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UNDERSTANDING INEQUALITY—COMPARATIVELY

Review of *Income Inequality: Economic Disparities and the Middle Class in Affluent Countries*, edited by Janet C. Gornick and Markus Jäntti (Stanford University Press, Stanford, California, 2013)

This book is a valuable addition to the existing literature on economic inequality. Coming relatively soon after the publication of the comprehensive and authoritative *Oxford Handbook of Economic Inequality* (Salverda *et al*., 2009), two major reports on inequality by the OECD (2008, 2011), and the detailed account of recent global shifts in top incomes by Atkinson and Piketty (2010), one might have expected that income distribution research has already been more than adequately served. Not so. As this fine book exemplifies, there are many intriguing questions about the nature, causes, and consequences of economic inequality that remain unresolved and/or unexplored and the complex theoretical and empirical issues confronting distributional analysts raise many new avenues of inquiry that provide fertile ground for future research.

A dominant feature of the rapid growth in income distribution studies over the last five decades has been the emphasis given to empirical examination of existing conditions, past trends and their possible causes, contributors, and consequences. This trend has been facilitated by the increased existence and availability of household income surveys that give researchers the ability to study what has happened, to check the robustness of findings using sensitivity analysis, to link these data to other survey and administrative data, and to explore the associations between income distribution parameters and other economic, social, and political variables. Such surveys, as Tony Atkinson notes in his Foreword to this volume, are under a dual threat represented by their increasing cost and declining response rates in the face of public resistance and privacy concerns (including, in some countries, those associated with data matching). These threats apply not only to the conduct of the surveys themselves, but also to the effort (and cost) involved in making the data available to researchers. Yet without the latter, the case for the former becomes weaker and with it the justification for asking (in some countries requiring) survey respondents to provide the information on which the entire edifice rests.

It is in this context that the Luxembourg Income Study (LIS) has played a pivotal role in showcasing the value of reliable comparative data by highlighting the importance of distributional issues and promoting research on the topic. It has achieved this by focusing attention on the nature of differences *between* countries, but in the process has given impetus to those wishing to better understand why these differences exist and thus to better understand the forces shaping the distributional profile *within* each country. There is ample evidence and argument available to support the value of both approaches in this book, and one striking feature of the four national case studies that appear at the end of the volume is the extent to which the approaches adopted reflect the body of comparative research that has emanated from the LIS project since its inception.

It is now over 30 years since the release of the first scholarly book based on data from the first wave of LIS data covering seven countries in years around 1980 (Smeeding *et al.*, 1990). That volume represented what Atkinson described in the Introduction as "one of the most exciting developments in applied economic research of recent years" and the results reported led two of the architects of LIS—Lee Rainwater and Gaston Schaber—to predict that: "Much more can and will be done with LIS . . . Extension of the LIS concept to comparative longitudinal studies, and to comparative labour force surveys is feasible today . . . we can expect a rich harvest of knowledge to follow from overcoming the boundaries of nations in our analyses of society and economy." As things have turned out, LIS has not evolved in the directions foreseen then, although its growth and influence has probably exceeded the expectations of its initiators who would, I am sure, be delighted to see that "rich harvest" come to such an edifying fruition in a project that Atkinson later described as having "set new standards for all comparative research" (Atkinson, 2004, p. 166).

The two central themes of this book—exemplified in its title—are to document and understand the nature of existing economic inequalities (and how they have evolved) and to examine how these trends have impacted on one specific group: the middle class. These topics are the explicit focus of the first four papers (in Parts I and II) and are followed by (and expanded on) in groups of papers with specific themes: the politics of inequality and redistribution (Part III); the distributional impact of female work, employment, and earnings (Part IV); aspects of wealth distribution (Part V); and four country case studies (Part VI). The papers in the final two parts expand the scope of our understanding of national and comparative distributional issues, while those that precede them provide detailed analyses that increase the depth of our understanding about the shape and determinants of the distribution of income.

