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# ***Capital in the Twenty-First Century:* A Review Essay**

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## **I. Introduction**

Amazon sales leader, reviews in *The New Republic* and *The New York Review of Books*, a comedy news appearance—Thomas Piketty has clearly struck a chord with *Capital in the Twenty-First Century* (2014). The book quickly became ammunition for advocates of egalitarian-enhancing policies on the left, generating in turn a backlash against its positive and normative claims on the right. This success says much about the contemporary American zeitgeist, as mounting evidence of increasing inequality has led to vigorous public debate on what policy responses, if any, are needed. But the extraordinary reception is also due to *Capital's* broad vision of the extent and nature of inequality. The book documents a range of aspects of inequality in advanced societies and presents a general conceptual framework to explain its empirical claims. Piketty takes an unabashed nor-

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mative stance that current levels of inequality are unjust and is unafraid to make very strong policy recommendations. He further challenges economics and other social sciences to better engage the study of inequality, with harsh criticism of the intellectual insularity of economics. These critiques have resonated in the policy world as well as in academia.

*Capital* is the offspring of a widely praised research program, based on the Top Incomes Database, conducted by Piketty together with Anthony Atkinson, Emmanuel Saez, and other coauthors, over the past 15 years. This program has involved massive data collection in 20 countries so far and has produced many new empirical findings, particularly concerning the top of the income distribution.<sup>1</sup> *Capital* moves beyond this research with its theoretical and normative dimensions. The target audience for *Capital* is not academia but the educated public and perhaps, in particular, those technocrats in ministries around the world whose policy decisions influence the distribution of income and wealth. One must admire Piketty's courage in displaying such an abundance of data and theory outside the academy. Nonetheless, there is much to capture the attention of scholars in the wealth of data he provides and in the theoretical arguments he makes, and we should all be concerned about the soundness of economic arguments directed at opinion shapers and policy makers. In this essay, we discuss *Capital in the Twenty-First Century* as a piece of scholarship.<sup>2</sup>

*Capital* is a weighty tome in every sense of the adjective. Its major substantive claims can be organized into four broad propositions.

1. The capital share in total income represents a primary source of persistent inequality in Western societies. Increasing capital shares across economies reflect a return to historical levels (an increase of 18 percent to 25 percent, 21 percent to 28 percent, and 17 percent to 30 percent of national income for France, the United States, and the United Kingdom since 1975).<sup>3</sup> The low capital shares of the twentieth century are due to a set of shocks.<sup>4</sup> High capital returns ( $r$ ), relative to overall growth ( $g$ ), are the linchpin of capital share growth, as instantiated in the now ubiquitous claim that  $r > g$  raises the share of capital income in an economy (see fig. 10.9).

<sup>1</sup> This signal achievement of the project is discussed at length in Milanovic (2014).

<sup>2</sup> One feature of *Capital* that distinguishes it from earlier highly visible social science books such as *The Bell Curve* (Herrnstein and Murray 1994) is its appearance in the age of social media. As a result, many commentaries on the book have appeared: some addressing the scholarship and others the political program. We have cited some of these commentaries and acknowledge that we have likely missed others that are important and visible.

<sup>3</sup> See fig. 6.5 of the technical appendix and numerous other figures. But see fig. 4.6 for the United States, which tells a somewhat different story.

<sup>4</sup> See figs. 11.8 and 11.9 for France and fig. 11.12 for France, Britain, and Germany.

2. The distribution of the wage share in aggregate income is becoming increasingly skewed as wage growth concentrates in the upper tail. The highest wage levels are not explained by productivity, but rather reflect social norms.
3. Observed levels of inequality are unjust and warrant a range of major policy interventions. These range from a global wealth tax to the restructuring of higher education finance.
4. Economics, as a discipline, as a result of ideological biases, lack of respect for other fields, and pseudoscientific pretensions, has failed to properly study inequality.

In our judgment, the book succeeds admirably in its presentation of the data that Piketty's research program has generated. These data span a variety of sources, both new and old, and range across many countries and many years. *Capital* is, nonetheless, unpersuasive when it turns from description to analysis. Our critique has three distinct themes.

1. Data do not speak for themselves. *Capital* is not careful about the meaning of its data. Wealth and capital are conflated. The analysis is strangely ahistorical in its failure to interpret data in the process of economic evolution and change. Identification problems involving substantive claims are ignored.
2. Theory matters. The aggregate framework for understanding capital share dynamics is too thin to provide empirical insights. The microeconomic claims on the determination of factor returns are inconsistent with the aggregate analysis and are, on their own terms, fundamentally defective. *Capital* thus does not contain a coherent theoretical view of income distribution.
3. Policy recommendations must take the logic of policy evaluation seriously. *Capital* does a poor job of describing objectives and constraints in policy, partly because of its theoretical weaknesses and partly because of its shallow normative underpinnings. Many policy claims are based on selective and sometimes misleading descriptions of the state of social science knowledge.

Our criticisms of *Capital*'s analysis of inequality are complemented by parallel criticisms of the book's stance on the history and state of economics.

Both of us are very liberal (in the contemporary as opposed to classical sense), and we regard ourselves as egalitarians. We are therefore disturbed that Piketty has undermined the egalitarian case with weak empirical, analytical, and ethical arguments. We seek to reveal these weaknesses, remaining confident that there is a stronger case to be made.

## II. Data

As rich as *Capital* is in data description, it is impoverished in ascribing meaning to these measurements. The interpretations that are presented suffer from problems in terms of ambiguity as to what is being measured and in the way in which long-run data are interpreted as observations of a dynamic economy.

One basic measurement problem for Piketty is signaled by an error in the title of the book: it should be *Wealth in the Twenty-First Century*. “To simplify the text, I use the words ‘capital’ and ‘wealth’ interchangeably, as if they were perfectly synonymous,” writes Piketty (47).<sup>5</sup> After a decade of dispute in the mid-twentieth century, macroeconomists have accepted the idea that different kinds of capital can be aggregated and that the aggregate can be taken as an input in an aggregate production function. Adding together capital stocks and consumer durables, however, creates an entirely new set of problems. For instance, it would seem to invalidate the marginal productivity theory of factor pricing that Piketty subsequently relies on. For distribution theory, the issues are related and, in our judgment, more crucial. If housing, machines, and van Gogh’s *Starry Night* all have different rates of return, wherever they come from, the distribution of “capital types” across individuals will be critical for the evolution of the wealth distribution. Changes in tastes concerning post-impressionist paintings will change the wealth share of national output even though such changes leave the productivity of the capital component of wealth unchanged. More generally, the rate of return on capital as a productive input is conflated with the rate of return from its ownership, which includes any capital gains or losses.

The failure to distinguish between capital as a production input and as an asset raises both theoretical and empirical problems. For theory, wealth dynamics are determined by asset returns. Whatever theoretical stance one wants to take on asset return mechanisms, they must move beyond static marginal product pricing. For empirics, one needs to decompose the wealth stock to see where returns are coming from in order to measure the role, for example, of the elasticity of substitution for the aggregate production function, which Piketty claims matters in understanding the dynamic patterns he reports.

