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A Century of Wealth in America

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American inequality in the long run

Unequal Gains: American Growth and Inequality since 1700, by **Peter H. Lindert** and **Jeffrey G. Williamson**. Princeton, New Jersey: Princeton University Press, 2016. 424 pp. $22.95 paper. ISBN: 9781400880348

A Century of Wealth in America, by **Edward N. Wolff**. Cambridge, Massachusetts: Harvard University Press, 2017. 888 pp. $39.95 cloth. ISBN: 9780674495142

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According to the famous thesis of Thomas Piketty's *Capital in the 21st Century*, "When the rate of return on capital exceeds the rate of growth of output and income, as it did in the nineteenth century and seems quite likely to do again in the twenty-first, capitalism automatically generates arbitrary and unsustainable inequalities that radically undermine the meritocratic values on which democratic societies are based" (2014, p. 1). Inequality undermines democracy, and inequality grows whenever $r > g$.

Can this theory explain why inequality is growing in the United States? Piketty asserted that his theory was best tested with data from France, whose history was, he argued, "more typical and more pertinent for understanding the future" than the historical experience of the United States (p. 29). Nevertheless, and no doubt because *Capital in the 21st Century* sold so many copies, some university publishers in recent years have been willing to gamble on big, dry books of historical inequality statistics that purport to test his arguments against American data. A perhaps extreme example is Edward Wolff's *A Century of Wealth In America*, which weighs in at almost three and a half pounds. I suspect that the "century" in the title is a nod to Piketty; in any case, it is not a very accurate description of the contents of the book, which, apart from a few data points presented in chapters 12 and 13, is mainly concerned with the last 40 or so years of wealth in America. That is the period for which consistent, high-quality survey data on household wealth are available, and the bulk of the book is spent reporting analyses of those survey data.

Wolff is a thoroughgoing empiricist of the old school. The practice of social science, for him, consists of accumulating facts and then making general statements about them. He opens the book with an encomium to "the Baconian method of inductive reasoning from empirical observation" (p. xi), and the book that follows is about as inductive as they come, presenting the reader with hundreds of pages of facts before finally announcing its argument on p. 679: "The thesis of this book could be summarized as the rise and fall of the middle class." That is not actually a thesis, it is a noun phrase; but, to be fair, this is a hard book to summarize. Much of it updates work that Wolff published over a period of decades.

It is a treasure trove of description. We learn, for example, that adults in the top 1% of households by wealth are disproportionately old, white, married, highly educated, and healthy (pp. 440-441, 443). Since 1983, they are increasingly likely to be self-employed. They are overrepresented in industries that fall within the umbrella category of “Finance, insurance, real estate, and business and repair services,” though not as massively overrepresented within that category in 2013 as they were in 1992; like everyone else, the wealthiest 1% have lately gravitated to industries that can be classified under the general umbrella of “Transportation, communications, utilities, personal services, and professional services” (pp. 441-442). All of this is, perhaps, as you might have guessed; but being unsurprised by something is not the same as knowing it, and if you actually knew any of these facts already, it is probably because you once read a study by Wolff, or by someone else who cited him.

We also learn from this book how the patterns of wealth accumulation over the life course differ among social groups. Wolff presents regression analyses to summarize how the age profile of accumulation differs by race, education, urban or rural residency, and asset class; in order to summarize the data, he ultimately argues for the heuristic value of a “three-class model,” consisting of capitalists who hold financial assets and invest in order to “build up large estates” for their heirs, a middle class that saves from labor earnings and dis-saves in retirement, and “the poor,” who do not have enough earnings to accumulate “any significant accumulation, except in the form of durables and perhaps housing” (p. 248). Because the wealthiest capitalists are investing in different assets from the middle class, they enjoy higher rates of return, and can entertain longer-term goals for wealth accumulation. The rich really are different from you and me.

