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ABSTRACT

Thomas Piketty's monumental *Capital in the Twenty-First Century* has transported us to a higher understanding of historical movements in inequality. This essay ranks the promise of different paths that scholars can usefully follow from the point to which his book has guided us. The main path to follow is the income inequality history so well paved by Piketty and his team, supported by the book's history of twentieth-century shocks and political responses. Less promising is the book's emphasis on wealth, capital, and the rate of return. Following the income route to better inequality predictions requires merging his team's history of top income shares with the history of inequality movements within the lower 90 percent. It also invites a merger with other scholarship that has shown positive growth effects of the kind of democracy Piketty calls for.

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> "All things considered, I believe that the manufacturing aristocracy that we see rising before our eyes is one of the harshest that has ever existed on earth. But it is also one of the most limited and least dangerous.
>
> "Nevertheless, friends of democracy must keep an anxious eye peeled in this direction at all times. For if permanent inequality of condition and aristocracy are ever to appear in the world anew, it is safe to predict that this is the gate by which they will enter." -- Alexis de Tocqueville[1]

Alexis de Tocqueville's famous warning proved even more broadly correct than he predicted. It wasn't just a new manufacturing elite that arose in nineteenth-century America and Europe, but a property-owning elite in all sectors of the economy. Thomas Piketty's new book makes the same broader prediction about the twenty-first century that de Tocqueville could have made about the nineteenth. And Piketty's prediction could prove correct, just like de Tocqueville's.

Thomas Piketty's *Capital in the Twenty-First Century* has lit up the sky across Britain and North America. Starting from the solid empirics of the multi-authored Top Incomes Project led by Anthony B. Atkinson, Piketty, and Emmanuel Saez, *Capital* adds many data extensions, a simple theoretical framework, bold predictions, and controversial policy prescriptions. In the process, the book has transported us to a higher understanding of historical movements in inequality. This essay points out the paths that scholars can most usefully follow from the point at which his bus dropped us off.

Which leads should we follow?

This essay's reactions and recommendations can best be introduced with a multiple-choice exam question:

Q: Which of the following historical trends is of great concern to Thomas Piketty?

(a.) trends in the inequality of disposable income,
(b.) trends in the inequality of original income,
(c.) trends in tax progressivity,

[1] Tocqueville, Alexis de (1839/2004, p. 652).

(d.) trends in the importance of unequal inheritance,
(e.) trends in the inequality of nonhuman wealth,
(f.) trends in the ratio of wealth to income,
(g.) trends in the ratio of productive nonhuman capital to income,
(h.) trends in the rate of return on nonhuman wealth,
(i.) trends in the rate of return on productive nonhuman capital,
(j.) trends in the share of property income in national income, or
(k.) all of the above.

The answer is (k.), of course, and even 685 pages cannot contain all of Piketty's ideas and evidence on these issues. The evidence spills over to some large internet sites offering downloadable data sets and explanations of their calculation procedures.[2]

My main reason for posing the multiple-choice question is to argue that trends (a.) through (j.), all of them of great interest to Piketty, are ranked here according to their usefulness as paths for scholars and policymakers to follow. The first four, (a.)-(d.), are of top social priority, and Piketty and his collaborators have given economic historians a whole new research agenda. Yes, we should care greatly about income inequality trends (a.) and (b.). Unless they are checked by tax progressivity (c.), they may lead to the corrosive social effects on future generations of unequal patrimony (d.). Even if today's top income rewards were based on productive innovation, which is only partly true, they could hand political and social power to less productive heirs. Piketty conjures up the horror of a society dominated by the political, economic, and social power of unproductive heirs.

Paying more attention to these "final four" core inequality concerns, (a.) – (d.), promises greater insights than some of the other paths that Piketty has pointed out. This essay proceeds up the list from bottom to top, from (j.) up toward (a.)-(d.). It starts with one path that future scholarship should avoid, and finishes up with the

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[2] In addition to the book's own internet archive, see the home pages of Emmanuel Saez, Gabriel Zucman, the Top Incomes Database, and the Economic Inequality Chartbook of Anthony Atkinson and Salvatore Morelli.

For a convenient survey of income and wealth inequality in OECD countries since 1870, see Roine and Waldenström (2014). For earlier and less developed settings, see Branko Milanovic *et al*. (2011), and its underlying displays of "social tables", at http://gpih.ucdavis.edu, under "early inequality estimates".

most promising paths, those on which Piketty and his collaborators have advanced our knowledge the most. The final section points out some golden opportunities for scholars to enhance Piketty's core message, using a different literature on the economic effects of political inequality.