This class of measurement issues underlies a number of critiques of *Capital*’s empirical analysis. The debate over housing’s contribution to increases in the value of capital is one such example. Bonnet et al. (2014) make this argument and conclude that Piketty’s results are due to changes in the value of housing, primarily driven by price appreciation rather than growth of the stock. So-called productive capital, they claim, exhibits dif-

<sup>5</sup> Two- and three-digit numbers standing alone in parentheses refer to page numbers in *Capital*. References to pages in the technical appendix will be explicitly noted.

ferent behavior. Rognlie (2014) similarly argues that capital share increases are due to housing but takes a different view on the determinants of housing prices.

Second, Piketty's wealth/capital measures are at once too inclusive and too exclusive: all alienable and tradable physical commodities are included, while anything else is not. Human capital, for instance, barely makes an appearance. Why? "There are many reasons," says Piketty. "The most obvious is that human capital cannot be owned by another person nor traded on a market (not permanently at any rate)" (46). This is the only reason offered, and it amounts to saying, "I don't include it because I can't measure it." Measurable or not, if wealth inequality is supposed to capture claims to resources, then ignoring human capital makes reported inequality meaningless. Similarly, lifetime budget constraints depend on pension and social security systems, which are also ignored. Nor is there discussion of the public capital involved in institutions ranging from universities to museums. One wonders whether inequality in the private ownership of Picasso's paintings matters much for Manhattanites who have access to the Museum of Modern Art, the Guggenheim, and the Whitney.

A third measurement problem involves the use of capital and labor shares as sufficient statistics for "inequality." Welfarist-based concerns about inequality of income and wealth are proximate; what matters is their relationship to inequality in the quality of life individuals achieve. While there are of course no time series of well being, other measures can be brought to bear on the evolution of quality of life. There has been, for instance, much research over the past 30 years on historical anthropometric data.

Fogel (2004) documents the dramatic improvements in nutrition and life expectancy that occurred during the twentieth century. He further makes an argument that improved nutrition played a major role in the attenuation of economic inequality because it allowed the disadvantaged to qualitatively increase labor force participation and effort at work. These phenomena are not going to be reversed any more than mass vaccination or other medical advances will be. Interestingly, Fogel argues that the relative stability of the measures of the nineteenth-century income distribution in the United Kingdom (which is consistent with *Capital's* fig. 6.1) contrasts with increasing divergence between rich and poor with respect to life expectancy and health.<sup>6</sup> This discrepancy, Fogel argues, indicates that anthropometric data may contain more information on income inequality than the standard measures. It is possible that the story of relative welfares of workers and rentiers is different from what Piketty infers from his income data.

<sup>6</sup> Steckel (1983) and Floud, Wachter, and Gregory (1990, fig. 5.1) make this point for different time periods with height as the dependent variable.

The two problems Fogel points to are linked. Since Kuznets (1941), there have been debates about the measurement of national output. Nordhaus and Tobin (1972) discuss adjustment of GNP to better measure economic welfare, with a focus on the services of consumption capital and the disamenities of urbanization. Green accounting is a contemporary counterpart. Household production, services from natural capital, government-provided services such as education and health care, as well as the services provided by social relationships have all been tagged as items not valued or incorrectly valued by markets but are nonetheless significant for assessing quality of life; yet they are ignored here.<sup>7</sup> This is puzzling given Piketty's criticisms of the narrowness of economic thinking.

Beyond conceptual ambiguity as to what *Capital* claims to measure, we see distinct problems in terms of the meaning ascribed to the measurements. For Piketty, a capital share on the order of 25–35 percent is treated as some sort of basin of attraction for modern countries, while the lower levels found between 1910 and 1990 result from shocks (war, exceptionally high growth, wealth-destroying inflation, unusually progressive tax policies, etc.) to a timeless growth process that he embodies in his fundamental laws of capitalism.

A competing vision of long-run inequality dynamics is that economies, as well as associated polities and societies, have evolved across the twentieth century from one approximately invariant structure to another. The analysis of long-run economic outcomes should thus try to understand this evolution and its attendant changes in the nature of the income distribution.<sup>8</sup> A first problem with Piketty's interpretation of his data is that he ignores the distinction between transitional and steady-state (really metastable steady-state) observations. Any nontrivial positive and normative conclusions about factor shares must account for this difference, since such conclusions depend on the time-series properties of factor shares, not on their levels at a point in time.

There is another interpretation problem. Despite the claim that *Capital* is "as much a work of history as of economics" (33), Piketty takes a fundamentally ahistorical view of capital shares. A historically situated view of long-run behavior requires that one simultaneously consider how

<sup>7</sup> For an interesting study that demonstrates the breadth of possible social capital effects on the wealth distribution, see Geruso and Spears (2014) on Hindu and Muslim defecation.

<sup>8</sup> We note two classic examples in a large literature. Hansen and Prescott (2002) argue that the growth of knowledge altered the relative profitability of a land-intensive Malthusian technology relative to a Solovian one in which capital and labor are the only inputs. Technical change leads to a transition from exclusive use of one technology to, asymptotically, exclusive use of the other. Galor and Weil (2000) argue that population growth leads to three distinct development regimes: A Malthusian regime of low output and low population growth is succeeded, because of technical change, by a regime of higher output and population growth, which is succeeded by a modern regime of low population and high income growth. This literature is ignored in *Capital*.

fertility patterns, political institutions, scientific and medical knowledge, religious beliefs, and ethical values (one could easily treble the list) have coevolved with the capital share during the last two centuries.<sup>9</sup> Once one thinks about capital shares along transition paths for different social, political, and economic environments, then it is evident that these levels are not commensurable objects. Improvements in the levels of mortality and morbidity will be reversed only under bizarre scenarios. The psychological impact of inequality depends on beliefs about the ethics of distribution, as well as the determinants of inequality. Any normative evaluation of the capital share cannot be made outside of its social milieu.

The lack of historical care in *Capital* is evident in its dismissive treatment of the emergence of human capital as an income source. Piketty pays lip service to the structural transformation of “a Society of Rentiers” into “a Society of Managers,” but this plays little substantive role in his thinking: “The probable long-run decrease in capital’s share of national income from 35–40 percent to 25–30 percent is, I think, quite plausible and surely significant but does not amount to a change of civilization. Clearly, skill levels have increased markedly over the past two centuries. . . . Capital has not disappeared . . . for the simple reason that it is . . . hardly less useful than in the era of Balzac and Austen” (224).

This claim is surely wrong. A society in which a fixed factor, land, plays a primary role in income and wealth accumulation is very different from one in which individual ability, educational investments by families, and the broader society play first-order roles. To borrow a phrase from Posner and Weyl (2014), the world of Jane Austen, in which landed wealth and an aristocratic social order were fundamental to the nature of inequality, will not return, regardless of the level of the capital share.<sup>10</sup>

### III. Capital Theory

In this section we consider Piketty’s capital theory, ignoring for the moment our earlier observation that he is not writing about capital. Piketty’s

<sup>9</sup> Acemoglu and Robinson (2015) make the distinct argument that one cannot ignore the evolution of institutions and technology when formulating laws of capitalism, so that “laws” concerning factor shares that ignore this are not possible. We agree but do not think that long-run claims about the fundamental role of institutions as opposed to, say, ideology are any more law-like than claims about factors unless the claims are tautological.