Wolff ventures few causal inferences, and these are mostly negative inferences, or judgments in favor of one or another null hypothesis. He finds no evidence that the increasing concentration of wealth in our lifetimes results from dynastic inheritance: wealth transfers as a share of household net worth have exhibited no particular trend since 1989 (p. 305). He also finds no evidence that inequality grows automatically whenever the rate of return on invested capital exceeds the growth rate of output. In place of "Piketty's law," Wolff proposes what he calls a "rule of thumb: that wealth inequality tends to decline when the return on wealth for the middle class is greater than that of the rich and, conversely, increases when the opposite is the case" (p. 210). This provisional rule of thumb is extrapolated from only five data points covering the period after 1983, but it seems believable; indeed, if it is hedged with a suitable *ceteris paribus* clause covering the propensity to save, I think it is a truism. Rather than an explanatory law, this "rule" is a useful heuristic for focusing our theoretical attention on events that affected the relative rates of return enjoyed by the rich and the middle class. That is how Wolff accounts for the arc that he describes as the "thesis" of his book, the rise and fall of middle class wealth in the last century. With the emergence of mass homeownership and the spread of defined benefit pensions after the Second World War, middle class people had access to greater wealth, and greater returns on investment, than they had previously enjoyed. Wealth inequality fell. Then the retreat from defined benefit pensions beginning in the 1980s, the dramatic increase in household debt after 2000, and the depreciation of housing in the Great Recession, together undid much of that middle class accumulation. Rates of return on the kinds of assets held by the middle class turned negative. The net worth of the median household was lower by 2013 than it had been at any time in the previous fifty years (p. 50). Wealth inequality was correspondingly higher.

So much for wealth inequality. What of income inequality? Peter Lindert and Jeffrey Williamson trace the movements of income inequality in *Unequal Gains: American Growth and Inequality since 1700*. By comparison to Wolff's tome, this book seems like beach reading; it is available in paperback, and it weighs in at a comparatively modest one and a half pounds—as long as you don't print out the 80 spreadsheets of supplemental data and computations available online. It is nonetheless a densely argued book that aims at central questions of American economic history: "How and when did Americans become so prosperous and so unequal?" (p. 1).

To give definite answers to questions like these about such a long span of history, most of it before the collection of official economic statistics, requires heroic feats of data collection, and maybe also heroic faith in the quality of those data. Lindert and Williamson are heroes in the mold of Sherlock Holmes. They inspect each clue and sift the evidence. No detail seems too trivial to occupy their attention. And then all at once they bravely hazard a guess, which they report to the third decimal place.

Their key methodological device is the "social table," which is an occupational frequency distribution table much like those that have comprised the raw material for generations of sociological class analyses. To derive a Gini coefficient of inequality of household income for the thirteen British colonies in 1774, for example, they first divide the colonies into three regions, and then subdivide into big city, small town, and rural populations; within each population, they estimate the numbers of adults in each of 19 occupational groups, from "officials, titled, professional" through "merchants and shopkeepers" and "male and female menial laborers" (p. 17) to "slaves age ten and up" (p. 22), then assign an average labor income and an average property income to each group; aggregate those personal incomes to households; and convert the data from different colonies into a common currency by applying the available data on exchange rates. Finally, they compute a Gini coefficient from the grouped data. It is 0.441 (see p. 38).

To piece together this picture requires a patchwork of sources stitched together with countless little judgment calls. Consider one of the occupational and regional types they offer to illustrate their method: "A New England small-town shopkeeper" (p. 265). To estimate the incomes of self-employed shopkeepers for all of New England, they begin with wealth data inferred from the few New England shopkeepers in Alice Hanson Jones's multi-stage probability sample of colonial probate inventories. They apply an assumed 6% rate of return, and add self-employed labor income, which they impute from the labor earnings of artisans and "officials, titled, and professionals" (for which occupational categories fragmentary records exist) by linear interpolation, on the assumption that ratios of average labor earnings by occupation correspond to the ratios of average wealth by occupation that could be inferred from probate records (p. 270). They estimate the number of small-town shopkeepers for all of New England in 1774 by extrapolating from the occupational shares of the labor force reported in a study of Lancaster, Pennsylvania (NB, not in New England) circa 1800 (pp. 18, 266); the total labor force is estimated by applying state-specific labor force participation rates, which were backcast from late 19th century Census data by Thomas Weiss, to 18th-century population figures from colonial census reports. Lindert and Williamson present their final point estimates without error bars only because most of their data for the colonial period are not probability samples. That does not mean that the point estimates are known exactly. To the contrary: they are, basically, well-educated guesses.