Many chapters examine the degree of inequality and how it has changed and it is only possible to highlight a few of the main findings here. The most common pattern, identified by the editors at the outset, is one of increasing inequality between 1980 and 2000 with a slowdown thereafter (up to 2004—the latest year covered here), although there are several exceptions to this general trend. The extent to which changing inequality has been accompanied by growing polarization and a "hollowing out" of the middle class is examined by Alderson and Doran, who conclude that this has happened in both upwards and downwards directions and to a broadly similar extent across countries. This similarity leads the authors to suggest that the causes are likely to reflect a common set of global transnational processes. In an interesting analysis, Lane Kenworthy examines whether there is any association between the increase in top-heavy inequality (identified by the changing income share of the top 1 percent) and the absolute incomes of middle class households (defined as those with incomes at the 25th, 50th, and 75th percentiles). The evidence presented suggests that there is not, the author arguing that this is because the income-reducing impact on median incomes of the growth in top incomes has been offset by the middle-income-increasing effects resulting from the growth in government transfers. This, Kenworthy argues, reflects political choices made by governments rather than (or as an offset to) the constraints imposed by the capturing of a larger slice of growing incomes by those at the top, although how enduring this conclusion is in the face of recent events (the financial crisis) remains open to debate (to which future waves of LIS will undoubtedly contribute).

Although much has been written about the trend toward growing income inequality in recent decades, the results presented (in figure 5.1) by Mahler, Jesuit, and Paradowski provide a sobering perspective on the extent of distributional change. These compare the averaged quintile income shares across 14 high-income countries over the first six waves of LIS, from 1980 to 2004, and show a remarkable degree of stability. Over the quarter century covered—a period when concern over growing inequality has gathered pace internationally—there was a small shift (1.1 percentage point) in the income share of the top quintile and smaller declines in the shares of deciles one to four. The share of the middle three quintiles fell by 0.8 percentage point—hardly enough to signal the end of capitalism or impel citizens to mount the barricades. The income shifts are larger on a pre-government basis, but even here are not massive—a 2.4 percentage point decline in the combined share of quintiles 2 to 4, mainly in quintile 2, which the authors attribute to the decline in manufacturing jobs.

This stability in part reflects the aggregative nature of the analysis and contrasts starkly with other results, most spectacularly with those for Iceland presented by Ólafsson and Kristjánsson, who use national tax data to examine changes in income distribution in the period before and after the financial crisis. Here, what is remarkable is the degree of change experienced. Table 15.1 indicates that between 1996 and 2007, the income shares of the top 20 percent, 10 percent, and 1 percent of Icelanders increased by 14.4, 16.6, and 15.7 percentage points, respectively. The shift toward those at the very top over such a short period is huge and was achieved partly at the expense of others in the top decile and quintile, although the income shares of deciles one to nine all declined over the period. These shifts began to reverse equally rapidly when the financial bubble burst, the share of the top 1 percent declining back to 8.5 percent by 2009, but even so the figures point to a massive and unsustainable economic and social upheaval. Income tax reform between 1995 and 2006 contributed to the growth in inequality by raising the taxes of lower-income groups, although a series of major redistributive tax changes were introduced between 2006 and 2009 following growing public disquiet about the inequitability of several key instruments of redistribution, including tax allowances and mortgage interest rebates. The discussion illustrates not only the important (and diverse) redistributive role of taxes (particularly tax allowances or tax expenditures), but also how rapid changes in inequality can lead to increased public pressure for a greater redistributive role for the state. The relationship between inequality and redistribution can work in both directions and political choices can both shape and reflect what is happening to inequality.

Since it is given special status in the book, how the middle class is identified warrants detailed examination, in terms of both definition and findings. It is unusual to find a book written largely by and clearly aimed primarily at, economists to contain the phrase "middle class" in its title. As Chauvel notes in Chapter 4, the term can be defined in terms of either income (the approach taken by most economists) or occupation (as in much of the sociological literature). However, the literatures generated by these alternative approaches have addressed different issues and rarely intersect, providing an opportunity to redress this shortcoming and draw together the relevant themes into a more consistent and coherent approach. This volume takes some steps in that direction, but leaves unresolved how *the* middle class can be defined, or whether a single definition has any meaning or value. The emergence of the Luxembourg Wealth Study (LWS) has generated data that allow a definition that encompasses income as well as (net) assets to be developed (as is done by Atkinson and Brandolini in Chapter 2) but the limited occupational information in LIS and LWS prevents a more comprehensive application of the sociological approach. As the editors note in their concluding chapter (p. 492): "What is needed is a more multi-dimensional portrait of the middle class, based on middle-class definitions that draw on indicators of well-being other than income." This seems a rather tame conclusion to reach after 500 pages of detailed analysis of the topic!