<sup>10</sup> If *Capital* had been systematic in using literature as an evidentiary source, this would have been clear. William Makepeace Thackeray (e.g., *Vanity Fair* and *The Newcomes*) chronicles the transformation of marriage markets as the landed aristocracy interacted with a rising haute bourgeoisie. The Industrial Revolution created a permeability of the upper classes that is very different from the middle of the Hanoverian era in England (see Doepke and Zilibotti [2008] for a formal model of this transition). Similarly, World War I, which Piketty treats as a shock to the capital stock, is interpreted by Ford Madox Ford (*Parade’s End*) as contributing to the destruction of the social order of Edwardian England. A deeper reading of literature would lead to a more nuanced analysis than Piketty’s.



central aggregate causal claim is that inequality, equated with a large capital share, is a consequence of high returns to capital  $r$ ; compared to output growth  $g$ . “This inequality expresses a fundamental logical contradiction. The entrepreneur inevitably tends to become a rentier, more and more dominant over those who own nothing but their labor. Once constituted, capital reproduces itself faster than output increases. The past devours the future” (571). This is a theoretical claim: a long-run excess of the rate of return to capital  $r$  over the growth rate of output  $g$  will lead to ever-increasing inequality. This is just rhetoric. If capital continually reproduces itself faster than output increases, at some point the return to capital would exceed national income. Obviously something more must be at work here. Piketty’s theoretical claims are essentially those of balanced growth in a Keynesian or early neoclassical growth model. The theoretical statements are his so-called First and Second Fundamental Laws of Capitalism:

1. Capital’s share of output is the product of the rate of return on capital and the capital/output ratio (52). In Piketty’s notation,

$$\alpha = r \times \beta,$$

where  $\alpha$  is the capital share of national income and  $\beta$  is the capital/output ratio. As Piketty observes, this is an accounting identity that holds along any time path of an economy. It is a law of arithmetic—an accounting identity.

2. In a steady state, the capital/output ratio is the ratio of the savings rate to the growth rate of output (166):

$$\beta = \frac{s}{g}.$$

Readers will recognize, and Piketty notes (230), that the Second Fundamental Law is the Fundamental Equation of Harrod (1939) and the balanced growth condition of Domar (1946). This is a long-run equilibrium condition. When the two statements are put together, the long-run capital share of output is

$$\alpha = r\beta = s\frac{r}{g}.$$

Despite the hints throughout the book that the capital share of output will continually grow, that “the past devours the future,” Piketty claims here that the capital share stabilizes, and so the central political economy question becomes: is the limit capital share of output socially destabiliz-

ing? How much is too much? Nowhere in the book are these questions raised let alone answered.

Piketty uses the two fundamental laws to explain capital/income ratios. For instance, he concludes that slow growth caused the return to high capital/income ratios over the past few decades. “Decreased growth,” he writes, “is thus responsible for capital’s comeback” (166). In our judgment this aggregate theoretical framework is fundamentally flawed.

First, Piketty engages in a sleight of hand when he employs a steady-state definition to discuss dynamics. Why does the second law hold? Piketty’s growth of wealth model derives neither from the Keynesian analysis of Harrod (1939) nor from the Keynesian growth model of Domar (1946), its exact antecedents, nor is it derived as the limiting AK case of the Solow (1957) model (in which output is linear in capital and labor is not productive). Piketty does sketch some dynamics in his online technical appendix: Wealth  $W$  in year  $t + 1$  is wealth in year  $t$  plus year  $t$  savings  $S$ :  $W_{t+1} = W_t + S_t$ . Define the relevant ratios, and upon rewriting,

$$\beta_{t+1} = \beta_t \left( \frac{1 + s_t/\beta_t}{1 + g_t} \right). \quad (1)$$

Thus  $\beta_t$  increases or decreases as  $s_t/\beta_t$  exceeds or is exceeded by  $g_t$ . Piketty writes “that *if* the savings rate and the growth rate both stabilize at some given level  $s_t = s$  and  $g_t = g$ , then the wealth/income ratio  $\beta_t$  must necessarily converge to  $\beta = s/g$ ” (tech. app. 28; emphasis added). That is the sum total of the growth theory: the difference equation is another identity, and all the theoretical claims are buried in the undiscussed *if*.

Second, suppose that the steady state is descriptive of developed countries. How can  $g$  and  $r$  “explain” high capital/income ratios? These variables (and  $s$ ) are not model primitives; they are endogenous, and the fundamental steady-state relation limits the variables only to a two-dimensional surface in the three-dimensional space of possible values. The variables  $r$ ,  $s$ , and  $g$  are bound by necessary economic relations that limit the possibilities for both steady-state behavior and the undescribed dynamics away from balanced paths. Further, their precise relationship is determined by exogenous variables omitted from Piketty’s theoretical apparatus. In conventional growth models, the properties of balanced growth paths are determined by technology and preference parameters. Models of the *if* would tell us under what conditions on model primitives are high-limit capital/output ratios produced.

Third, there are problems with the second law and Piketty’s use of gross versus net variables. Krusell and Smith (2015; in this issue) argue that the capital/income ratio in modern textbook growth models is not, as Piketty would have it,  $s/g$  but is instead  $s/(g + \delta)$ , where  $\delta$  is the rate

of capital depreciation. Crucially, they observe that this leads to truly implausible behavior if the net savings rate is held constant as  $g$  becomes small. (We will discuss below another problem of gross vs. net for Piketty's theory of the individual distribution of income.)

Fourth, throughout the book Piketty claims that  $r > g$  causes growth of the capital share of income. On the other hand, the First and Second Fundamental Laws imply that in a steady state, capital's share is  $sr/g$ . The savings rate  $s$  will be less than one, so it is certainly possible in the steady state for  $r$  to exceed  $g$ . Does  $r > g$  signal a problem? Quite the contrary. Ray (2014) reminds us that in the Ramsey-Cass-Koopmans and related optimal growth models,  $r > g$  is a necessary transversality condition for optimality of the consumption plan. One might object that Ray is referring to normative models rather than market equilibrium. But in equilibrium models too, low future values of the capital return signal capital overaccumulation. This phenomenon is easily seen in the inefficient equilibria that are the first example in any lecture on overlapping generations models and is well known in production models as well. The problem of finding price characterizations of efficient (as opposed to weakly efficient) paths goes back at least to Malinvaud (1953).<sup>11</sup>

The failure of *Capital's* two fundamental laws to provide any income or wealth distribution dynamics is implicitly recognized when its analysis shifts to the importance of the elasticity of substitution between labor and capital,  $\sigma$ , for factor shares. Piketty notes the basic result of marginal productivity factor pricing, that if the elasticity of substitution in production exceeds one, an increase in the capital/income ratio will widen the gap between  $r$  and  $g$ . Notice that if  $\sigma > 1$ , then a growing capital share is compatible with capital deepening for Solow-type growth models; in this sense Piketty's larger theoretical baggage is irrelevant.

