



BOOK SYMPOSIUM

## Capital, inequality, and power

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I am most grateful to the editors of *HAU* for putting together such an impressive set of review papers about my book. I would like to view my book more as work of social science than one of economics or history. It seems to me that we often lose a lot of time in the social sciences because of little disputes about disciplinary boundaries. I could not dream of a better recognition for my work than the stimulating collection of interdisciplinary essays that *HAU* is now publishing. There is no way I can do justice to the richness of each review and address the many points that they raise. I would like, however, to take this opportunity to briefly clarify a number of issues.

### Capital and the social sciences

Let me first summarize what I have tried to achieve in *Capital in the twenty-first century*. This is primarily a book about the history of wealth and property, and the political and economic conflicts generated by its unequal distribution. Thanks to the cumulative efforts of several dozen scholars, we have been able to collect a relatively large historical database on the structure of national income and national wealth and the evolution of income and wealth distributions, covering three centuries and over twenty countries. The first objective of my book is to present this body of historical evidence in a consistent manner. I start from the evidence and then try to analyze the many economic, social, and political processes that can possibly account for the evolutions that we observe in the various countries since the Industrial Revolution. I stress from the very beginning that we still have



too little historical data at our disposal to be able to be sure about anything. As a consequence, there are many important issues on which my book has little to say, and even more issues where what I write is highly incomplete and exploratory. But at least we have substantially more evidence than we used to have, so it seems worth trying. My book is probably best described as an analytical historical narrative based on this new body of evidence. By doing so, I hope I can contribute to put the study of distribution and of the long run back at the center of economic, social, and political thinking.

Many nineteenth-century authors, including Thomas Robert Malthus, David Ricardo, and Karl Marx, were putting the distribution question at the center of political economy. They were very much motivated by the impressive social and economic evolutions that they saw around them (a lesson that today's economists should remember). Unfortunately, they had limited systematic data at their disposal, so their approach was mostly theoretical. This tradition was pursued in the twentieth century with the more data-intensive and historical approaches pioneered by Simon Kuznets and Anthony Atkinson (who contributed in a decisive manner to the data collection project on which my book is based). I am working directly in their sidesteps.<sup>1</sup>

My work is also an attempt to pursue and to renew a long tradition of research by historians and sociologists on the long-run evolution of wages, prices, and wealth. This line of research was particularly active in France between the 1930s and the 1970s, with major work by François Simiand, Ernest Labrousse, Fernand Braudel, and others.<sup>2</sup> As I emphasize below, my approach to capital, social classes, and the perpetuation of inequality is in my view complementary with Pierre Bourdieu's emphasis on the transmission of cultural and symbolic capital. My thinking on these issues was strongly influenced by this tradition. In my book I also attempt to study the evolution of collective representations of social inequality in public discussions and political debates as well as in the literature and in movies. I believe that the analysis of representations and beliefs systems about income and wealth is an integral and indispensable part of the study of income and wealth dynamics.

Indeed, as stated in the introduction, the main conclusion of my work is that “one should be wary of any economic determinism in regard to inequalities of wealth and income.”

The history of the distribution of wealth has always been deeply political, and it cannot be reduced to purely economic mechanisms. . . . It is shaped by the way economic, social, and political actors view what is just and what is not, as well as by the relative power of those actors and the collective choices that result. It is the joint product of all relevant actors combined. . . . How this history plays out depends on how societies view inequalities and what kinds of policies and institutions they adopt to measure and transform them. (Piketty 2014: 20, 35)

1. In particular, the historical data collection project on which my book is based directly follows the pioneering works by Simon Kuznets (1953) and Anthony Atkinson and Alan Harrison (1978).
2. Classic references include Simiand (1932), Labrousse (1933), Bouvier, Furet, and Gilet (1965) and Daumard (1973).



To put it another way: “In a sense, both Marx and Kuznets were wrong. There are powerful forces pushing alternatively in the direction of rising or shrinking inequality. Which one dominates depends on the institutions and policies that societies choose to adopt” (Piketty and Saez 2014: 842–43).

The role of political shocks and changing representations of the economy is especially obvious when one studies inequality dynamics during the twentieth century. In particular, “the reduction of inequality that took place in most developed countries between 1910 and 1950 was above all a consequence of war and revolution and of policies adopted to cope with these shocks. Similarly, the resurgence of inequality after 1980 is due largely to the opposite political shifts of the past several decades, especially in regard to taxation and finance” (Piketty 2014: 20). In her review, Sylvia Yanagisako rightly notes the period of decreasing wealth inequality between 1910 and 1970 was also “an exceptionally formative period of social science scholarship,” and that many of the most influential models of modern capitalist society were indeed “forged in this period.” In my view, this is why it is particularly important to readdress these issues and question these models today.

