today to prevent them from taking actions that they will regret tomorrow. Indeed, there is no place for "regret" in the conventional utility-maximizing behavioral foundation upon which the life-cycle model rests.

The idea, however, that the short-term "temptation" of high consumption today poses a barrier to saving for the long term strikes me as realistic. One application that comes to mind from macroeconomics is understanding the strong association between short-term variations in current income and variations in consumption. If households really make true "life-cycle" plans, their consumption should not be particularly sensitive to short-term changes in their income. For example, households would borrow to "smooth" consumption when their income falls due to temporary unemployment, and consumption would not decline by much at all. Extensive evidence rejects this implication of the life-cycle model; consumption appears to be "excessively sensitive" to short-term fluctuations in income. The conventional explanation for this phenomenon in the economic research literature is constraints on the ability of households to borrow. While this "liquidity constraint" hypothesis is probably valid, I doubt that it explains the entire excess sensitivity of consumption to temporary income variations. For example, households reduce consumption in the face of a temporary income decline even when they have access to liquid assets or a line of credit (such as a home equity credit line). The idea presented

in the papers here is that people are somewhat myopic in their consumption and saving decisions. That is, they consume more when they have income available than would be in their long-term interest. This hypothesis is consistent with the excess sensitivity evidence because households that behave this way will have to cut back consumption when income falls, even if that decline is temporary, and they will "splurge" when they receive an income windfall.

Another application of this idea, perhaps more relevant to the main issues of this conference, is that households may find it easier to save out of infrequent lump-sum payments than out of steady income. Households spending habits based on their typical weekly or monthly income may be difficult to change to raise personal saving. But it may be easier for households to constrain themselves from spending all of infrequent payments. (Note that if the life-cycle model applied strictly, spending behavior would not differ out of periodic or lump-sum payments, all that would matter is the present value of lifetime incomes.) This observation suggests an opportunity for low-income households to build assets from annual, and often substantial, rebates from the Earned Income Tax Credit, as analyzed in the papers by Sondra Beverly, Jennifer Tescher, and David Marzahl as well as the work of Timothy Smeeding.

The possible failure of conventional economic models to explain saving behavior also calls into question the appropriate way to analyze the effect of saving incentives. Economic models usually focus on what are called "substitution effects" and "income effects" to determine whether increases in the return on saving stimulates households to save more. A higher return makes saving (which provides future consumption) more attractive than spending today, and therefore gives households the incentive to save more (the substitution effect). Higher returns on saving, however, also raise the total wealth of households which encourages them to spend more in all periods (including the current period). This "income effect" will reduce saving. The net effect is ambiguous. Extensive empirical work has been undertaken to determine the sensitivity of saving to changes in the rate of return, especially the effect of interest rates and tax policies that raise the after-tax return on saving. Most of this research has come to the conclusion that changes in the rate of return on saving have little effect on total saving. This finding would be disappointing for programs such as the Individual Development Account (IDA) initiative. The "matching" features of these programs are equivalent to changes in the rate of return on saving. Therefore, economic research showing that saving is largely insensitive to the rate of return suggests that matching will be ineffective at raising saving. The experience with IDA programs, however, seems somewhat different; there appears to be some positive response of saving to higher matching rates (see Schreiner et al.). Perhaps this result can be explained with theories that go beyond the conventional income and substitution effect analysis. If people have difficulty in constraining their short-run consumption, even though they perceive a long-term benefit from higher saving, a substantial matching rate may provide a kind of focal point for households that causes them to resist the consumption temptation.

I believe there are some interesting research topics along these lines that perhaps should be pursued. If the foundation for saving in the target population for the programs discussed here is more complex than the simple utility maximizing behavior in conventional economic models, we may need to think about incentives in a different kind of way. For example, it may be less the size of the match rate that matters in IDA programs and more of the presence of a substantial match. Such a finding could be very important for using the program resources efficiently.

There might not be much gained in terms of participant savings from raising the match rate above a level high enough to get peoples' attention.

In addition, while I would be interested to see evidence that the income-substitution effect framework of the standard utility maximizing model is too simplistic to understand actual saving behavior, one result in the papers looks like a classic case of the conventional theory in action. Schreiner et al. find that saving increases when the match rate rises from 1:1 to 3:1, but then saving declines beyond a match rate of 3:1. They write "[i]t may be that participants save more with higher match rates, but eventually the match rate is so high that they can meet a fixed savings goal with less net deposits." This kind of result would be exactly what one would expect if substitution effects dominate behavior at low levels of assets, but income effects take over as assets rise.

John Caskey's research offers an additional insight on this issue: if the problem is that people cannot discipline their short-run spending to meet their long-run goals, the problem can be especially severe for the "unbanked." People without regular banking access use alternative sources for financial services, such as check-cashing outlets. Therefore they must use cash to make payments rather than checks, debit cards, etc. But this fact means that their financial resources are always in the most liquid form that Moore et al. find makes spending discipline the most difficult. This observation makes social welfare costs more severe of the absence of good financial services for low-income people. Caskey writes that "the unbanked rarely complained about the expense or inconvenience of obtaining payment services. Rather, they complained mainly about the insecurity and stress associated with living from paycheck to paycheck." This response does not surprise me. I suspect that among all the problems low-income people face in today's society, they would not perceive the absence of a banking relationship as being at the top of the list. But the effects of being "unbanked" on welfare and standards of living may be less direct. The lack of financial services prevents individuals from saving and accumulating assets. The absence of assets means that they will not have a buffer stock to reduce the stress of running short of funds from time to time.