What case is presented that  $\sigma$  is in fact greater than one? Piketty argues that, historically, the elasticity of substitution  $\sigma$  lies between 1.3 and 1.6. These values derive from a back-of-the-envelope calculation in which a constant elasticity of substitution (CES) production function is used to back out an elasticity value that reconciles the capital/output and capital share numbers he has constructed. Taken at face value, the argument is not circular but ignores any alternative explanations for rising capital shares.<sup>12</sup>

<sup>11</sup> For the simple neoclassical growth model, research on this question culminates in the necessary and sufficient conditions of Cass (1972). Similar results are also known for overlapping generations models with production.

<sup>12</sup> Other authors have used evidence of rising capital shares to rethink aspects of Solow-type growth models. Nordhaus and Phelps (1997) suggest that capital-augmenting technical change can explain the decline of labor shares in Europe. Given the incompatibility of capital-augmenting technical change with balanced growth paths, observed originally by Uzawa (1961), economists have been reluctant to explore models with this type of technical change;

Unfortunately, Piketty's calculation goes against the grain of most (although not all) careful analyses of  $\sigma$ . Diamond, McFadden, and Rodriguez (1978) is the classic demonstration that, in the presence of technical change, the elasticity of substitution is identified only under strong assumptions on the nature of technical change, unless one chooses the form of the production function a priori. Diamond et al.'s result is a general nonparametric nonidentification result for  $\sigma$ .

Piketty might respond that the use of particular functional forms for the aggregate production function and the process of technical change to estimate factor shares and elasticities and the like has been a successful strategy since Solow (1957). Following this route does not help his case. Research such as León-Ledesma, McAdam, and Willman (2010) shows that elasticity estimates in empirical CES models are extremely fragile unless one uses both the production function and its associated first derivatives, equated to factor prices. This suggests great imprecision in Piketty's calculations. Further, many studies using the CES production function have estimated  $\sigma$  and failed to find strong evidence that its value exceeds one. Antràs (2004) estimates  $\sigma$  for US aggregate data and finds that while, under the assumption of Hicks neutrality of technical change,  $\sigma = 1$ , allowing for biased technical change produces estimates of 0.5 and lower. McAdam and Willman (2013), allowing biased technical change in conjunction with demand-side factors, find that  $\sigma$  is between 0.62 and 0.87 for eurozone data. Studies based on micro data also find elasticities less than one; for example, Chirinko, Fazzari, and Meyer (2011) produce an estimate of 0.4 for American firms. Oberfield and Raval (2014), not available to Piketty at the time of his writing, use US firm-level data to construct an aggregate elasticity level that moves from 0.67 in 1972 to 0.75 in 2007. There are also studies whose conclusions are more supportive of Piketty; for example, Bentolila and Saint-Paul (2003) report a number of elasticities of substitution not significantly different from one for different sectors in OECD countries. But such studies are rare. Against the weight of the evidence, nothing in *Capital* suggests a principled basis for believing that Piketty's rough calculation can be reconciled with more careful previous work.

Another problem with Piketty's use of the elasticity of substitution has to do with the interpretation he places on his capital measure as net of depreciation. A textbook exercise shows that the effect of an increase in the capital/output ratio on capital's share of total output is positive or negative, depending on whether the elasticity of substitution is greater than or less than one. On the basis of historical data, Piketty estimates an elasticity of substitution between 1.2 and 1.6. It is important to un-

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Acemoglu (2002) is a prominent exception. We believe that Piketty's findings reinforce the importance of exploring this avenue.

derstand that this is a net capital elasticity. Rognlie (2014) undertakes a rough calibration making use of US data to conclude that a net elasticity of 1.5 requires a gross elasticity of about 2.27, certainly a bigger number than most studies find.<sup>13</sup> He goes on to show that under the assumption of a constant savings rate, getting  $r - g$  to increase as  $g$  falls, one of Piketty's central themes, requires an outlandishly large gross elasticity of substitution.

A final problem with *Capital's* aggregate framework is due to a problem raised earlier: its omission of human capital. This is a puzzling modeling choice given the role human capital plays in modern growth theory. The wealth creation that Piketty tracks (as opposed to capital creation) is in part a return on human capital. A major component of wealth creation is the conversion of human capital into market capital. This is how innovation and entrepreneurship create wealth. We are told that human and nonhuman capital have played complementary roles and that each must be treated separately (46). This is a non sequitur. How much can be said about the dynamics of one variable in a dynamical system without reference to the others?

To summarize, the aggregate analysis of the two fundamental laws cannot say very much about inequality by themselves. Inequality will depend on the distribution of claims to the factor returns and the multiplicity of factors determining levels of intergenerational mobility. Long-run inequality dynamics require an explicit model of the many mechanisms that map the distribution of claims to factor returns in one generation to the incomes of the next, not the mechanical application of a knife-edge special case in which all capital income is saved, all labor income is consumed, and human capital does not exist. Piketty's empirical claims concerning the neoclassical theory of labor and capital output shares are informal, and his application of the theory is incorrect in not accounting for the distinction of capital measured gross versus net of depreciation. The weight of the existing empirical evidence is not on his side. Perhaps, however, this is not so crucial because, as we will see, Piketty subsequently rejects the marginal productivity theory of wages on which neoclassical distribution theory is based.

#### IV. Microeconomics of Capital Income

*Capital* presents distinct explanations of what is termed "distribution at the individual level" for both capital and labor returns. In both cases, marginal product factor pricing is rejected. Before considering the spe-

<sup>13</sup> See Chirinko (2008) for a survey.

cific claims made about factor returns, it is important to recognize that in its rejection of marginal product factor pricing, the microeconomic claims in *Capital* undermine its aggregate distribution analysis. The second fundamental law, as a steady-state condition, has been established to hold only under marginal product factor pricing. If factor returns deviate from marginal product pricing, the condition is no longer required in a steady state. The link between  $\sigma > 1$  (actually  $\sigma \gg 2$  if one follows Krusell and Smith [2015] or Rognlie [2014], as was discussed above) and a positive relationship between the capital/labor ratio and the capital share is also a consequence of marginal product factor pricing. It may be possible to identify alternative factor pricing rules that preserve these relationships, but *Capital* provides no such theories. To be clear, we are not defending the empirical validity of marginal product factor pricing per se, but rather arguing that *Capital's* macroeconomic and microeconomic treatments of factor pricing are inconsistent.

### *Capital Income*

*Capital's* individual-level explanation of the distribution of capital income has two dimensions. First, *Capital* rejects marginal product factor pricing as an explanation of capital returns. “Broadly speaking the central fact is that the return on capital often inextricably intertwines elements of true entrepreneurial labor (an absolutely indispensable force for economic development), pure luck (one happens at the right moment to buy a promising asset at a good price), and outright theft” (446). This statement is somewhat confused since it conflates the return on capital with high-skilled labor and confuses expected and realized marginal product. No empirical evidence outside of anecdotes is presented for its individual claims. Thus *Capital* does not establish a basis for rejecting neoclassical theory let alone replacing it.

Skewness of the wealth distribution is the second dimension of capital income inequality. Piketty's theoretical basis for this skewness is unclear since *Capital* does not have a theory of savings behavior, although there is an implicit assumption in much of the book that capital income is all saved while wage income is all spent. Many forms of individual heterogeneity are ignored. Variations across individuals in their attributes, which come to them by genetics, by luck, and by choices made earlier in their lives, are determinants of the distribution of capital ownership at a point in time. This heterogeneity is significant if one wants to understand the mechanisms (and therefore remedies) generating inequality.