I also try to show that beliefs systems about the distribution of income and wealth matter a great deal if one wants to understand the structure of inequality in the eighteenth and nineteenth centuries, and indeed in any society. Each country has its own intimate history with inequality, and I attempt to show that national identities play an important role in the two-way interaction between inequality dynamics and the evolution of perceptions, institutions, and policies. Throughout the book, I emphasize the importance of educational institutions (in particular the extent of equal access to high-quality schools and universities) and fiscal institutions (especially the chaotic advent of progressive taxation of income, inheritance, and wealth). I also refer to a large number of other institutions and public policies that play a substantial role in my historical account of inequality dynamics across three centuries and over twenty countries. This includes: monetary regimes, central banking, and inflation; labor market rules, minimum wages, and collective bargaining; forced labor (slavery); colonialism, wars, and revolutions; expropriations, destructions, and privatizations; corporate governance and stakeholder rights; rent control, and other price controls limiting the power of owners (such as the prohibition or limitation of usury); financial deregulation and capital flows; trade policies; family transmission rules and legal property regimes; fertility policies; and many others.

I should make very clear, however, that many of these institutions are not analyzed as completely as they should. For instance, the role of unions and wage bargaining—analyzed mostly in chapter nine—should have been dealt with in more detailed manner. The issues of working conditions, precarious labor and the construction of labor market status, together with their interaction with property relations, are not sufficiently addressed. I will mention other important limitations below.

## **A multidimensional history of capital, power, and inequality**

Let me now turn to my approach to capital. I attempt to develop a multidimensional approach to the history of capital and the power and inequality relations that come with the different forms of capital assets. Theoretical economic models

of capital accumulation, abstract concepts, and equations (such as the inequality  $r > g$ , to which I briefly return below) do play a certain role in my analysis. However this is a relatively modest role—as I believe the role of theory should generally be in the social sciences—and it should certainly not be exaggerated. By oversimplifying the real world up to an extreme point, formal models can sometime contribute to clarify certain interesting logical relationships between particular assumptions and conclusions. This can be useful, but only if one does not overestimate the meaning of this kind of abstract operation. One also needs to keep in mind that all economic concepts, irrespective of how “scientific” they pretend to be, are intellectual constructions that are socially and historically determined, and that are often used to promote certain views, values, or interests. Models are a language that can be useful only if it is solicited together with other forms of expressions, and if one recognizes that we are all part of the same conflicting, deliberative process.

In particular, let me say very clearly that the notion of an aggregate capital stock and of an aggregate production function are pretty abstract concepts. From time to time I refer to them in my analysis. But I certainly do not believe that such grossly oversimplified economic concepts can provide an adequate description of the production structure and the state of property and social relations for any society. One central reason why my book is relatively long is because I try to offer a detailed, multidimensional history of capital and its metamorphosis. Capital ownership takes many different historical forms, and each of them involves different forms of institutions, rules, and power relations, which must be analyzed as such.

As I explain in chapter one, when I define capital and wealth,

Capital is not an immutable concept: it reflects the state of development and prevailing social relations of each society. . . . The boundary between what private individuals can and cannot own has evolved considerably over time and around the world, as the extreme case of slavery indicates. The same is true of property in the atmosphere, the sea, mountains, historical monuments, and knowledge. Certain private interests would like to own these things, and sometimes they justify this desire on grounds of efficiency rather than mere self-interest. But there is no guarantee that this desire coincides with the general interest. (Piketty 2014: 47)

The fact that capital ownership and property rights are historically determined is particularly clear when I study the role of slave capital in the Southern United States before 1865, which can be viewed as the most extreme form of ownership and domination of owners over others (chapter four). This is also evident when I examine (in chapter five) the lower stock market capitalization of German companies relative to their Anglo-American counterparts, a phenomenon that is certainly related to the fact that German shareholders need to share power with other stakeholders (workers, governments, NGOs, etc.) a little more than in other countries. This apparently is not detrimental to their productive efficiency and exporting performance, which illustrates the fact that the market and social values of capital do often differ.



More generally, I attempt to analyze the diversity of the forms taken by capital assets and the problems raised by property relations and market valorizations throughout history. I study in some length the many transformations in the nature of capital assets, from agricultural land to modern real estate, business, and financial capital. Each type of asset has its own particular economic and political history and gives rise to different bargaining processes, power struggles, economic innovations, and social compromises between the different productive forces.

