

The intertemporal allocation of consumption: theory and evidence

A comment

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Attanasio has written an excellent review of the recent literature on the effects of liquidity constraints on intertemporal consumption choice, and has provided some new evidence of his own. I begin my comments by reviewing the very considerable areas in which we agree, going on to some topics where I am not sure whether I agree or not, and concluding with a few points of disagreement. One issue that I shall keep coming back to is why *macroeconomists* should care about whether individual consumers face liquidity constraints.

Attanasio and I agree in general approach. Like him, I think that it is hopeless to address issues of liquidity constraints using aggregate data, especially with models of representative agents. Macroeconomics thrives on shortcuts, especially shortcuts that bypass the finer points of aggregation, but this is simply not the right shortcut. We must try to address aggregate issues by thinking about the behavior of individuals, and by admitting that aggregate behavior will be different from individual behavior writ large. This is hardly a new point nor one that is unfamiliar in the macroeconomics of consumption. The rate of growth effects that are perhaps the most famous implication of the Modigliani-Brumberg life-cycle model are aggregation effects, phenomena that are central to aggregate behavior, but have no direct counterpart in individual behavior. Of course, if we make a full retreat to microeconomic behavior, we must keep the aggregates firmly in mind if we are not to lose the interest of macroeconomics. For this purpose there is much to recommend the cohort approach, working not with data on individual households, but with correctly specified aggregates that are not subject to aggregation bias and that bridge individual and aggregate behavior.

A major difficulty with tracing the macroeconomic determinants of consumption behavior in the United States, as in several other countries, is the

weakness of the data base on consumption. Not only do we lack a panel of households who report total consumption, with the bizarre result that the most important studies on intertemporal allocation are studies of food consumption, studies that by construction are incapable of addressing saving issues, but we even lack a cross-section of households that is large enough and sufficiently error free to allow a match between the NIPA and survey data. The most fundamental difficulty in explaining aggregate saving behavior by a model of individual behavior is that we cannot reconcile data on aggregate saving with the aggregates of data on individual household saving. It should be an urgent priority of data collection in the United States to enlarge and improve the Consumer Expenditure Survey.

An important message of Attanasio's paper is that the presence of liquidity constraints is hard to detect because any behavior attributed to an inability to borrow can be alternatively rationalized by an appropriate re-specification of preferences. And this is clearly Attanasio's own choice, in his other work if less in this paper, to hold fast to the standard theory, to be skeptical about liquidity constraints, and to elaborate preferences to account for the data. In particular, if changes in consumption are correlated with forecastable changes in income, as they are in most data sets, then nonseparability of consumption and leisure can be called to the rescue. If consumption and work are complements, which is plausible enough if work-related consumption is important, then consumption and income changes will be predictably correlated. I have no objection to this general line of argument, and work-related expenses are important even from the most cursory comparison of the expenditure patterns of one- and two-earner households. But just to cite the nonseparability of consumption and leisure is perhaps a little too easy and a little too neatly tailored to the puzzle at hand. I wonder whether the whole story really hangs together. If work patterns explain intertemporal consumption choice, consumption levels should help explain intertemporal labor supply, and there are symmetry restrictions linking the two effects. But my reading of the literature suggests that intertemporal labor-supply models have been even less successful than have been the intertemporal consumption models, and at least in my own work on the British experience, the symmetry conditions were violated. Leisure is a substitute for consumption, but consumption is complementary with leisure. Beyond this, some elaborations of preferences are much harder to take than others. When it becomes necessary to make the intertemporal elasticity of substitution a function of household characteristics, one begins to wonder whether the light is worth the candle.

I believe that there is now quite general acceptance of Attanasio's argument that the examination of Euler equations to test for liquidity constraints is a procedure that has been much less informative than was originally ex-

pected, not only for aggregate data but also on individual data from panels and repeated cross-sections. The presence of liquidity constraints can shift the level and profile of consumption, for example, from a declining lifetime profile financed by borrowing to one in which consumption is trendless, yet these changes may involve arbitrarily infrequent violations of the Euler equation. At the same time, the presence of common macro shocks makes it hazardous to substitute short panel data for longer time-series data, so that, once we also permit the presence of habit formation or of imperfect information, there exist (multiple) plausible explanations for all the macro and micro findings that have often been attributed to liquidity constraints.

What is much more difficult to reconcile with life-cycle theory is the low frequency behavior of consumption, and this is the area in which I disagree most sharply with Attanasio's analysis. It is worth recalling that even the standard versions of the life-cycle story as promulgated by Franco Modigliani rely on liquidity constraints to eliminate unsecured borrowing by young households. Attanasio's simulations suggest that, even with precautionary saving and allowing for household composition, young households are likely to want to borrow. If this is permitted, then at high enough growth rates of per capita income, the average age of dissaving will be lower than the average age of saving, so that further increases in growth will *depress* saving, something that appears not to be supported by the evidence, and is an implication that Modigliani and other authors have sought to avoid. But Attanasio relies on the life-cycle demographic profile of households to explain the tracking of consumption and income over the life cycle, a phenomenon emphasized by Carroll and Summers as evidence against the standard model. It is far from clear, however, that demographic differences across occupational groups do as good a job as does income in tracking consumption, and the demographic story does nothing to address Carroll and Summers' findings that international comparisons of consumption age profiles are radically different from what they ought to be if consumption is determined by life-time resources.

The evidence is consistent with consumption and income being detached from each other over short periods but not over long periods. As often put, there is high frequency smoothing but little or no low frequency smoothing. At high frequencies, consumers can either use the credit market, or can do a good deal of smoothing by accumulating and decumulating buffer stocks of assets. Over long periods of several years or of fractions of a lifetime, there is simply no market for unsecured consumption credit, and income and consumption are closely tied. In terms of an older language in the macroeconomic literature, the consumption *horizon* is a good deal longer than a year, let alone a week or a month, but is considerably less than the half-lifetime left to the average consumer.

Does any of this matter for macroeconomics, and if so, how? Certainly not for the old question of the size of the marginal propensity to consume. Liquidity constraints or not, the short-run propensity to consume is less than the long-run propensity to consume, and nothing in recent research would challenge the sort of magnitudes that have been long established. For other topics, though, the failure of standard life-cycle models is important. If consumption is determined by the discounted present value of lifetime (or eternal lifetime) resources, state-engineered transfers that are present-value-neutral will not affect consumption. Such is not the case in the presence of liquidity constraints or sufficient precautionary saving. If we accept the interpretation of the evidence in my previous paragraph, the analysis of such transfers, whether Ricardian equivalence of more mundane social security and pensions policies, should proceed along more or less Keynesian lines. Even more important is the analysis of growth and its relationship with saving. The recent documentations of failures of the life-cycle model make it extremely hard to believe that cross-country differences in saving rates are the consequence of cross-country differences in growth rates, with the mechanism running through life-cycle saving. That fact makes it much more respectable to explore models in which it is saving that drives growth, whether transitional models of a Solow variety, or endogenous growth models in which the accumulation of saving is not subject to diminishing returns. There is also room for stories in which the direction of causality is as before, with growth causing saving, but with a different mechanism, perhaps habit formation, or perhaps the need to save for housing in countries with heavily restricted markets for mortgages. I suspect that the most profound implications for macroeconomics of recent research on household consumption will ultimately come from the reworking of the analysis of growth.