The shares of labor versus capital in current income, or (j.), have never proved to be good predictors of inequality, and continue to be poorly correlated with it over time and space. They are antiquated, dating back to nineteenth-century classical economics and to early postwar attempts to fit production functions econometrically. Their alleged link to inequality has never made sense. Having 60 percent of national income go to labor could reflect perfect equality, with 60 percent of the population equally sharing labor incomes and the other 40 percent equally sharing property incomes. Or it could mean horrific inequality if the 60 percent going to labor were shared by everybody but one propertied ruling family. Furthermore, in recent economic history, the share going to property has had no reliable correlation with inequality, either within the top ranks or for the entire economy.[3]

Which rate of return? One needs to proceed carefully when following Piketty's "rate of return" **r**, along paths (h.) and (i.). In the end, for inequality purposes, he rightly defines it from accounting aggregates, as the flow of income from nonhuman wealth, either pre-tax or post-tax, divided by the market value of the stock of that wealth. It is not the rate of return on capital, nor is it the market rate of return on bonds or on equity shares.

If we want to end up with a theory of income inequality, we find that Piketty has left us at an immediate crossroads. To use his model, we must believe that the same "**r**" applies both to his first equation, the accounting identity $\alpha = r * \beta$, and to his derived payoff equation, the growth-model eventual state where $\alpha = r * s / g$, where α is the share of some rich-person type of property income, β is the ratio of such wealth to national income, s is the share of income saved, and **g** is the growth

[3] For the lack of a clear correlation to the capital share within the top ranks, see Roine and Waldenström, "Long-run trends" (2014), esp. Figure 5.

rate of national income. This set-up led him to emphasize "**r** versus **g**", the economy's growth rate, for a given saving rate **s**.

Which **r** do we want to follow -- movements in a nominal rate of return on household wealth or movements in a real rate of return on productive capital inputs, which adjusts for inflation? If movements in α are to shed light on movements in conventional measures of income inequality, then everything in the first equation should be in current terms, without any adjustment for actual or anticipated inflation. This is because we wish to follow movements in a size distribution of current national income, one that shows how unequally all the income ranks face the same set of consumer prices. Yet his use of growth theory highlighting **r** versus **g** requires a "real" rate of return on "capital", one that would have to be adjusted for inflation – and for the inconvenient fact that capital and national product have different price deflators.[4]

Which way to proceed? Since we really want to explore trends in current income inequality, or paths (a.) and (b.), it would be better to stick with unadjusted current-price measures, and with the first "fundamental law", the accounting identity $\alpha = \mathbf{r} * \beta$ decomposing that share of some rich-person type of non-human capital income (α) into the nominal rate of return times the ratio of such wealth to national income. An immediate corollary is that if we wish to keep using the **r**-minus-**g** predictor of inequality trends, the growth rate **g** must be the growth rate of national income in current prices. Once we do that, then the real-growth model is not so useful, and all bets are off about whether **r** will stay above **g** across the twenty-first century. Piketty and we still lack reliable predictors, either of **g** or of **r** itself – until Piketty predicts the future of **r** with a political-economy trump card, to which I will return when discussing voice and governance.

Even if we had missed this conflict between the two concepts of r, we would soon have to confront the fact that Piketty's "**r** > **g**" device, for all its amazing

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[4] The growth model that Piketty has borrowed is based, unfortunately, on a one-commodity economy in which capital is the same "putty" that we eat. It therefore misses important movements in the rental and purchase prices of capital goods relative to consumption products.

rhetorical power, does not take us very far. Our task of explaining and predicting inequality movements is not made any easier by the requirement that we must first predict both a “rate of return” and the growth rate of the economy. The formula **r – g** takes us no further than we were transported fifty years ago by the concept of total factor productivity as a “source” of growth. It will be another “measure of our ignorance.”