For instance, large upward or downward movements of real estate prices play an important role in the evolution of aggregate capital values during recent decades, as they did during the first half of the twentieth century (see in particular chapters three to six). This can in turn be accounted for by a complex mixture of institutional and technological forces, including the evolution of rent control policies and other rules regulating relations between owners and tenants, the transformation of economic geography, and the changing speed of technical progress in the transportation and construction industries relative to other sectors. In her review, Jane Guyer rightly emphasizes that the evolution of housing and land values is the product of many complex social, political, and cultural processes, and cannot be explained by a one-dimensional economic model of capital accumulation.

To take another example: I study in various parts of the book the importance of oil capital and its world distribution, with special emphasis on the power relations and military protections that go with it (in particular in the Middle East), as well on the consequences for the financial investment strategies followed by the corresponding sovereign wealth funds (particularly in chapter twelve).

The hypertrophy of gross financial asset positions between countries, which is one of the main characteristics of the financial globalization process of recent decades, is another recurring theme, from chapter one to chapters five, twelve, fifteen, and sixteen. I analyze the very large magnitude of the net foreign assets positions reached by Britain and France at the height of their colonial empires, and I compare them to today's net positions of China, Japan, or Germany. I repeatedly stress that international property relations—the fact that some countries own significant parts of other countries—are particularly complicated to regulate in a peaceful and democratic manner and often involve violent political conflicts. This was certainly true during the colonization and decolonization period, and could happen again in the future.

The hypertrophy of gross financial asset positions—the fact that countries and corporations now own each other as financial assets, and that the magnitude of these cross-ownership patterns has reached unprecedented historical proportions in recent decades (see chapter five)—has also changed the nature of corporate relations. In particular, financial deregulation and globalization has blurred the distinction between nonfinancial and financial corporations. In her review, Karen Ho rightly points out that I should have put more emphasis on this crucial transformation.

Public capital, sometime positive and sometime negative, depending on the changing patterns and complex political histories of public investment and deficit trajectories, nationalization, and privatization policies, also plays a critical role in the book (see especially chapters three and four). I emphasize the sharp

dissimilarities in country experiences (contrasting in particular the cases of Britain and France in the eighteenth and nineteenth centuries), as well as the commonalities (such as the historically large level of public capital in the postwar period, and the large decline in recent decades in rich countries as well as in Russia or China, with important consequences on the distribution of private wealth and the rise of new forms of oligarchs).

Throughout the book, I stress that the history of capital and inequality is multidimensional and involves a large variety of institutional compromises. The fact that it is technically possible to add up all the market values of the different existing assets (to the extent that such market values are well defined, which is not always entirely clear) in order to compute the aggregate value of the capital stock  $K$  does not change anything to this basic multidimensional reality of assets and corresponding property relations. I attempt to show that this abstract operation can be useful for some purposes. In particular, by computing the ratio  $\beta=K/Y$  between the aggregate market value of capital  $K$  and national income  $Y$ , one can compare the overall importance of capital wealth, private property, and public property in societies that are otherwise impossible to compare. For instance, one finds that in spite of all metamorphosis in the nature of assets and institutional arrangements, aggregate capital values—expressed in years of national income—are approaching in a number of countries the levels observed in the patrimonial societies that flourished in the eighteenth and nineteenth centuries and until World War I. I believe that this is an interesting finding in itself. But at the same time this certainly does not alter the fact that a proper comparison of these different societies requires a careful separate analysis of the various asset categories and corresponding social and economic relations.

Let me make clear that my book is at most an introduction to a multidimensional history of capital and power. Many important dimensions are not sufficiently dealt with. In his review, Michael Ralph points out that I do not sufficiently emphasize the key role played by the legal system in determining the boundaries of capital (he gives the telling example of the regulation and deregulation of the profit-making business of life insurance since the eighteenth century). Although I do analyze a number of ways in which the legal system shapes property relations (see for instance the above discussion regarding slavery, rent control, corporate governance, and worker rights), I certainly agree that some other crucial dimensions of the legal system and how it impacts property relations are missing.

Let me also point out that the geography of capital—including the north-south, city-countryside, core-periphery dimensions—should have been addressed more explicitly. For instance, one of the key structural changes pointed out in my book is the transformation of top elites from land to urban centers. This geographical dimension should again have been stated in a more explicit manner.