Ethical arguments about inequality, claims concerning distributive justice, depend on the nature of this heterogeneity, in particular, whether it arises from preferences or from constraints. Instead of presenting a

theory of capital ownership, Piketty makes much of the rise of income generated by inherited capital, arguing that capital income delimits intergenerational mobility. The strongest evidence on the rising role of inheritance comes from France and led him to conclude that 25 percent of income will come from inherited wealth by 2030, a level comparable to that of 1790.

The import of this claim about inherited capital and inequality is weaker than it might appear. First, the distribution of the remaining 75 percent consists of wages and noninherited capital income, which bounds the inequality associated with Piketty's rentier dystopia. This simple calculation ignores any interrelationship between inheritance and wages. The ultimate role of wages in total could be bigger or smaller, depending on how wages affect inheritance and inheritance affects wages. The lack of microeconomic foundations in Piketty's analysis also makes it difficult to link *Capital's* inheritance data with observed levels of intergenerational mobility.

The microfoundations of capital ownership matter for the aggregate model as well. A richer theoretical model, one that took account of labor and human capital, could have aggregate properties very different from those implied by the two laws. For example, socioeconomic class is central to Piketty's story, so imagine the following slight enrichment of Piketty's theoretical structure: an economy consists of capitalists and workers, and each class has its own distinct savings rate. This scenario was analyzed by Stiglitz (1969), who showed how complicated the asymptotic behavior of a two-class model with distinct consumption and capital goods production functions can be. Multiple balanced growth paths may exist, they need not be locally stable, and complicated limit sets involving oscillation around an unstable balanced growth path are possible. At a minimum, analysis that purports to talk about the distribution of wealth should at least account for differences in the behavior of rich and poor, capitalists and workers. Stiglitz's analysis suggests that models that do so exhibit behavior very different from that of the one-class models that lie behind Piketty's first and second laws.

### *Wages*

While the primary focus of *Capital* is on capital income, there is substantial discussion of wage inequality, with distinctions drawn between continental Europe and the United Kingdom and, especially, the United States. For the United States, the rise of "supermanagers" is argued to have led to wage growth being concentrated in the extreme upper tail. What explains the growth of very high wages? In its proposal of an alternative to marginal product wage determination, *Capital* starts with an argument that this theory cannot apply to the highest earners.



The vast majority of top earners are managers of large firms. It is rather naïve to seek an objective basis for their high salaries in individual “productivity.” . . . When a job is replicable . . . we can give an approximate sense of the “marginal product.” . . . But when an individual’s job functions are unique, or nearly so, then the margin of error is much greater. Indeed, once we introduce the hypothesis of imperfect information into standard models . . . the very notion of “individual marginal productivity” becomes hard to define. . . . It becomes something close to a pure ideological construct . . . a justification for higher status. (330–31)

This theoretical discussion does not rise to the level of a scholarly argument. Uniqueness of tasks does not imply the impossibility of constructing empirical proxies for productivity. One cannot argue that if Adobe hires a manager from Microsoft, information from the past is irrelevant to forming beliefs about her future performance. Imperfect information has nothing to do with the concept of marginal productivity per se. Beyond the evident problems with the internal logic of these claims, they are startlingly insensitive to contemporary economics. One can read *Capital* and have no idea that microeconomics in general and labor economics in particular have evolved beyond the introductory course model of wage determination. Principal-agent problems and labor contracts are hardly a new area of research. Baranchuk, MacDonald, and Yang (2011) show how many stylized facts on executive compensation can be understood using modern ways to think about the setting of incentives.

*Capital* also argues that for the highest wages, a marginal product interpretation fails empirically: “it is very difficult to explain the observed variations in terms of firm performance” (334). Regardless of one’s view of the relevant literature, *Capital* does not justify this empirical statement. While empirical studies that argue against a productivity interpretation are cited, studies that argue in favor are ignored. Prominent examples include Kaplan and Rauh (2010), which elaborates evidence on a wage/productivity link for managers, and Frydman and Saks (2010), which argues that executive compensation has been linked to firm performance for most decades of the twentieth century and more tightly so since 1980. This role of productivity in high salaries is hardly resolved; contrast Bertrand (2009) and Kaplan and Rauh (2013). Strong claims about the state of knowledge are not appropriate.

*Capital* claims that the extent of extremely high wages in a given economy is determined by social norms. The position is justified on the basis of the argument that the far higher wages of US managers compared to those of continental Europe and Japan show that national differences in tolerance for inequality, rather than differences in the pro-



ductivity of top managers, explain these disparities.<sup>14</sup> The claim about cross-country compensation differences presupposes that there are common production functions across corporations, which is hardly obvious if one considers how legal regimes and norms affect the transformation of given levels of capital and labor into output. Clark (1987) is a classic historical study of how, for a given technology, effort norms can induce dramatic productivity differences. Further, the claim assumes that managerial talent is equal across countries. If the market for CEOs were truly international, then it is hard to see how massive salary discrepancies that are the basis of the argument can be sustained. And if these flows do not occur, one can just as easily argue that Anglo-American firms create more scope for managerial productivity. By analogy, one cannot plausibly use the fact that the Soviet Union earned far fewer Nobel Prizes in the natural sciences than the University of California, Berkeley, to argue that Nobel Prizes are socially determined, as opposed to a demonstration of the limited possibilities for many forms of exceptional scientific achievement under communism.

Even if the claim that cross-country discrepancies in CEO salaries are not consistent with marginal productivity wage determination were true, it implies only that at least one of the two sets of CEOs is not paid this way. One could argue that American (and in this case British) exceptionalism means that these societies do not have norms against inequality, as Piketty argues, but that this absence simply means that US/UK CEOs are paid their marginal products and that it is the egalitarian norms of continental Europe that lead to a deviation between productivity and wages. There is a venerable tradition of arguing the capitalistic American character, which could be interpreted as suggesting a marginal productivity explanation that is unique to American salaries. Classic studies include Sombart ([1906] 1976) and Potter (1954), which respectively emphasize how American views of opportunities for prosperity helped inhibit the emergence of a socialist party and created the belief among Americans that societies should level up rather than level down.

We belabor these two examples because they illustrate a general problem with *Capital*: that when Piketty moves to topics for which he has not done original research, he is careless with theory, and empirical evidence is presented in an unreflective and selected fashion. These problems reappear when Piketty turns to normative considerations.

<sup>14</sup> In their application of the Sattinger (1979) model of employee assignment, Gabaix and Landier (2008) argue that increasing chief executive officer pay is a consequence of increasing firm size combined with complementarity between firm size and managerial quality.

## V. Policy

*Capital* takes many policy stances. Piketty's most visible policy recommendation is a global capital tax, which would directly address the patrimonial capitalism that he deplores. While he refers to this as a "utopian idea" (515), a sketch is given as to how forms of such a scheme could be implemented. We concur that a global wealth tax is utopian. We therefore focus on what *Capital* has to say about more likely policies. We see two distinct problems.