Wealth/income ratios. As for the ratios of either wealth or productive nonhuman capital to national product, (f.) and (g.), these were moderately good predictors of top-income shares before World War I. Piketty’s book spends less time on the prewar era, aside from summarizing his own path-breaking coverage of the French case, in collaboration with Gilles Postel-Vinay and Jean-Laurent Rosenthal.[5]

Better measurements of prewar wealth may actually strengthen his assertion that the ratio of nonhuman wealth to income was correlated with income inequality. Such better measurements are now available for America 1774-1870, and they do improve the correlation. Piketty argued that “no doubt … the capital [i.e. private net worth] / income ratio was much lower in the New World colonies …” than in Europe. Indeed, it was even lower than Piketty and Zucman have been told. The only error in the exemplary work of Alice Hanson Jones, which Piketty and Zucman have used, was her income conjecture. Jones applied a capital/output ratio of 3-3.5 from the USA in the 1970s to the wealth of the 1770s. In our current work, Jeffrey Williamson and I now place this ratio much lower, at 1.89. Similarly, we find that American wealth/income ratios stayed below, but converged upward toward, the Piketty-Zucman estimates between 1774 and 1860. Since we also find slightly steeper increases in the inequality of income and wealth between 1774 and 1860 than did Piketty and Zucman, the net result of our revisions is to improve the Piketty-predicted correlation between the wealth/income ratio and income inequality for this early period.[6]

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[5] Piketty, Postel-Vinay, and Rosenthal (2006).
[6] Lindert and Williamson (2013), p. 747; and our *American Incomes since the Seventeenth Century* (in progress, Chapter 2).

Oddly, however, for the twentieth century trends that he and his collaborators have documented so well, the relevance of the wealth/income and capital/income ratios for the income distribution is less compelling. Across countries, the levels and movements of this ratio do not correlate well with those in income inequality. Over time, there is more correlation, within Britain, or France, or Germany, or the United States. Yet, as we shall see later, the same overall movements will show up when we look at the inequality movements in incomes that have little to do with wealth, such as wage rates or in middle/lower income ratios.

Wealth inequality (e.) Data on households' wealth inequality are particularly helpful as clues about income inequality before the twentieth century, when direct income measures were sparse. Yet for the twentieth and twenty-first centuries, wealth inequality does not quite make it into the "final four". For these more recent times, the wealth data are weaker than those we have for income inequality, as Piketty has acknowledged in a blog reply.

More fundamentally, the inequality of nonhuman wealth is inherently less interesting than the inequality of total income or total wealth (including human). The material inequality we really care about is a person's lifetime resources, as shared within a household. It can be measured either as an inflow, by one's lifetime human earnings plus inheritance, or as an outflow, by one's lifetime consumption plus bequest. For most people any calculation of their lifetime resources (all capitalized or all annuitized) shows the quantitative dominance of human earnings or of consumption flows, not of nonhuman wealth. In the life cycle, current wealth inequality is only an intermediate by-product – except to the extent that it is made unequal by inheritance. To the extent that the study of wealth inequality (e.) is a prelude to the study of the role of inheritance, it relates to the top-priority studies, even for the twentieth and twenty-first centuries.

Income inequality, fiscal progressivity, and inheritance. At the top of the multiple-choice list come the paths that scholars and the wider public should follow most closely, exploring those trends (a.) through (d.). The Atkinson-Piketty-Saez team has delivered a history of the shares of national income captured by the top income ranks in dozens of countries over the last hundred years. They have solved

the "top coding" problems that have hidden top incomes from our view for so long. This empirical triumph allows them to establish two great twentieth-century movements in the shares of income captured by the top income elite:

(1) The Great Leveling: In every developed OECD country, the income share of the top one percent -- or 0.1 percent, or five percent, or ten percent -- of households dropped between about 1913 and about 1973.

(2) The recent rebound of inequality: In several developed countries, most of them English-speaking, the same top income shares have marched upward since the 1970s. Yet the top income shares have hardly risen at all in a dozen other developed countries, notably in continental Europe and Japan.[7]

The first of these finding opens up a top-priority research project. Before the writings of the World Top Incomes team, we had only scattered evidence of the intercontinental reach of the great leveling of incomes across the early and middle twentieth century. Now there is no mistaking it. Table 1 shows the remarkable consistency of this decline among data-supplying countries, both for the entire period 1913-1973, and separately for Table 1's three sub-periods, two of them spanning the two world wars and one capturing much of the Golden Era of early postwar growth. As Piketty rightly emphasizes, the 1913-1973 era stands out in all of world history as the one in which the rest of society gained ground on the elites, here represented by the top one percent. The egalitarian effect was not small: on the average, the top one percent's share declined by about ten percent of national income.