### **On the notion of social class in *Capital in the twenty-first century***

Another important way in which the history of capital and inequality that I develop in my book is multidimensional is the following: Throughout the book, I constantly distinguish between the inequality of labor income and the inequality of capital



ownership. Of course these two dimensions of inequality do interact in important ways—for example, rising inequality in labor earnings at a given point in time may tend to fuel rising wealth concentration in following decades or generations. But they largely involve different forces and social hierarchies. “In the case of unequal incomes from labor, these mechanisms include the supply of and demand for different skills, the state of the educational system, and the various rules and institutions that affect the operation of the labor market and the determination of wages. In the case of unequal incomes from capital, the most important processes involve savings and investment behavior, laws governing gift-giving and inheritance, and the operation of real estate and financial markets.” (Piketty 2014: 243). In particular, the notions of top deciles or percentiles are not the same for the distributions of labor income and capital ownership. In certain societies they might be highly correlated, and sometimes they represent entirely different social hierarchies, in particular in traditional patrimonial societies. The extent to which these two dimensions differ gives rise to different representations and beliefs systems about social inequality, which in turn shape institutions and public policies affecting inequality dynamics.

Is it possible to define social classes by using deciles and percentiles? In my book, I compare two different hierarchies of social groups: one hierarchy is defined by the distribution of labor income (I typically distinguish between the bottom 50 percent labor incomes, the middle 40 percent, and the top 10 percent, sometimes with special emphasis on smaller groups within the top 10 percent, particularly the top 1 percent or top 0.1 percent); and the other hierarchy is defined by the distribution of capital property (I again distinguish between the bottom 50 percent property owners, the middle 40 percent, and the top 10 percent, with special emphasis on smaller groups such as the top 1 percent or top 0.1 percent). I stress that traditional agrarian societies were mostly based upon the patrimonial hierarchy, and that to large extent this situation prevailed during the nineteenth century until World War I. In the future, it could be that a new form of inequality structure emerges, combining the return of sharp patrimonial hierarchies together with the development of large labor hierarchies and strong culpabilizing discourses for those who lose on both dimensions. Extreme inequality in income and wealth can fuel extreme inequality in access to intellectual and symbolic capital (e.g., the average income of the parents of Harvard university students corresponds to the average income of top 2 percent of the US distribution of family income; see chapter thirteen). At the same time we see the rise of what I refer to as “meritocratic extremism” (a set of strong statements about the fact the losers deserve to lose, so to speak). The future structure of inequality might bring together extreme forms of domination based simultaneously on property and culture (in brief: Marx and Bourdieu reconciled).

I attempt to show that social classes should be analyzed as power and production relations between social groups, not just as deciles in statistical distributions. In my view, deciles and percentiles should be viewed as a language allowing for comparisons between societies that are otherwise impossible to compare, such as France in 1789 and China or the United States in 2014, in particular because they themselves used very different languages to describe their social classes. In some cases, one may need to further refine the classification and analyze smaller

groups. For instance, it would be useful to distinguish the very poor (say, the bottom 10 percent) within the bottom 50 percent, which I do not do in my book.

Also, the different groups could and should be broken down by sector, age, and gender. In her review, Sylvia Yanagisako rightly points out that the need of further “gendering” my analysis of inequality. I fully agree on this point. Gender issues are not entirely absent from the book. I stress the role of marriage patterns for the transmission of wealth inequality. I refer repeatedly to the fact that the property regimes instituted in the eighteenth and nineteenth centuries were based on the exclusion of women from these rights. When I analyze fertility differentials between countries, I also stress the role of policies in favor of gender equality (see chapter two). However it is clear that the gender dimension of inequality should have been addressed in a more systematic and explicit manner. Some of the available historical data sources allow for renewed analysis of gender inequality in important ways, and this should be considered as a central priority for future research.<sup>3</sup>

### The regulation and politics of capital

In *Capital in the twenty-first century*, I attempt to develop a historical and political economy approach to institutional change and inequality dynamics. I particularly stress the interaction between economic forces and institutional responses (especially in the area of educational, labor, and fiscal institutions).