Since most of the contributors are economists, it is no surprise to discover that most define the middle class in terms of income, although even here there are differences of approach. Some authors focus on (changes in) the income shares of those in the middle deciles of the income distribution, while others look at (changes in) how many have incomes that fall within the middle of the distribution—expressed as percentages of median income. Most of those who adopt the first approach focus on the shares of the middle three quintiles, while those who adopt the latter approach focus mainly on incomes between 75 percent and 125 percent of the median. However, not all authors use these definitions, as the editors note in their Introduction (where they also note that they did not impose a common definition when commissioning the chapters). Thus, for example, Bradbury examines separately the middle three income quintiles of the distributions of prime-age (45–59) and old (65+) individuals, Estévez-Abe and Hethey-Maier define "egalitarian households" as those where wives contribute between 30 and 70 percent of family earnings, and Frick and Grabka focus on those with incomes between 70 and 150 percent of the median.

These are minor differences but as Atkinson and Brandolini point out, they can lead to different results and thus make it harder to discern the overall picture that emerges from the different approaches taken in different chapters. Their analysis shows that the dominant pattern is one of a decline in the income share of the middle three income quintiles and a fall in the numbers with "middle class incomes" defined as falling between 75 and 125 percent of the median. They argue that both definitions are narrow, arbitrary, and hence problematic and examine several interesting alternative approaches. One of these differentiates between the middle class and the "rich," defined using either a much higher income cut-off (possibly as high as 300 percent of the median), or by calculating who can afford to employ a full-time worker to provide domestic services, or by estimating who has enough wealth to live comfortably off the interest income alone while avoiding employment. Operationalizing these approaches involves making assumptions (about the nature of the services provided by others or the level of interest-supported standard of living attained) but the approach opens up new avenues of enquiry that warrant deeper examination. They also use the (limited and nation-specific) LIS occupational variables to develop a social stratification classification for five countries and show that this approach produces a middle class that varies less in size across countries than an income-based approach and, more interestingly, that the two classifications do not coincide. They end by arguing that there is a need to reintegrate analyses of income, wealth, and occupation, reinforcing the editors' call for closer integration of the economic and sociological literatures.

One small step in this direction is provided by Vanneman and Dubey in their analysis of income inequality in India—a country where the vertical disparities are so great as to require that inequality at the top is best analyzed separately from inequality at the bottom. In these circumstances, the definition of middle class is of critical importance and although the authors use an income cut-off approach (between 50 and 200 percent of the median) they emphasize that this has little global meaning in relation to affluent countries because absolute incomes are so low in India. For most middle-income households defined in this way, the primary source of income is non-agricultural wages and this suggests a definition that could be based on the *source* of income rather than its (relative) level. Although not examined in the paper, the idea is worthy of further examination in the low- and middle-income countries that are slowly being added to the LIS database. The general discussion in this chapter (and in that on South Africa by Leibbrandt, Finn, and Woolard) highlights the need to adopt inequality measures that reflect the nature of inequality that exists, as well as the broader economic and social context within which it sits.

The earlier remarks about the public responses to growing inequality lead naturally to the two chapters in Part III on the role of politics and public opinion in shaping inequality. Mahler and colleagues use regression analysis to examine the relationship between a range of variables that capture political participation and redistribution. They find that the partisan composition of government has no impact on net redistribution toward or away from any income quintile, although the overall results for quintile 4 differ from those for quintiles 2 and 3 to a degree which suggests that "the middle class is not of one piece" (p. 157)—echoing a theme identified in several other chapters. (Although it is, of course, only a small step from a three-way classification into poor, middle, and rich households, to the less emotive use of five quintiles that is commonly used to describe the income distribution.) The authors then examine quintile differences in voter turn-out using data from two recent surveys—the Comparative Study of Electoral Systems and the European Social Survey—and show that those who place themselves in higher quintiles have higher turn-out rates (figure 5.2). These results should be treated with caution because it is well-known that people's perceptions of where they sit in the income distribution are highly skewed toward the middle (as noted in the following chapter, but see also Osberg, 2010), making it difficult to conclude from these results anything about how turn-out varies with actual distributional position. Even so, the authors do show using LIS data that there is a tendency for greater political participation by some groups in ways that promote redistribution from which those groups benefit. Although hardly surprising, the support for even this proposition is weak—weaker than the authors had anticipated—and this is another area calling out for better data and more research.

The role of inequality perceptions in affecting redistributive preferences is examined in the chapter by Tóth and Keller, who use data from the 2009 Special Eurobarometer survey on poverty and social exclusion to construct a Redistributive Preference Index. The Index is derived as the first principal component of five variables that measure attitudes to the prevailing distribution of wealth, the desirability of state intervention to support jobs, education, and social goals, and general attitudes toward the role of the state. The Index is interpreted as reflecting attitudes to *redistribution*, although it more accurately captures attitudes to state *intervention* and the two may not move together: one can favor greater state involvement while being opposed to more redistribution. The Index is generally high and variable across European countries, but regression analysis shows that in countries with more inequality, there is more support for redistribution. Although there are concerns about how to interpret the Index developed, it is difficult to disagree with the authors' argument that more needs to be known about the nature of people's preferences for inequality and redistribution before we can begin to understand more about the social and political impacts of changes in inequality.

