Review: [untitled]
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Reviewed work(s):
What Money Can't Buy: Family Income and Children's Life Chances by Susan E. Mayer
Source: *Journal of Policy Analysis and Management*, Vol. 17, No. 3 (Summer, 1998), pp. 535-538
Published by: John Wiley & Sons on behalf of Association for Public Policy Analysis and Management
Stable URL: http://www.jstor.org/stable/3325561
Accessed: 06/05/2010 12:55

Book Reviews

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What Money Can't Buy: Family Income and Children's Life Chances by Susan E. Mayer. Cambridge, MA: Harvard University Press, 1997, 256 pp., $35.00 cloth.

It surprises nobody to say that parents are important in guiding children's lives. Having said that, most people also go on to conclude that there is a systematic relationship such that children of parents with higher socioeconomic status do better in many ways than children from lower socioeconomic status families. But what are the implications of that, particularly if one thinks in government policy terms?

Susan Mayer takes this problem head-on. There is an extraordinarily simple logic to her book. Beginning with the common observation that parental income is associated with a variety of child outcomes, she questions whether the observed relationship is a causal one. In other words, if we provided extra money to poor families, would we expect to see the performance of children in these families improve? If yes, there might be a strong case for a variety of transfer programs to the poor. If no, well, what do we do? The answer from her research is simple: No, the observed correlation does not appear to be causal.

The analytical attack on this question is sophisticated. Mayer recognizes a fact that has been oft-repeated but perhaps not taken seriously enough: Correlation does not imply causality. This old adage has been part of the introduction to basic statistics courses throughout the social sciences, even if empirical studies too often ignore it. Particularly within the last decade, increasing attention has gone to the possibility that the underlying causal structure is more complicated than the simple correlations and regressions might suggest. Mayer questions whether the income-outcome relationship so commonly found in the data reflects a true causal relationship.

Journal of Policy Analysis and Management, Vol. 17, No. 3, 535–568 (1998)

Published by John Wiley & Sons, Inc. CCC 0276-8739/98/03535-34

The heart of Mayer's work is a series of new empirical investigations that pursue alternative solutions to this causation problem. The fact is that the data for looking at family effects are frequently far from ideal. Her own empirical work is subject to this. In addition, the solutions to the issues of causality typically rely upon a series of strongly maintained hypotheses about the structure of performance relationships and about the way the data were generated. These maintained hypotheses simply cannot themselves be tested within the analysis, and any particular conclusions from the empirical work will typically hold *only if* these hypotheses are true, but not necessarily otherwise. The uncertainty that these two problems introduce leads Mayer to marshal evidence from a variety of analytical approaches.

The contribution is not, however, so much the novelty of any of the individual empirical forays. Instead, it is the unified approach to the study. The analytical work would make good textbook material for students who are beginning the analysis of natural data. By traveling in a clear and organized manner across a range of underlying theoretical constructs and data analysis strategies, Mayer provides an easily motivated tableau of approaches that could serve as a primer for students being introduced to regression analysis. In the course of investigating a range of outcome variables—standardized achievement test scores, school completion, and teenage childbearing, among others—and a variety of estimation approaches, she demonstrates not only how to handle and manipulate data to focus on specific issues but also how to interpret statistical estimates. Her underlying statistical analyses, while individually open to question, collectively add up to a set of forceful conclusions. The plain fact is that there is considerable consistency across analytical approaches that are not necessarily correlated with each other.

The results of her empirical work are straightforward. There are significant outcome differences between children in poor and richer families. But the combined results of the statistical analysis is that this relationship does not appear to be a causal one.

The implications of her analysis, however, are not straightforward. Almost certainly her results will enter into the "battle of the sound bites." One of the ever-present arguments for expanding various welfare programs has been the "clear and well-documented" adverse effects of poverty on children. For example, AFDC was motivated in part as a way of providing for the minimal material needs of children but in larger part as a way of supporting children's development so that they would have different life outcomes. Those who wish to reduce welfare programs will undoubtedly find Mayer's results to be the kind of research conclusions for which they have been looking. At the same time, Mayer does show that there are important and systematic differences in outcomes across families with different incomes. In other words, just because simple income differences may not be the cause, a substantial problem remains.

The objection that people will have with the analysis is that the true causal factors are not well identified. The evidence points to a set of systematic but unmeasured factors that are correlated with both income and child outcomes, but these are not convincingly defined or measured. This shortcoming leads to inherent difficulties when thinking about how to address the very real problems of child development.