I should stress, however, that the political forces explaining institutional change could and should be analyzed in a more systematic manner. I do try to analyze some of these forces but this is clearly insufficient. I stress the role of national identities and country-specific narratives about inequality and economic development (for instance the role of comparison with other countries in the emergence of the Anglo-American conservative revolution of the 1980s). I also attempt to emphasize the role of political conflict. In particular, wars and revolutions play a large role in my account of inequality dynamics and institutional change in the twentieth century. Of course, I agree that democratic forces and the extension of suffrage also played an important role in the rise of more inclusive social, educational, and fiscal institutions during the nineteenth and twentieth centuries. But many of the most important changes did not come simply from the steady forces of peaceful electoral democracy: rather, specific historical events and political shocks often played an important role. This is evident if one looks at the chaotic evolution of progressive income and inheritance taxation over the 1900–2010 period. In particular, there is little evidence of a natural movement toward more progressive taxation until the violent military, political, and ideological shocks induced by World War I. Beliefs systems and collective representations about social inequality and the role of government were deeply affected by World War I and the rise of communism, as they were by the Great Depression and World War II (and then at the end of the twentieth century by the stagflation of the 1970s and the fall of the Soviet Union). Before

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3. For instance, historical inheritance files can be used to study the interplay between the evolution of matrimonial property regimes and within-household inequalities. See Piketty, Postel-Vinay, and Rosenthal (2006, 2014).





World War I, most of the European elite refused the social and fiscal reforms, and these reforms were finally adopted in the wake of the 1914–1945 shocks.

It is particularly interesting to note that the French elite was very strongly opposed to the creation of a progressive income tax until 1914, and that in order to justify their conservatism they were often referring to the French revolution. In their view, France had become equal after 1789 thanks to the end of aristocratic privileges and the development of well-protected property rights for the entire population. Since everybody was made equal under the right of property, there was no need for progressive taxation (which would be suitable for aristocratic Britain, the story goes, but not for republican France). What I find particularly striking in this pre-1914 debate is the combination of strong beliefs in property-rights-centered institutions and an equally strong denial of high inequality. One key finding of my historical research is that wealth inequality was in fact as extreme in France as in Britain at the eve of World War I (with about 90 percent of total wealth for the top 10 percent, and 60–70 percent for the top 1 percent). In my book, I try to understand what we can learn from the fact that wealth inequality was as large in France in 1914 as it was in 1789, and also from the fact that much of the elite was trying to deny this. I believe there are important warning lessons for today. In particular, it would be a mistake to believe that the forces of modern growth and competitive markets are sufficient to address these challenges. Pre-1914 France or Britain were not static, agrarian societies: this is the time when major innovations occurred, such as the automobile, the electricity, the radio, financial globalization, et cetera; these innovations were arguably at least as important as today's innovations in information technologies. However this was not sufficient to prevent extreme concentration of income and wealth to prevail, in large part due to the  $r-g$  logic. That is, modern industrial growth did not substantially affect the gap between the rate of return to capital and the economy's growth rate. This high and persistent differential between  $r$  and  $g$  seems to be an important part of the explanation for the very high and persistent level of wealth concentration that we observe in most societies in the eighteenth and nineteenth centuries and until World War I.

Of course I am not saying that it will always take wars, revolutions, and other violent political shocks to make institutional changes happen. Beliefs systems and resulting perceptions and policies can also be affected by peaceful public discussion. For instance, rising inequality in the United States or at the global level might generate appropriate policy responses in the near future. However, we should not take this for granted. It is important to recognize the role of political conflict in the history of inequality and institutional change. It often took major fights to deliver change in the past, and it is not impossible that it will be the same in the future.

Finally, I should again make clear that my historical and political approach to inequality and institutions should be viewed as exploratory and incomplete. In particular, I say far too little about how new forms of social movements and political mobilizations will give rise to institutional change in the future. Also, I tend to devote too much attention to progressive taxation and too little attention to a number of other important institutional evolutions, such as the development of alternative forms of property arrangements and participatory governance. One central reason why progressive capital taxation is particularly important is because

it can also bring increased transparency about company assets and accounts. In turn, increased financial transparency can help to develop new forms of governance (for instance it can facilitate more worker involvement in company boards). I should have explained more clearly that this evolution, if it happens, is only one of the many institutional changes that would allow democracy to regain control of capitalism.

The last chapter of my book concludes as follows: “Without real accounting and financial transparency and sharing of information, there can be no economic democracy. Conversely, without a real right to intervene in corporate decision-making (including seats for workers on the company’s board of directors), transparency is of little use. Information must support democratic institutions; it is not an end in itself. If democracy is someday to regain control of capitalism, it must start by recognizing that the concrete institutions in which democracy and capitalism are embodied need to be reinvented again and again.” (Piketty 2014: 570). As Anush Kapadia rightly points out in her review, the fact that I do not push this line of investigation much further is certainly one of the key shortcomings of my work. In my view, this is probably the main reason why my book is at best an introduction to the study of capital in the twenty-first century.

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