If the Great Leveling appeared so consistently among advanced economies, our usual instinct would be that a single common cause was at work. There are four prime suspects for an exogenous shock that would have spanned all advanced

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[7] The share of income received by the top one percent has risen since the 1970s in Australia, New Zealand, Britain, Ireland, Canada, and the United States among long-term OECD countries, and also in Argentina, China, Singapore, and South Africa. In Portugal and Sweden it has begun to rise slightly since about 1980.

economies. Piketty controls the inside track in this race, emphasizing that the world wars and revolutionary expropriations of that era triggered a sustained rise in investors' uncertainty and pessimism, depressing wealth values throughout the financially advanced world. In his view, these historic shocks changed the whole balance of political power and social norms, causing redistribution away from capital and toward the masses.[8] We are likely to agree with him that chaos and investor pessimism played a central role in explaining why 1913-1973 was history's one great era of income leveling. Economic historians will nonetheless think of three other kinds of shocks that could have spread the leveling across all advanced countries in that era: a global decline in the rate of labor force growth; an acceleration in labor force productivity, due to accelerated public supply of education; and a shift away from labor-saving bias in technological change.[9]

If Piketty has supplied us with a common cause in the form of wars and revolution between 1913 and 1973, why bother to dig deeper? Shouldn't we just apply Occam's razor? The question cannot be answered without a good deal of further research into such multiple causes. As for the effects of war and revolution, one might or might not accept Piketty's implicit premise of diffusion, namely that in a connected world asset markets would spread investor pessimism, and a political *Zeitgeist* would spread progressive redistribution, across all advanced countries, including ones that did not suffer the shocks. One reason for doubting the primacy of this single cause is the behavior of the competing exogenous forces. Unlike the consistent inequality outcomes in Table 1, the exogenous shocks were varied greatly. As for the political shocks, how could the similar rates of income leveling have spanned defeated nations, victorious nations, and non-participants in the world wars? How could the net leveling have been so similar in stable-price countries as in countries that experienced hyperinflation? How could the leveling have turned out so similarly between countries that expanded government's share

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[8] See, for example, *Capital*, pp. 146-9, 275, and 284-6.
[9] These three exogenous forces have been featured in interpretations of inequality history within the United States. See Williamson and Lindert (1980, Chapters 6-13) and Goldin and Katz (2008).

of their economies to welfare state levels and countries that did not? A similar question could be raised about any of the other competing explanatory variables, since they too differed across countries. One obvious way to sort out these forces would be econometric testing on the pool of international experiences. That would not be easy, of course, since the twentieth century was not a well-behaved sample of randomly selected national experiences. Still, a balanced use of different techniques should be able to achieve consensus on the main causes of the great leveling.

Their second major finding, the rebound of income inequality in many countries since the 1970s, effectively renders the Kuznets Curve obsolete: no longer can one say that economic development from middle income levels leads to permanently more equal incomes. Revealing that income gaps between the rich and the rest are widening in so many countries evokes the usual split of ideological reactions. Critics will of course challenge Piketty's assumption that inequality is bad, and will revive the old argument that redistributing to the richest somehow creates jobs and growth. This is where Piketty scores another empirical triumph with a simple stroke. His Figures 14.1 and 14.2 (pp. 499, 503) on the history of top tax rates deliver a truth that is inconvenient for the trickle-down view: The three decades when the leading countries had their best growth performance of all time came when they kept the highest tax rates on top incomes and inheritances. Later came lower top tax rates and lower growth. Of course, correlation is not causation. Yet this simple display of history calls the bluff of anyone who extravagantly claims that "history shows" that high taxes on top incomes and inheritances are bad for growth. To advance beyond this simple point and to achieve deeper multi-causal explanations calls for the same kind of deeper analysis prompted by the remarkable Great Leveling.

Look at all incomes, all ranks

A key next step toward better inequality predictions is to transcend the book's emphasis on wealth and on top income shares, and to pursue the past and future of movements in *overall* inequality, over all types of income and over all

income ranks. Those who share Piketty's interest in nonhuman wealth must join forces with scholars mapping the history of human earnings inequality. Thus far Piketty has merely dipped his toes into these waters. His de-emphasis on human earnings inequality seems to reveal an aversion to the human capital literature, to the superstar "winner take all" literature, and to the larger labor-economics literature emphasizing shifts in the supply and demand for skills. Perhaps he shunned these just for revisionist impact, or perhaps because others have sometimes used human capital arguments to support the smug view that "people tend to get what they are worth".

Yet the empirical literature on earnings inequality – from a team led by his collaborator Anthony Atkinson, and from the labor economics profession -- is so substantial and well based that it needs to be enlisted as an ally in the struggle to explain inequality movements, especially for the twentieth and twenty-first centuries.[10]

The clearest way to underline the urgency of shifting our attention from capital to capital-plus-labor, and from top income shares to all income gaps, is to note what has been happening to income gaps, and to wage-salary gaps, within the non-elite income ranks since the mid-twentieth century. Incomes within this majority of the population are relatively free from the under-reporting that is so severe within the top five percent of the household-income ranks. And these same non-elite incomes are the ones receiving less attention in Piketty's book.