First, many of the policy recommendations have very little to do with either the empirical findings summarized in *Capital* or the broader research of Piketty and coauthors. The recommendations ignore relevant literatures and so are elliptic or simplistic in formulation and cavalier with regards to efficacy. Appendix A evaluates *Capital's* discussion of college tuition and college attendance relative to extant research.

While those policies that are described are not well defended, a converse problem exists: *Capital* exhibits a startling lack of imagination in the policy set that it considers. Many of the factors that *Capital* raises as sources of inequality suggest policy responses beyond ex post income redistribution. For example, a claim is made about monopoly rents associated with Microsoft, Facebook, and so forth in the context of undercutting the position that their founders' wealth is merited. If this is the case, it is strange to ignore the possibility that monopoly power may be attenuated via regulation. Similarly, he does not consider how changes in patent law could transfer profits from pharmaceutical companies or other types of firms. Similarly, the role of financial firm profits in generating inequality suggests the potential importance of changes in financial regulation. As noted by Stiglitz (2014), taxes are a very limited way to think about how government policy can alter capital shares.

Similar limitations in thinking occur with respect to wage inequality. It is odd for a purportedly historically sensitive treatment of wages to ignore changes in the role of unions across the course of the twentieth century. Levels of unionization and the power unions can exercise, of course, depend on labor law. As for supermanager salaries, the concern that these are not determined by productivity has led to numerous changes in corporate governance regulations, for example, in terms of shareholder rights. Our purpose here is not to evaluate or propose new regulations; our point is that there are ways to think about policy that directly respond to the inequality-enhancing mechanisms that *Capital* asserts are important.

There are also obvious omissions with respect to policies that ameliorate the harms of inequality. Piketty expresses concerns in various places about the corrosive effects of inequality on democratic governance.

Schlozman, Verba, and Brady (2012), one of Piketty's evidentiary sources, contains a variety of policy ideas, none of which involve taxes per se, for equalizing political power. One obvious example is campaign finance reform. Others involve changes in voting laws or representation rules. Political reforms to reduce the influence of wealth are not utopian. The 1907 Tillman Act, banning campaign donations by interstate banks to presidential or congressional campaigns, and the 1910 Publicity Act (amended 1911), requiring disclosure of donors to House and Senate races, are examples of how the influence of wealth was combated in the Progressive era. The Federal Corrupt Practices Act of 1925, adopted in the wake of the Teapot Dome scandal, limited contributions. While we do not make claims about the efficacy of such legislation, our argument is that, as a historical matter, campaign finance reform is possible.

Why is the policy discussion in *Capital* so weak, despite its intention not to just describe the world but to change it? One reason is the weakness of *Capital's* microeconomic foundations. Without these, Piketty is reduced to an excessive focus on the limited policy instruments of his aggregate income distribution model or variants (including human capital). The microeconomics of human capital and skill formation suggest far more complex policies than, for instance, simply lowering the tuition component of the price of college. See Appendix A for further discussion.

A second reason stems from the lack of serious engagement with political philosophy. Piketty's ethical stance on distributive justice is summarized thus: "social inequalities are acceptable only if they are in the interest of all and in particular of the most disadvantaged social groups. Hence basic rights and material advantages must be extended insofar as possible to everyone, as long as it is in the interest of those who have the fewest rights and opportunities to do so. The 'difference principle' introduced by . . . Rawls . . . is similar in intent. And the 'capabilities' approach favored by . . . Amartya Sen is not very different in its basic logic" (480).

Several distinct ideas of distributive justice are conflated here. "Interest of all" would seem to mean that observed inequalities are unjust on social welfarist grounds. Rawls's (1971) difference principle is based on a positive claim about choices of social, economic, and political structures that would be agreed on behind a veil of ignorance. Sen (1999) and Nussbaum (2006) shift the focus of justice away from income to capabilities and so represent a curious basis for arguing against income and wealth inequality per se. Other discussions are equally muddled. Marc Fleurbaey's (Fleurbaey and Maniquet 2011) and John Roemer's (2000) work, treated here as an extension of Rawls (1971) and Sen (1999, 631, n. 23), in fact develops a responsibility-sensitive version of equality of opportunity that is very distinct in its foundations.

Why do philosophical underpinnings matter? One general reason is that none of these philosophical positions does much to directly justify the reduction of high income and wealth. Piketty's focus is on the rich, while the strongest egalitarian arguments, in our judgment, involve improving particular features of the lives of the disadvantaged. This is the broad policy import of Rawls, Nussbaum/Sen, and Fleurbaey/Roemer. Similarly, classical welfarist arguments for redistribution do not regard high income or wealth as intrinsically bad: it is the concavity of the social welfare and/or individual utility functions that creates a justification for redistribution. If the objective of redistribution is to help the badly off, then its justification is dependent on the effects of the transfer on beneficiaries. Lindert (2014) argues that the historical dynamics of inequality require far more focus on the bottom 90 percent than is provided in *Capital*. We believe that this focus is also necessary in order to construct ethically compelling arguments for egalitarian policies.

A more thoughtful approach to problems of distributive justice would have required more attention to the microeconomics of inequality. Capital shares, fractions of income due to inheritance, and supermanager salaries are determined by individual choices and constraints. These measures do not speak to differences in life expectancy, educational attainment, and other criteria that define capabilities or any other conception of a good life. Nor do they speak to the relationship between existing levels of inequality and personal responsibility, which is the essential Roemer insight in identifying unjust inequality. The limits of *Capital's* measurements as discussed in Section II interact with the lack of any conception of individual choice to create a gulf between empirical claims and ethically compelling justice principles.

Piketty might respond that the reduction of high incomes and wealth is intrinsically desirable, regardless of the use of the resources that are removed from the upper tail. We think this is a defensible position, but in order to be persuasive, such a claim must be predicated on a demonstration of the consequences of purely economic inequality for the social and political spheres.<sup>15</sup> In a world of parallel Robinson Crusoe economies, we cannot imagine a good argument for reducing the productivity of anyone, no matter how affluent. Even if such spillovers between spheres exist, any policy of reducing income and wealth of the upper tail must be compared to policies designed to palliate the spillovers; our earlier discussion of policy remedies to inequities in political power is one example.

<sup>15</sup> We ignore possibilities such as the possible positive effect on others due to relative prices if some set of endowments is destroyed. The standard welfare theorems make uninteresting such scenarios in isolation.

We think the lack of careful philosophical thinking might even explain the dissonance between *Capital's* macro and micro inequality mechanisms. As we have noted, the book's rejection of marginal product factor pricing is based on very shallow theoretical arguments and poor evaluation of empirical studies. Given his reference to "theft" as a component of capital returns and his dismissal of a marginal product explanation of very high salaries as an "ideological construct," we conjecture that Piketty is committed to rejection of marginal product factor pricing because he thinks, if it holds, that it would justify inequality of returns. It is true that during the time when marginalism emerged as a foundation of economic theory, marginal productivity pricing was thought to have normative content. John Bates Clark, for example, thought this way. But the equating of marginal productivity prices with a just distribution has long been understood to be a non sequitur.<sup>16</sup> Rawls (1971, 71) famously argues that winners of the genetic lottery have no claim to what their genes produce, marginal product pricing or not. Nozick's classic (1974) libertarian defense of the primacy of property rights does not depend on the determinants of wages or asset prices. Voluntary bargaining between agents, subject to rules on enforcement of contracts, is sufficient to justify the resulting income distribution. We do not endorse either the Rawls or the Nozick position. Here we argue only that contemporary political philosophy gives no principled (as opposed to instrumental) significance to marginal product factor pricing. If *Capital* had taken distributive justice ideas seriously, it might have avoided major conceptual problems.