The chapter by Susan Harkness uses LIS data to examine the impact of women's earnings on inequality between households—a topic that has attracted considerable attention but, as the author notes, has produced some disparate conclusions. Harkness uses a decomposition approach to examine the impact of female employment on earnings inequality among heterosexual couples where both partners are aged 18–59 across 17 countries using LIS wave VI data (around 2004). Three counterfactuals are specified and examined: where no women are employed; where all women are employed; and where the gender pay gap is eliminated. The first two are shown to imply that wives' earnings have a clear equalizing impact in all countries, as does the third in all but two countries (Denmark and Finland), although the size of the impact is much smaller. Folbre, Gornick, Connolly, and Munzi estimate the different, but nonetheless important impact of including an estimate of the value of women's unpaid domestic labor in earnings. The method involves valuing the hours devoted to housework and child care using a market value approach based on the minimum wage in each country. The analysis applies to married or co-habiting couple-only households aged 25–59 and involves merging data from the Harmonized European Time Use Survey with LIS. They find that including the value of unpaid work almost doubles wives' contributions to household earnings but has little effect on inequality (measured, like Harkness using one-half of the squared coefficient of variation). This leads the authors to conclude that their analysis "clearly demonstrates the substantial equalizing effect of unpaid work on economic well-being" in high-income countries, but they also note that more attention should be paid to taking account of the reductions in living standards that flow from the reduced time devoted to domestic work that is a direct consequence of the increased earnings associated with higher female employment rates and earnings. Similar issues are the focus of the chapter by Estévez-Abe and Hethey-Maier, although the precise measures sometimes differ in ways that add avoidable confusion (compare figures 7.1 and 9.1 which differ in how couples are divided into male breadwinner, female breadwinner, and a third category). They examine country differences in the percentage female contribution to family earnings, where female earnings include an estimate of maternity leave and parental benefits—a definition which has a marked impact on the findings. The main result is that the generosity of paid leave benefits improves women's economic position relative to their husbands, although the impact of child care provision is not statistically significant. Small sample size and the complexity of the institutional variables included in the analysis lead the authors to describe their results (appropriately) as "tentative" (p. 278)—once again highlighting the need for further research.

Sierminska, Smeeding, and Allegrezza use data from LWS to examine the distribution and composition of wealth holdings and how it varies across the bottom, middle, and top of the income distribution (defined in terms of quintiles, as described earlier). Wealth is defined comprehensively to include financial assets, owned dwellings, other real estate and business assets, with all outstanding debt deducted. Pension wealth that has not been converted into an annuity or converted to financial assets is not included. The results highlight the importance of home ownership in the wealth portfolios of all households, particularly the middle (income) classes. (This issue is taken up later by Bradbury in an interesting analysis of the role of home ownership in Australian retirement incomes policy, while Frick and Grabka examine the role of pension wealth in Germany.) Single parents fare badly in all countries, with low rates of home ownership and high levels of outstanding debt combining to make their appearance in the upper deciles of the wealth distribution a rarity. The results are new and important and they are given added value by the chapter by Jäntti, Sierminska, and Van Kerm, which examines the joint distributions of income and wealth. There are obvious opportunities to exploit the wealth data to define the middle classes using the kinds of approach identified by Atkinson and Brandolini that hopefully will be taken up in future work.