I suspect that it will be a while before the discussion turns more seriously to addressing the problems—as opposed to using the results as convenient debating points. At least a portion of this is natural, because the policy problems are truly hard. The policy implications are also the missing chapter of this book. This is not really a criticism of the book, because it has already done considerable work to clarify the issues and to provide serious research. But when one turns from the empirical work in this book to view the policy implications, one is on his own.

My guess is that it is going to take a long time to digest this overall body of research and to develop sensible policy implications. If I had to speculate, the overall empirical conclusions are likely to be supported and accepted in the relatively near future, but the policy discussion is unlikely to become either focused or helpful for a much longer period. Two key issues are raised by a consideration of the policy issues. First, there is the legitimate problem of adverse child outcomes, but society differs in opinions about the appropriate role of government in addressing this issue. Second, even if there is consensus that government should intervene, there are serious questions about what instruments to use.

Maybe because of my own interests in schools and education policy, I see parallels to the school resource case. In addition, I see the problems of policy development with family as more severe than those with schools.

The school resource case revolves around the same issues as the family resource case. Since the Coleman Report [Coleman et al., 1966], questions have been raised about whether school spending directly influences student outcomes. My own version of "what money can't buy," presented in the inaugural issue of this journal [Hanushek, 1981], had much the same character as Mayer's work. It set out some data and interpretations that suggested little causal impact of school spending, even if there were observed correlations between spending and outcomes. That article also stopped well short of developing the policy implications.

The reactions to conclusions about school resources are similar to those I might expect for conclusions about the effects of family income. First, the school resource conclusions conflicted with the conventional wisdom of the day, so that many people were unconvinced (and many still are). Second, and more important, there was a policy disconnect. Legislatures and, in the case of school spending, courts find spending to be an ideal policy instrument. The policymaker needs only to specify how much to put into the activity, and then local schools (families) can make the detailed allocation decisions. But if schools (families) cannot be trusted to turn money into the child outcomes that we want, what policies should we pursue to deal with the problems we see?

Where, on the other hand, do these policy areas differ? For schools, there is an extensive and well-organized lobbying group that is directly affected by the spending itself, that is, all current school personnel. Moreover, given the broad participation in public schools, a large segment of the population wants to support school quality, which is frequently translated into spending. Although there is a conservative lobbying group that rallies around limiting school spending, its strength and its support by the population face natural limitations that differ noticeably from that of groups lobbying to limit the amount of welfare funding and transfers, spending which directly affects much smaller segments of the population. Even though there is a "welfare lobby," the differences in politics are still likely to make it easier to get agreement on the basic factual conclusions in the family research than in the school research. A second area where the policies differ is that there is much more general support for the idea of public involvement in the schools. With some 90 percent of the population in public elementary and secondary schools, there is little question about a role for government policy and intervention. Again, while some groups would like to lessen the role of government, say, through the use of vouchers, this is not a general argument for keeping government out of schooling. With families, however, there is much more debate about when and how the government should intervene. Although transfers to families are well established, other policies involving more active intervention are subject to much less consensus. Finally, there currently are a number of ideas about how to improve schools by means other than pure resource strategies. Specifically, a variety of policies ranging from merit pay to private contracting to vouchers involve ways of changing the incentive structure in schools so that they might provide better student performance [Hanushek, with others, 1994]. Such a range of policies, even at the conceptual level, is much less well developed when one talks about families. In addition, the possibilities clearly interact with the willingness to intervene in how families conduct their normal activities.

My assessment is that improving the functioning of families—at least given current knowledge and the present form of our underlying social contract—is a much more difficult and uncertain activity than developing policies to improve our schools. Indeed, while the original Coleman Report made the case for the importance of families in school achievement, few have taken this as a serious call for developing family improvement policies rather than school policies. Essentially, there is no agreement from researchers, politicians, or the public on how to improve families from the perspective of improved child outcomes. Some use "bad families" as an excuse for bad student achievement, in the sense of "how can we hope to improve achievement if we don't first improve families?" This in turn frequently becomes a call for larger income redistribution policies to deal with student achievement. I have always interpreted this as either people with a different agenda or people who want to take the heat off of schools. Mayer reinforces the view that improving school performance through simple redistributive policies is neither easy nor obviously effective.

A broader way to interpret Mayer's book is that it provides more evidence about how little we know about some key social issues. This becomes particularly relevant whenever we think about governmental policies that intervene in complicated social relations.

Mayer's book is one that can be read from a number of vantage points. It is also one that will provoke a lot of new thinking about governmental policies, even if it takes some time before this thinking settles down to serious discussion.

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