Suppose that Piketty and his collaborators had never been able to solve the problem of measuring top incomes. Suppose that our information about income inequality were restricted to studying the incomes of the bottom 90 percent of the income ranks, and we knew nothing about the top 10 percent except that they had some unknown income advantage over the rest of us. What kind of inequality movements would we have seen since the mid-twentieth century? Let me first

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[10] For the big history of earnings inequality in 20 OECD countries since the mid-twentieth century, see Atkinson (2008), and www.chartbookofeconomicinequality.com. For a convenient update on the earnings-inequality literature in labor economics using postwar American data, see Autor (2014).

illustrate with some evidence from the United States and the United Kingdom, and then summarize the multi-country patterns of inequality among the non-elite.

American inequality history since the early twentieth century yields the movements shown in Figure 1. Even within the lower 95 percent of the household income ranks, the movements in the gap between middle and bottom income classes show a striking fall and rise, as did the top-income and top-wage shares. So say the crude measures of the ratio of middle incomes to lower incomes, again confined to the lower 95 percent of the population to avoid the "top coding" understatement of top incomes, which tends to mean the top five percent in postwar US data. Indeed, the middle-versus-lower income ratios deliver a more ominous result than do the American top income shares made famous by Piketty and Saez. The gap between middle and lower incomes has widened well beyond its level back in 1929, whereas in such Piketty-Saez series as the top one-percent share, the income gaps have just barely recovered their 1929 levels. The same tale is told by the simple wage-rate ratios from labor market surveys. The pay gap between a person earning the 90th salary percentile and the median salary earner, which dropped from the 1930s to the 1950s, has continued to widen ever since. So say the data for the United States, as shown in Figure 1, and also for Canada. This story of rebounding income gaps in the middle and lower ranks is at least as dramatic as the now-famous rebound of the share received by the top one percent.

Something similar happened to British incomes below the top decile. Figure 2 suggests that the middle incomes, here shown with crude ratios of the 40th-90th percentile "middle" incomes to the average incomes in the bottom 40 percent, rose from the 1940s to the 1980s. Other measures suggest that such ratios continued to rise at least to the end of the twentieth century, though the British data series have switched income definitions a number of times. Again, as for the United States, that salary gap between a person earning the 90th salary percentile and the median salary earner has widened since the 1950s, or at least since the late 1970s.[11] Thus

[11] Britain had a rise and fall in the wage gap between median and lower wage groups in the early postwar era, as Figure 2 implies, and as noted by Atkinson (2008, pp. 378-379).

for Britain, as for America, the upward march of inequality since the 1950s or 1970s seems to have been dramatic over the entire income spectrum, and not just in the gaps between the very top and the rest of society.

Which other advanced OECD countries shared such experiences with America and Britain, and which did not? The set of countries for which earnings gaps have widened within the bottom 90 percent since the 1970s overlaps fairly well with the set for which the Top Incomes Database shows a rise in the share of all income going to the top one percent. Seven countries belonged to both sets in recent history: Australia (where gaps in wages and total incomes in the lower 90 percent widened 1975-2012), Canada (wage widening since the 1950s), New Zealand (1986-2012), Portugal (1982-2000), Sweden (1983-2011), United Kingdom (1978-2013), and the United States (1948-2012). There were two exceptions for which the gaps in wages and salaries widened (Germany 1978-2010 and Switzerland 1994-2010) but their top 1 percent share did not rise. Most of the countries for which the wage-salary gaps did not widen noticeably were countries for which the top income shares also did not rise, e.g. France and Japan.

These patterns raise a question: If the inequality history outside of the top decile so often resembles the history within these top ranks, and the history of the top decile's share of the total pie, what hint does that give us about the flow of causation among these developments? Looking at the kinds of factor incomes involved suggests a likely asymmetry. It seems more plausible to suggest that the causation runs more from inequality in human earnings to property inequality, than vice versa. It is easier to see how winning the human earnings lottery could cause a rapid accumulation of capital for yourself and your heirs than it is to see how having a lot of capital could buy top human earnings for yourself and your heirs, even if you all go to Harvard. For this reason, too, we should put a high priority on supplementing Piketty's *Capital* with a renewed emphasis on what forces make human earning power so unequal.