## VI. Criticisms of Economics

*Capital* contains many criticisms of economics that have contributed to the public visibility of the book. A number of the objections Piketty raises, such as the overmathematization of economics and its lack of attention to data, are useful for a pop economics exposition; sales are enhanced when an economist claims to reveal the dirty laundry of the profession. We ignore these, except to share our disappointment that Piketty ignores the subfield within philosophy of science that focuses on economics and has generated and evaluated a broad range of such critiques. We focus on two objections. The first, which Piketty repeatedly makes, is that political beliefs have distorted the state of accepted

<sup>16</sup> Stigler (1941, 297), hardly one who would have been sympathetic with Piketty's economics or ethics, dismissed any normative links to marginal productivity theory when discussing Clark's work on marginal productivity pricing: "Clark performed one function for which economics has less call for gratitude. . . . His marginal productivity theory contained a prescription as well as an analysis. The dubious merits of this ethical system need not concern us, but it is a cause for regret that Clark's exposition . . . afforded some grounds for the popular and superficial allegation that neo-classical economics was essentially an apologetic for the existing economic order."

knowledge in economics. He argues that claims by economists about phenomena such as stability of factor shares or the importance of inherited wealth in inequality have been driven by ideological agendas. Second, we comment on Piketty's rebuke of economists for arrogance and a failure to recognize and employ the knowledge of other disciplines.

Piketty makes claims about specific cases based on inaccurate and misleading evaluation of the evidence. We recognize that this is a serious charge and can be justified only by a detailed analysis of what is said in *Capital's* text. In Appendix B we work through his evidence with reference to claims about Simon Kuznets; our online appendix discusses claims about prewar beliefs on factor share stability and Gary Becker's views on human capital. In all three cases we conclude that Piketty's claims are undone.

There is a broader problem. Piketty argues by example that the collective views of the profession are clouded by ideology. But these examples offer no evidence about the equilibrium distribution of theoretical and empirical commitments of the mainstream of the discipline. Kitcher (1993) demonstrates through a careful analysis of cases how the presence of nonepistemic (i.e., non-truth-seeking) factors influenced, but did not stop, scientific progress. One can formalize conditions under which nonepistemic factors in theory acceptance can either retard or accelerate the adoption of epistemically superior theories by a scientific community (Brock and Durlauf 1999). The metaphor of a marketplace of ideas may be hackneyed, but it captures the essential insight that scientific claims compete with one another. It is essential to consider the collectivity, not the individual. The rhetorical strategy of impugning the academic integrity of individual scholars followed by a facile generalization to a generation of economists merely to hype the alleged radical stance of *Capital* does a disservice to Piketty's scholarly work as well as to the work of his targets.

Piketty's views on economics and other fields of knowledge are quite scathing. He refers to economists' "contempt for other disciplines and their absurd claim to greater scientific legitimacy, despite the fact that they know almost nothing about anything" (32). We are further told that "economics should never have sought to divorce itself from the other social sciences and can advance only in conjunction with them" (32).

Piketty greatly exaggerates. Behavioral economics is deeply indebted to psychology just as social economics has explicitly attempted to integrate sociological ideas into economic theory and empirical work. Public choice has become a joint project of economics and political science. Economic historians can hardly be argued to be systematically ignorant of history and historiography. Piketty makes assertions about the economics discipline without attention to the dynamics of knowledge and ideas. As researchers, we personally have been long concerned to inte-

grate sociological ideas with economic formalism. True, economists often possess an imperialistic view of social science, and many of the ideas about behavior found in other fields can induce automatic reactions about lack of rigor. But these complaints do not equate to an autarchic discipline.

In light of its criticisms of economics, it is all the more remarkable that *Capital* itself contains no serious engagement with other social sciences. Piketty discusses social norms without reference to the sociology or psychology literatures, and there is nothing in his empirical work that represents a move from conventional economic history methodology. Piketty's provocative remark that he admires Lucien Febvre and Fernand Braudel more than Robert Solow and Simon Kuznets (32) is particularly ironic because the links between his research and the Annales school of history do not extend beyond a common interest in long-run phenomena. Nor were we able to identify any influence by the others, all cultural anthropologists, singled out for admiration. Interdisciplinarity should be made of sterner stuff.

## VII. Conclusion

*Capital in the Twenty-First Century* is written as a prosecutor's brief. Analytical argumentation and interpretation of evidence are constructed to conform to an a priori vision of inexorable and unjust inequality. We strongly suspect that Piketty's ethical intuitions (which we largely share) have led him to inappropriately blur the distinction between scholarship and advocacy, to the great harm of *Capital's* scholarship. We fully accept that ethical positions matter in determining the questions a social scientist chooses to study just as we believe that theoretical commitments are needed to understand data. However, neither of these makes scholarly standards nugatory in principle or arbitrary in practice.

At a general level, *Capital* fails because it is based on a teleological worldview. Whether one focuses on Hegel's theory of history, in which the state evolves to maximize the freedom of the individual mind; the Whig interpretation of history, in which Britain evolves inevitably toward greater liberty;<sup>17</sup> or forms of Marxism in which dialectical materialism is a universal law of motion leading to the communist utopia, each is a failed predecessor to *Capital's* mode of thinking.<sup>18</sup> Stephen Jay Gould famously derided teleological evolutionary explanations of every facet of human behavior as "just so" stories, in which the assumption that something is adaptive determines the interpretation of a given fact about people. This criticism summarizes much of the weakness of *Capital*.

<sup>17</sup> We thank Glen Weyl for suggesting this analogy to us.

<sup>18</sup> See Acemoglu and Robinson (2015) for a comparison of Piketty's thinking with that of Marx.



We began this review with a reference to *The Bell Curve*, another example of a social science book that created an uproar upon publication. In reflecting on this earlier case, there is an interesting paradox. While Herrnstein and Murray's discussion of genes and intelligence had very little effect on social science, their focus on ability as a determinant of life outcomes has mattered greatly.<sup>19</sup> We believe that *Capital in the Twenty-First Century* may follow a similar path. Piketty's empirical work should stimulate theorists to develop frameworks that both explain his aggregate facts and capture the microeconomic mechanisms that the broad inequality literature has identified as important. Similarly, we believe that Piketty has only scratched the surface of inequality measurement, as dimensions beyond income and wealth ultimately provide the ethical foundations for redistributive policies. Failures of interpretation do not diminish the value of the World Top Incomes Database. What will matter in the long run is the original scholarly signal and not the subsequent noise that constitutes *Capital*.

## Appendix A

### *Capital in the Twenty-First Century* on College Tuition

*Capital*, chapter 13, contains a section "Do Educational Institutions Foster Social Mobility?" that discusses how college tuition delimits equality of opportunity in the United States. The United States is compared unfavorably with Europe. For the United States, "parents' income has become an almost perfect predictor of college success" (485). Piketty is elliptic about what specifically should be done with respect to the American context, in that he does not directly advocate a particular policy. However, his discussion is couched in a way that argues for reducing tuition to near zero.

