Editor’s Comment on “What Have We Learned from Cross-National Research on the Causes of Income Inequality?”

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Robert Marsh submitted the paper published in this issue of Comparative Sociology just ten days before his death, so it did not undergo a normal review process. If it had, I would have accepted it as a valuable contribution on an important issue, but would have asked for revisions and additional discussion on some points. This note will discuss what I see as its major contributions, and the issues that I would have asked Robert Marsh to address in a revision. I offer it in the hope that others will follow up on some of the issues that he raised in this paper, the last of his many contributions to comparative sociology.

Journals are interested in novelty, which may take the form of a new “finding” or a challenge to accepted views. As a result, someone reading the research literature will often find it difficult to get a sense of what actually has been established with a reasonable degree of confidence. This is particularly true in an area like this one, where data are relatively scarce and the independent variables are strongly correlated, so that the results of statistical analyses are very sensitive to model specification.

The way to address this problem is to critically review a range of different studies. Meta-analysis has become popular in some areas of social science, but it is not generally possible in comparative sociology. Exact replication is impossible: even if we obtain data from other nations or time periods, we cannot be sure that the underlying process is the same as in the original sample. More generally, it is hard to reduce the differences among studies to distinct variables. Marsh’s approach is to review a range of studies and use his judgment to come to conclusions. Given the depth and breadth of his knowledge of the literature, he was well qualified for this task, and his judgment should be taken seriously. Nevertheless, I would have liked more discussion of how he identified and selected the studies that were considered in this paper.
A strong point of his review is the wide range of hypotheses considered, including work by economists, sociologists, and political scientists. In an address to the American Economic Association, Kuznets (1955, p. 28) noted the need to go across disciplinary boundaries when considering income inequality: “even in this simple initial sketch, findings in the field of demography were used and reference to political aspects of social life were made. Uncomfortable as are such ventures into unfamiliar and perhaps treacherous fields, they can not and should not be avoided. . . . Effective work in this field necessarily calls for a shift from market economics to political and social economy.” Unfortunately, his advice has not been followed: instead, economic and sociological work has drawn farther apart in the sixty years since he wrote. Contemporary economists sometimes consider factors that were traditionally left to sociologists and political scientists, but only if they can be put into the framework of individuals maximizing their utility. For their part, sociologists often simply ignore work from economists, or bring it up only in order to make the easy criticism that “things are more complicated than that.” Marsh, like Kuznets, recognizes that any effort to explain income inequality must draw insights from multiple disciplines.

Another contribution of this paper is providing a succinct account of measurement issues. Inequality is inherently difficult to measure, and there can be pitfalls even when “high quality” data are used. For example, the Gini coefficient calculated by the U.S. Census Bureau shows a sharp increase between 1992 and 1993, from .433 to .454 (United States Bureau of the Census, 2014). The change in this year was as large as the cumulative change in the previous ten years. However, Atkinson (2015, pp. 42–43) observes that in that year there was also a shift from paper-and-pencil surveys to computer-assisted interviews, as well as a substantial increase in the limit for top-coded earnings. Some of the change in the Gini coefficient was merely an artifact of these changes in data collection procedures. Although the Census Bureau noted the changes, it is easy for researchers to download the data and run analyses without considering them. The measurement issues suggest that we should put too much weight on any single finding; rather, we should look for results that are robust.

The major point on which I would have asked for further discussion would have been the interpretation of negative results. As everyone who has taken an introductory statistics class knows, not rejecting the null hypothesis does not mean positive evidence that the effects are zero, or even that they are small. A more subtle point is that using hypothesis tests to decide on the relative importance of different variables is problematic, because standard errors depend on the distribution of the independent variables. Consequently, I would have liked some discussion of the precision of the estimates and to try to distinguish