A finding by Hogarth and Lee is interesting in this regard. They find that there is a strong race effect on ownership of a bank account, holding constant income, education, net worth, etc. One possible reason for this result is that the culture and habits of minority social groups lead minorities to favor the use of cash over bank accounts for transaction purposes. (One reason this preference might emerge is the lack of trust in financial institutions, which may be more prevalent among low-income minorities.) Schreiner et al. support this view (in the broad sense) because they recognize how "social norms and interactions shape preferences." They also provide some specific evidence that is relevant to this point; they find that monthly saving is lower for Native Americans and African Americans than for Caucasians and Asian Americans, even after controlling for income and other factors.

It would be interesting to test the implications of these alternative behavioral foundations for saving directly. However, the empirical analyses in the papers here focus less on these "psychological" issues than on the somewhat more mechanical questions of how low-income people adjust their spending behavior to provide resources to participate in IDA programs. The Moore et al. paper suggests that the "case studies" which involve the response to some open-

ended questions provide evidence that people do adopt psychological strategies to artificially constrain current consumption, but the evidence is anecdotal and the quotations provided are very general. I would like to see a more systematic empirical analysis of this interesting issue.

The main focus of empirical work in Moore et al. is to provide evidence on the way people adjust behavior to lower spending and make IDA contributions. The authors find no particularly strong evidence that any subset of the strategies considered dominate others. I note that the strategies considered can be implemented in the short run; changes that can be made in a matter of weeks. Longer run adjustments might also be possible, especially reducing housing costs which are likely to consume a big part of budgets. (The median participant has been in the study for 14 months. Is this long enough to adjust housing choices?) It may be possible to look at a new set of strategies as evidence accumulates over time. One striking result on the factors that motivate saving in IDA programs comes from Schreiner et al. They find a very strong empirical effect of financial education on average monthly deposits. The effect is large and statistically significant. Will this effect persist? As time passes, does this change in behavior continue? Again, this point suggests looking at longer horizons.

The papers in this conference raise many important empirical questions, many of which deserve further research attention. What are the long-run implications of higher saving and asset values for low-income people? Do they accumulate enough in a reasonable time to make a difference in their standards of living? For example, can their saving lead to a home purchase or an improvement in educational achievement for themselves or their children? Smeeding's paper provides an interesting list of things that people receiving an Earned Income Tax Credit say they want to do with saving: improve their housing, buy a car, enhance their education. These responses are encouraging because they suggest preferences for an asset-accumulation strategy if there are funds from which to save. Is it realistic to assume that current programs will provide the means for low-income people to change their lives through asset accumulation? Schreiner et al. provide some specific numbers: Participants in the "American Dream Demonstration" project accumulate \$880 per year (about \$300 from their own saving and \$600 matching). This is not a trivial amount of money if you want to buy a used car. Perhaps it makes a dent in the required down payment for a modest home, if the household can afford to service the mortgage. But this is an expensive program (so far costs have been \$3 for each \$1 of "net deposits" by participants). Is this program making enough of a difference in peoples' lives to justify this level of spending relative to other kinds of programs to assist the poor? We want to know if assets make a difference over a longer horizon. If so, the case for Individual Development Account and Earned Income Tax Credit programs becomes much stronger.

Clearly, longitudinal data would help to answer these questions. Data that tracks many individuals across time provides powerful ways to control for what Schreiner et al. call "unobserved participant heterogeneity." This paper also raises the important question of whether IDA programs induce additional saving or whether they simply encourage eligible participants to reallocate previously existing assets (or saving they would have undertaken without the program) to the IDA to take advantage of the matching funds. (This question is broadly related to the income-substitution effect theory summarized above.) Panel data that includes information on assets outside of the IDA would help to answer this question. It would also be very helpful to have more controlled experiments. For example, could research from the "Extra Credit Saving

Program” (Beverly, Tescher, and Marzahl) be designed to track subsequent saving behavior of both participants and non-participants?

On a related issue, we can ask whether assets enhance individual welfare. This is really the question David Sahn and David Stifel ask in the context of the developing world where asset data may be easier to obtain and more accurate than income or consumption. But the issue is of general relevance. I think it would be interesting to regress measures of welfare and standard of living on both assets and income. For example, one could regress the variance of consumption on income and assets. My hypothesis is that assets could be quite important in reducing consumption variance.

Finally, I applaud and encourage the attention to detailed policy issues in this research program. Often practical policy discussion is confined to a few speculative sentences in the conclusion of a paper. We need careful theoretical and empirical research that focuses on the mechanics and detailed outcome of social policy. Many of the papers in this conference provide this kind of analysis and I believe it is very important.