The focus on tuition as a barrier to higher education shows little knowledge about the determinants of college attendance. Piketty's excoriation of Harvard's high tuition and associated high family incomes of undergraduates ignores the financial aid system of both the university and the federal government. The literature on credit market constraints and educational access has a far more subtle view on college affordability than appears in *Capital*, and some of the findings might surprise Piketty.<sup>20</sup> Does this matter empirically? Kinsler and Pavan (2011) find that, for high-achieving high school students, the link between tuition and family income has substantially attenuated in the last several decades.<sup>21</sup> If one focuses on one of the most salient aspects of educational inequality in America, the black-white gap, evidence on the role of tuition is even weaker. Heckman

<sup>19</sup> We thank James Heckman for this observation.

<sup>20</sup> See, e.g., those of Lochner and Monge-Naranjo (2012) on government guarantees of student loans.

<sup>21</sup> Remarkably, Landerso and Heckman (2015) find that intergenerational income correlations in Denmark and the United States are equal, despite the fact that college is free in Denmark.



(2011) finds that conditional on ability, blacks attend college at a higher rate than whites, a finding that suggests the efficacy of affirmative action policies. We certainly do not argue that the financial aid opportunities make tuition irrelevant, but given the availability of financial aid, the high parental income of Harvard students is probably due to reasons other than binding financial constraints.

While Piketty notes that “it would be naïve . . . to think that free higher education would resolve all problems” (486), one would never know that research on college attendance suggests far more complicated reasons for low attendance among the disadvantaged than price per se. Hoxby and Avery (2013) document the existence of large numbers of disadvantaged students with high academic achievement who fail to apply to elite colleges for which financial aid would render them viable. Dynarski and Scott-Clayton (2006) and Dynarski and Wiederspan (2012) describe evidence on how the complexity of the financial aid system discourages college attendance among the disadvantaged by making net price calculations difficult if not impossible. These types of findings shift policy considerations from price to information and transparency. Similar findings exist with reference to the black-white gap. Neal (2006) documents the puzzle of overall low educational investment/high education returns for blacks, suggesting the presence of implicit costs in addition to explicit ones. Fu (2014) finds that the low African American college application rate can be reconciled in a neo-classical model only by extraordinarily high marginal application costs.

When one moves away from the specifics of college applications and choices, very different policy approaches are suggested. Important literatures on cognitive and noncognitive skill formation, fetal origins, and social influences on education are ignored (see Almlund et al. [2011], Almond and Currie [2011], and Epple and Romano [2011], respectively). Policies ranging from increased early childhood investment, to reduction of cognition-related environmental hazards such as lead, or to changes in the admissions policies of public universities are never mentioned, despite good reasons to think they address first-order reasons why higher educational attainment is tied to parental socioeconomic status. All of them involve microeconomic mechanisms that are absent from *Capital*. At the same time, such policies can require ethical considerations that go beyond *Capital's* assertions.<sup>22</sup> “Low tuition” is an easy policy to advocate, but far too simplistic if one is serious about improving educational access and reducing educational inequality.

## Appendix B

### *Capital in the Twenty-First Century and the Kuznets Curve*

Piketty's views on ideology and economics come out most clearly in his claims that Simon Kuznets had a political agenda in arguing that there exists a U-shaped relationship between the level of economic development and inequality. Kuz-

<sup>22</sup> See, e.g., Durlauf (2008) on affirmative action.

nets's failing (in Piketty's eyes) has much significance, since "for too long, economists have neglected the distribution of wealth, partly because of Kuznets's optimistic conclusions" (16).

Specifically, Piketty argues that the "Kuznets curve" paper (1955) "offered a far more optimistic interpretation of his results than he had given in 1953" (13), the gravity of which leads to the extraordinary remark that "it is clear from reading his books (as opposed to his papers) that he shared the scientific ethic" (14). Piketty describes the leap from Kuznets's research to the Kuznets curve:

After reminding readers of all the reasons for interpreting the data cautiously and noting the obvious importance of exogenous shocks in the recent reduction of inequality in the United States, Kuznets suggests, almost innocently in passing, that the internal logic of economic development might also yield the same result. . . .

. . . The reduction of inequality observed in the United States between 1913 and 1948 could therefore be portrayed as one instance of a more general phenomenon, which should theoretically reproduce itself everywhere, including underdeveloped countries then mired in postcolonial poverty. The data Kuznets presented in his 1953 book suddenly became a political weapon. In order to make sure that everyone understood what was at stake, he took care to remind his listeners that the intent of his optimistic predictions was quite simply to maintain the underdeveloped countries "within the orbit of the free world." (14)

What does Kuznets in fact write?

The somber picture just presented may be an oversimplified one. But I believe that it is sufficiently realistic to lend weight to the questions it poses—questions as to the bearing of the recent levels and trends in income inequality, and the factors that determine them, upon the future prospect of underdeveloped countries within the orbit of the free world. (1955, 24)

One extreme—particularly tempting to us—is to favor repetition of past patterns of the now developed countries, patterns that, under the markedly different conditions of the presently underdeveloped countries, are almost bound to put a strain on the existing social and economic institutions and eventuate in revolutionary explosions and authoritarian regimes. There is danger in simple analogies; in arguing that because an unequal income distribution in Western Europe in the past led to accumulation of saving and financing of basic capital formation, the preservation or accentuation of present income inequalities in the underdeveloped countries is necessary to secure the same result. . . . Because they have proved favorable in the past, it is dangerous to argue that completely free markets, lack of penalties implicit in progressive taxation, and the like are indispensable for the economic growth of the now underdeveloped countries. . . . It is equally dangerous to take the opposite position and claim that the present problems are entirely new

and that we must devise solutions that are the product of imagination unrestrained by knowledge of the past, and therefore full of romantic violence. (25–26)

If the formulation of the Kuznets curve was meant to create a weapon in Cold War, Kuznets would seem to have made a singularly ineffective argument as to its utility. His use of the term “full of romantic violence” presumably reflected his experiences during the Russian Revolution and Civil War. But this in no way demonstrates that his 1955 article was discontinuous with his prior thinking or that his substantive claims were ideologically driven. Interestingly, a recent intellectual biography of Kuznets (Fogel et al. 2013) has a rather different evaluation of the 1955 paper and its impact from Piketty’s: “It is interesting to note that Kuznets’ 1955 paper has been treated not only as important theoretically but also as providing empirical support for the inverted-U hypothesis. . . . This is a strange development since Kuznets was at pains to stress its theoretical nature, repeatedly warning that his allusions to fragmentary data were not evidence but little more than guesswork. Most of the paper is devoted to explicating factors that arose during the course of growth and created pressures both to increase and reduce inequality” (102).

We do not see how Piketty can reasonably interpret Kuznets (1955) as a political distortion of Kuznets’s own empirical work. The gap between Piketty’s reading of Kuznets and what Kuznets wrote says more about Piketty’s own ideological position than it does about Kuznets. That said, we urge readers to examine Kuznets (1953, 1955) and form their own conclusions.

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