The long-run story that Lindert and Williamson tell on the basis of judgments like these is that American income inequality has had no particular secular tendency. Its history is a trendless fluctuation. Income inequality increased gradually from 1774 to 1860, a period during which, "contrary to Piketty's emphasis on the positive correlation between the rate of return and inequality" (p. 138), the rate of return on invested capital declined as the economy grew. The Civil War equalized incomes in the South but accelerated their concentration in the North (p. 153). From the Civil War until the Great Depression, an increasing concentration of wealth and capital income at the very top of the distribution was counteracted by greater equality of incomes below the 99th percentile; the income share of "the top one percent" increased even as the Gini coefficient over all households fell (p. 173). The "Great Leveling" (p. 194) of the following 40 years appears, in their narrative retelling, as a temporary dip in inequality, attributable to changes in the labor supply supplemented by regulation of the financial sector and an increase in fiscal redistribution. And the rise of inequality after 1970 resulted from a "perfect storm" of factors including deregulation, skill-biased technological change, increasing international competition, and slowing growth of the skilled labor supply (p. 240). Changes in fiscal redistribution affected the post-fisc income shares of the one percent, but were only a small part of the total change in inequality of incomes below the top centile.

What these two books conclude in common is this: the movements of wealth and income inequality in American history cannot be explained by any automatic tendency of capitalism. Sometimes inequality is up, sometimes it is down, and sometimes, in an economy as large as the United States, it is simultaneously trending up in some places or some respects, and down in others. We can identify causes of these movements, but those causes are historical and political. If you abstract from politics entirely and look for the dynamics of inequality in some supposedly fundamental law of capitalism (such as "inequality increases when $r > g$"), you will be disappointed.

That conclusion probably will seem congenial to many sociologists; I, for one, am willing to say I believe it. But it only carries us so far. One assumption of Piketty's *Capital in the 21st Century* was that analysts in pursuit of a *long-run* theory of inequality could afford to bracket political institutions, because, in the sufficiently long run, the politics of redistribution would be endogenous to the degree of inequality: if any society lets a few families get rich enough, those families eventually will be able to bend the political system to their will, and buy off, basically, anyone who would challenge them. If we reject that assumption—or at least, if we regard it as an insufficient explanation for fluctuations in the degree of inequality within the range that historical social science has observed hitherto—then it seems fair to ask what alternative assumptions we would put in its place. Grant that the ratio of *r* to *g* is not a satisfying explanation for why inequality now rises, now falls. What, then, *does* explain those movements? Lindert and Williamson conclude their book by suggesting that a lot of it comes down to political will: we could a more equal economy today, if only we wanted it. "Improving education, taxing large inheritances, and taming financial instability with regulatory vigilance—the opportunities are there, like hundred dollar bills lying on the sidewalk," they write. "Of course the fact that they are still lying there testifies to the political difficulty of bending over to pick them up" (p. 262). Their book doesn't have anything to say about what that "political difficulty" is, where it comes from, why it waxes and wanes, or why it might differ from here to there.

For answers to questions like these, we need a political sociology of inequality, and, in particular, more comparative historical studies of education, taxation, and regulation, to fill in our understanding of American inequality in the long run.

Reference

Piketty, Thomas. 2014. *Capital in the Twenty-First Century*, translated by Arthur Goldhammer. Cambridge, Massachusetts and London, England: The Belknap Press of Harvard University Press.