The first step toward explaining inequality movements thus calls for merging the historical movements of all income types and all income gaps into a single rate-of-change accounting. This will require an additive inequality decomposition along

the lines of the Theil index of inequality. For any given population, we should decompose changes in income inequality into changes in wage-salary rates, changes in the return to marketable wealth, and shifts in the income mix and in population types.

Voice and governance

Finding and weighing the deeper causes of inequality movements calls for bringing political voice and governance back to center stage. Piketty implicitly agreed with this urgency, when he repeatedly warned that inherited elite power could perpetuate economic inequality.

One of his warning devices was sounded when he predicted a rising after-tax rate of return, and by implication rising inequality, for the whole world up to the year 2200. How did he manage to predict the rate of return that far ahead? He did not try to sell us any forecast of trends in the labor saving bias in technological change, or of any other economic variable that drives inequality, aside from his assumptions about the savings rate. Instead he played the political-economy trump card I previewed earlier: "I have … assumed that fiscal competition will gradually lead to the total disappearance of taxes on capital in the twenty-first century." (p. 355). Here we see a strong form of the familiar "race to the bottom" imagined by conservative commentators. The classic conservative warning of a capital flight "race to the bottom" has already been punctured by other empirical studies.[12] High-tax welfare states have not lost much capital to tax havens, however annoying such flight might be. Indeed, Piketty himself tried to counter such fears in his superb Chapter 14, when showing that the major countries' highest-ever tax rates on top incomes and wealth were accompanied by their highest-ever growth rates between World War II and the 1970s.

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[12] See, for example, Rodrik (1997); Lindert (2004), Chapters 10-12, 18; and the earlier studies cited there. Note that Gabriel Zucman has found that "around 8% of the global financial wealth of households is held in tax havens, three-quarters of which goes unrecorded." (Zucman 2013). This magnitude speaks to several issues, but such practices have not cost any advanced economy a significant share of GDP.

Yet even if fiscal competition among nations and local governments shows no sign of having slashed top tax rates in four leading countries, their domestic political trends have achieved some such effect, as Piketty rightly reminds us in other parts of the book. Since the 1970s there has been an ominous accumulation of lobbying and election-buying power by the wealthiest, in those same countries in which income inequality has risen the most. What can be done about this? Again, Piketty calls for a societal shift toward broader political voice. What he seeks are intermediate mixtures of "the market and the ballot box" "capable of mobilizing the talent of different individuals …. and organizing collective decisions" (p. 569). He hopes that this set of institutions will deliver more progressive taxation to support an optimal amount of social expenditures.

Could his goal gain support from evidence that we can achieve such a "democratic control of capital" without it costing us anything in terms of economic growth? Yes, quite easily. Indeed, that supporting evidence has already been delivered by studies he has pushed aside. What Piketty calls the "democratic control of capital" seems equivalent to the "inclusive institutions" of governance in the writings of Daron Acemoglu, James A. Robinson, and their collaborators. They have delivered some supporting messages that Piketty's vision needs. Yes, the broader political voice of such inclusive governance does promote economic growth. Yes, it does so by "increasing investment, increasing schooling, encouraging economic reforms, improving public services, and reducing social unrest". And no, the strong association of inclusive politics with growth is not due to reverse causation from income growth to democratization.[13] Merging these scholarly literatures, we can reach better inequality predictions featuring the influence of political voice and governance institutions.

The most immediate priority, however, is to press on with merging research on human earnings inequality into the history of the inequality of total incomes, as

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[13] See Acemoglu *et al.* (2008), Acemoglu and Robinson (2012), and Acemoglu and Robinson, NBER working paper 20004 (March 2014), from which I quote here. For Piketty's puzzling dismissal of Acemoglu-Robinson, see *Capital*, pp. 444-5, 624, 639-40.

argued in the preceding section. Piketty and his collaborators are continuing to advance our collective knowledge on the top fringe of human earnings, including their work on CEO pay. More broadly, we need to strive for an accounting framework that decomposes changes in total income inequality. In the end, the causal chain we are likely to emphasize will run from political inequalities to overall income inequality, with only a secondary role for history's shocks to nonhuman wealth.