Mention has already been made of some elements of the four country case studies contained in Part VI. One of the most interesting aspects of the analyses contained in these chapters is the degree to which they have been influenced by the frameworks and practices that have evolved over the last four decades by researchers associated with the LIS project. This highlights the important role that LIS has played, not only in harmonizing the data and making it available, but also as an international hub (reinforced by the regular LIS training workshops, a strong LIS presence in the IARIW conferences, and the use of LIS by leading international agencies like the OECD and the World Bank) around which leading international scholars have gathered and interacted. Few of today's leading distributional scholars have not been associated with the LIS workshops, as either teachers or students. But these national studies also highlight one of the inevitable drawbacks of comparative research, which is that the search for cross-national comparability inevitably involves some loss of national specificity. An example from the chapter on Iceland has already drawn attention to this issue, but this is equally true of the other three chapters included here. McKenzie, for example, provides a compelling account of the difficulties facing distributional researchers in Japan in accessing data (yes, this is not just an issue for LIS!). He provides a valuable summary of Gini coefficients derived from the main Japanese data sources (table 14.1) and locates Japan within the LIS inequality hierarchy before discussing income mobility and the distribution of wealth. The large swings in inequality in Iceland in the run up to, and immediate aftermath of, the financial crisis have already been mentioned, as has the extreme vertical inequalities that characterize the income distribution in India. In post-Apartheid South Africa—described by Leibbrandt, Finn, and Woolard (p. 459) as "one of the most unequal countries in the world"—no account of inequality can ignore the severe racial divides that still exist. However, as the authors show, inequality within each racial group is generally a larger contributor to overall inequality than between-group inequality. This pattern is particularly true of within-African inequality which has played an increasingly influential role in driving overall inequality. The concept of a middle class also becomes problematic (as in India) when the distribution is so skewed in favor of those at the top, although the authors provide an account of the changing fortunes of those in income quintiles 2 to 4 that includes an analysis of how different factors (race, education, location) affect the probability of belonging to the middle class.

This is an excellent book that is highly recommended to those with an interest in all aspects of income distribution in contemporary societies—affluent as well as middle- and low-income. The chapters are of uniform high quality and most importantly, they fit together well to form a coherent volume that is easy to read and draw out common themes and implications that span different chapters (unlike the disparate nature of many edited collections). The editors have added enormously to its value by providing an excellent Introduction and a Conclusion that draws the main threads together and identifies important issues where more (or different) research is needed. Their role in requiring all authors to comply with four core requirements (see pp. 11–12) before commissioning them was no doubt a factor in producing the coherence of the final product—even accepting the different definitions of middle class adopted in different chapters. The editors also need to be congratulated for broadening the focus beyond a purely economic (and economists') perspective on the issues under examination. Although the steps taken toward better integration of an economic approach with relevant insights from the sociological and political science literatures on inequality are tentative, the foundation has been laid for a new approach that draws together contributions from across the social sciences. LIS cannot be expected to support such an endeavor on its own but as several contributions to this volume illustrate, there is untapped potential to merge it with other datasets to better address specific distributional issues. This might result in some revision to the existing LIS template but this would be worthwhile if it allows a broadening of the kinds of analysis that it can support.

As for the future of LIS itself, there are a number of difficult challenges that need to be addressed. One relates to the time lag between when a national survey is conducted and when the harmonized data become available to researchers. There are limits to how much this delay can be reduced given the technical requirements and sheer complexity of the harmonization process—unless the level of funding is increased substantially. The OECD is better placed to draw on national experts and informants to generate comparative tables that conform to a common template within a much shorter timeframe. But this is largely possible because there is no need to produce the documentation that is a necessary part of any public dataset, like LIS. The fact that the OECD data are not available in microdata (or any) form to researchers outside the organization makes it a complement to LIS, not a substitute, and there is no reason why the growth of OECD work in the field should pose a threat to LIS—as long as the complementarities are acknowledged and reinforced from both sides.

One unrecognized contribution of LIS has been to draw attention to the quality, accessibility, and consistency of national income distribution data, and it has played a lead role in the establishment and work of the Canberra Group that is helping to improve the comparability of national data (see United Nations Economic Commission for Europe, 2012). Ironically, if these efforts were to be completely successful, much of the rationale for LIS would disappear and along with it the support that national statistical agencies currently provide. Of greater concern is the longer-term financial viability of LIS. It currently relies primarily on annual country contributions that are coming under increasing scrutiny as national fiscal conditions tighten. Investment in research capacity and data quality is an easy target when funds are limited and targets are focused on achieving short-term, measurable outputs. The overall level of funding that is currently provided does not fund all of LIS's operations; it receives a heavy implicit subsidy through the numerous unpaid contributions made by its academic leaders (previously Lee Rainwater and Tim Smeeding, currently Janet Gornick and Markus Jäntti) and by a large network of national contacts (many of whom are members of the LIS Advisory Board) who work behind the scenes to ensure that things get done, that national know-how is imparted, and (most importantly) that the funds get transferred. These mechanisms are becoming increasingly difficult to sustain in today's climate of high staffing mobility, limited research funding, and performance-related accountability. The LIS database is essentially a public good with all the risks that that entails, and while it would be possible to introduce a system of user charges, this would defeat the vision that lies behind its inception and development. Long may it flourish.

Peter Saunders
Social Policy Research Centre, University of New South Wales,
Sydney, New South Wales, Australia
(p.saunders@unsw.edu.au)

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