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Table 1. Changes in the Shares of National Income Received by the Top One Percent of Economic Units, 1913 – 1973

Net change, as a percent of national income

	1913-1929	1913-1953	1913-1973
Denmark	-10.7	-15.0	-15.9
France	- 2.9	-10.0	-10.1
Germany	- 6.7	- 6.2	- 7.7
Japan	0.9	-10.0	- 9.8
Netherlands	- 2.9	- 9.0	-14.1
Norway	1.0	- 4.5	- 6.0
South Africa	- 1.8	- 7.8	- 9.0
Sweden	- 7.2	-14.0	- 8.2
United States	0.5	- 8.9	-10.2
Median	- 2.9	- 9.0	- 9.8
		1929-1953	**1929-1973**
Australia		- 2.0	- 5.0
Canada		- 5.8	- 6.8
Denmark		- 4.3	- 5.1
Finland		- 5.1	- 5.5
France		- 7.2	- 7.3
Germany		0.5	- 1.0
India		- 1.2	- 6.1
Japan		-10.9	-10.7
Netherlands		- 6.1	-11.2
New Zealand		- 3.1	- 5.5
Norway		- 5.4	- 6.9
South Africa		- 6.0	- 7.2
Sweden		- 6.8	- 8.2
United States		- 9.3	-10.7
Median		- 5.6	- 6.9
			1953-1973
Argentina			-8.0
Australia			-3.0
Canada			-1.1
Denmark			-0.8
Finland			-0.4
France			-0.1
Germany			-1.5
India			-4.9
Japan			0.2
Malaysia			0.7
Mauritius			-0.3

{Table 1, continued]

	1953-1973
Netherlands	-5.1
New Zealand	-2.4
Norway	-1.5
Singapore	-1.3
South Africa	-1.2
Sweden	-1.3
Switzerland	-0.01
United Kingdom	-2.7
United States	-1.3
Median	-1.3

Source and notes to Table 1:
The source is the World top Incomes Database, http://topincomes.g-mond.parisschoolofeconomics.eu/.

Some of the shares were available only for years close to the benchmark year, and not for that year itself. In particular, "1913" was a different year in these cases: 1900-1909 for France, 1914 for Netherlands, 1915 for South Africa, and 1912 for Sweden; "1929" was 1930 for Sweden; "1953" was 1950 for West Germany and 1954 for South Africa; and "1973" was 1974 for South Africa.

Some of the data, instead of covering the baseline household units, covered either married couples plus individual adults, or all adults, or tax units. In each such case we followed a change in a top income share consistently defined.

Most of the measures are pre-fisc, referring to original incomes before taxes and transfers, though a few include some transfer payments. See the notes provided in the source.

Figure 1. Inequality within the Bottom 95 Percent, United States 1929 - 2009

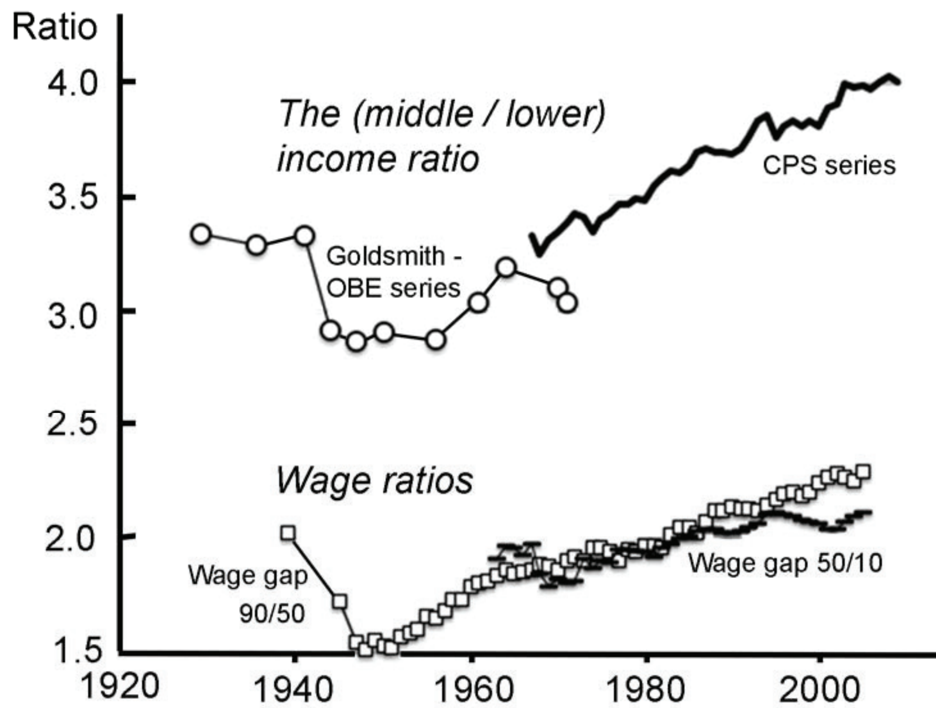
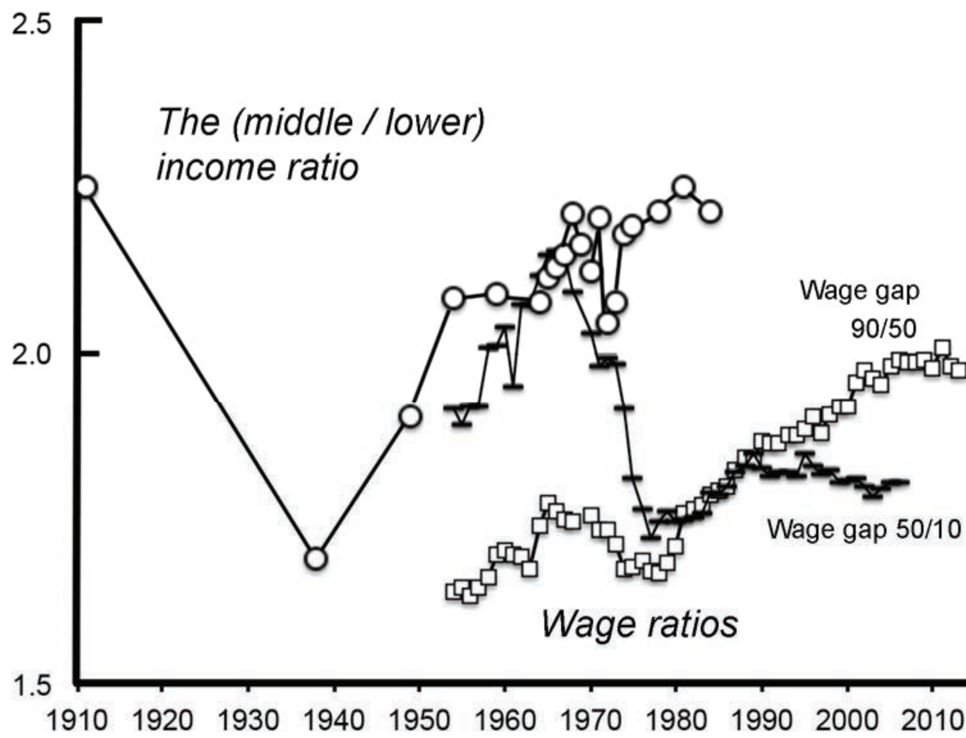


Figure 2. Inequality within the Bottom 90 Percent, United Kingdom 1911 - 2013



Notes to Figure 1:

Middle / lower ratio = the ratio of the average income of the 41-95 percentiles divided by the average income of the 0-40 percentiles in the household income ranks.

Goldsmith-OBE series = Shares of pre-tax money income received by households, as estimated by Selma F. Goldsmith (1967) for 1929-1961 and then by the US Office of Business Economics, *Survey of Current Business* for 1961 through 1971.

CPS money income = US Census Bureau, *Current Population Survey*, Money Incomes of Households. The money income series includes receipts of cash transfer payments, but not of payments in kind. See http://www.census.gov/compendia/statab/cats/income_expenditures_poverty_wealth/household_income.html, accessed 31 October 2013.

Wage ratio 90/50 = Atkinson (2008, pp. 411-424), series for the 90th percentile wage relative to the median. This starts with the Labor Market Survey wage of the 90% percentile for 1973-2000. To this it splices an Economic Policy Institute wage series for 2000-2006, a CPS male wage for 1967-1973, and a CPS series for workers of both sexes for 1939-1967.

Wage ratio 50/10 = same sources and procedures as for the Wage ratio 90/50, except that it takes the ratio of the median wage to the 10th-percentile wage.

Notes to Figure 2:

For the wage gaps, the base = FES, all workers, 1968-2003 (Atkinson 2008, Table S.4, Pp. 384-5). 1954-1979 = Schedule E income tax (on gross wages and salaries at primary job), Atkinson, *Changing Distribution of Earnings,* Table S.7 (p 390), spliced to FES at 1968. Splice 2003-2006 from Atkinson's Table S.5 at 2003.

The middle / lower gap (SPI) measures the ratio of pre-tax personal income for (average income of percentiles 41-90) / (average income for percentiles 0-40). It uses the Bowley-Stamp-Routh distribution of individual taxpayers' incomes for 1911, followed by the before-tax Survey of Personal Incomes data from the Royal Commission on the Distribution of Income and Wealth, *Third Report on the Standing Reference* (1977), p. 240.