A Historical Approach to Property, Inequality and Debt: Reflections on *Capital in the 21st Century*

Thomas Piketty*

In my book *Capital in the 21st Century*, I attempt to develop a historical approach to property, inequality and debt. Thanks to the cumulative efforts of several dozen scholars, we have been able to compile a relatively large historical database on the structure of national income and national wealth as well as the evolution of income and wealth distributions, covering three centuries and over 20 countries. Our efforts effectively represent an extension, on a larger scale, of the pioneering historical data collection work of Simon Kuznets and Tony Atkinson (see Kuznets 1953; Atkinson and Harrison 1978). My first objective in this book is to present this body of historical evidence, and to try to analyse the many economic, social and political processes that may account for the various evolutions seen in different countries since the Industrial Revolution (see Piketty and Saez 2014, for a brief summary of some of the main historical facts). I stress from the beginning that we have too little historical data at our disposal to be able to draw definitive judgments. On the other hand, we do have access to substantially more evidence than we used to. Imperfect as it is, I hope this work can contribute to putting the study of distribution and a longer-term perspective back at the center of economic thinking.

In this article, I will clarify a number of implications of my findings, and attempt to respond to some of the very interesting comments made by Clemens Fuest, Andreas Peichl, Debraj Ray, Ton van Schaik, Christoph Schinke, Till van Treeck, and Daniel Waldenström about my book. I am particularly grateful to Schinke and van Treeck for informing me of some of the debates over my book in Germany. Sadly, my understanding of the German language is not good enough to allow me to follow these discussions more closely, which is unfortunate, given the importance of the German public debate for our common future in the European Union, and particularly in the Eurozone.

* Paris School of Economics.

This article begins by clarifying the role played by r > g in my analysis of wealth inequality. This is followed by a discussion of some of the implications for optimal taxation of inheritance, property and wealth. Finally, I analyse the relation between capital-income ratios and capital shares, and stress the need for a multidimensional approach to capital assets, which I try to develop in my book. In conclusion I present some of the lessons that can be drawn from the history of public debt, and which, in my view, can fruitfully inform the current debate over the Eurozone public debt crisis.

Inequality of wealth vs inequality of labour income

One central reason why my book is relatively long is because the history of the distribution of income and wealth is complicated. The dynamics of inequality involve many different economic, social, political and cultural processes, several of which are often operating at the same time within a given country. In my analysis, the size of the gap between r and g, where r is the rate of return on capital and g the economy's growth rate, is one of the important forces that can account for the historical magnitude and variations in wealth inequality. In particular, I have come to the conclusion that the existence of a large gap between r and g may help to explain why wealth inequality was so extreme and persistent in almost every society up until World War I (see *Capital in the 21st Century*, Chapter 10).

That said, the way in which I perceive the relationship between r > g and inequality is often not well captured in the discussion that has surrounded my book. For example, I do not view r > g as the only, or even the primary tool, for considering changes in income and wealth in the 20th century, or for forecasting the path of inequality in the 21st century. Institutional changes and political shocks – which to a large extent can be

viewed as endogenous to the inequality and development process itself – played a major role in the past, and will probably continue to do so in the future.

Indeed, the main conclusion of my analytical historical narrative is stated in the introduction of the book (p. 20), that: "one should be wary of any economic determinism regarding 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 equitable 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". As I wrote in a follow-up essay with a co-author: "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, 842–843).

More specifically, I certainly do not believe that $r > g$ is a useful tool for discussing rising inequality in labour income: other mechanisms and policies are much more relevant here, e.g. supply and demand of skills and education. For instance, I point out in my book (particularly in Chapters 8 and 9) that the rise in top income shares in the United States over the 1980–2010 period is mostly due part to rising inequality in labour earnings, which can be explained, in turn, by a mixture of three groups of factors: firstly, rising inequality in access to skills and to higher education over this time period in the United States, an evolution which might have been exacerbated by rising tuition fees and insufficient public investment; secondly, exploding top managerial compensation, itself probably stimulated by changing incentives and norms, and by large cuts in top tax rates (see also Chapter 14; Piketty, Saez and Stantcheva 2014); thirdly, changing labour market rules and bargaining power, particularly due to declining unions and a falling minimum wage in the United States (see Chapter 9, Figure 9.1). In any case, whatever the relative weight one chooses to attribute to each factor, it is obvious that this rise in labour income inequality in recent decades has little to do with $r – g$ gap.

$r > g$ and the amplification of wealth inequality

I would like to clarify the exact role played by $r > g$ in my analysis of the long-run level of wealth inequality. Specifically, the model that I have in mind is one where a higher $r – g$ gap will tend to greatly amplify the steady-state inequality of a wealth distribution that arises out of a given mixture of shocks (including labour income shocks).

Let me first say very clearly that $r > g$ is certainly not a problem in itself. Indeed, the inequality $r > g$ holds true in the steady-state equilibrium of most common economic models, including representative-agent models where each individual owns an equal share of the capital stock. For instance, in the standard dynastic model where each individual behaves as an infinitely lived family, the steady-state rate of return is well known to be given by the modified 'golden rule' $r = \theta + \gamma g$ (where θ is the rate of time preference and γ is the curvature of the utility function). For example, if $\theta = 3$ percent, $\gamma = 2$, and $g = 1$ percent, then $r = 5$ percent. In this framework, the inequality $r > g$ always holds true, and does not have any implication with regard to wealth inequality.[1]

In a representative-agent framework, what $r > g$ means is simply that in steady-state each family only needs to reinvest a fraction g/r of its capital income in order to ensure that its capital stock will grow at the same rate g as the size of the economy, and the family can then consume a fraction $1-g/r$. For example, if $r = 5$ percent and $g = 1$ percent, then each family will reinvest 20 percent of its capital income and can consume 80 percent. This tells us nothing at all about inequality: this is simply saying that capital ownership facilitates higher consumption levels – which is really the very least one can expect of capital ownership.

Indeed, as is rightly pointed out by Ray in his paper, $r > g$ corresponds to a standard 'dynamic efficiency' condition in standard economic models. In contrast, the inequality $r < g$ would correspond to a situation that economists often refer to as 'dynamic inefficiency': in effect, one would need to invest more than the return on capital in order to ensure that one's capital

[1] Intuitively, in a model where everyone maximizes an infinite-horizon utility function $U = \int_{0 \le t \le +\infty} e^{-\theta t} u(c_t)$ (with $u(c) = c^{1-\gamma}/(1-\gamma)$), then $r = \theta + \gamma g$ is the unique rate of return to capital possible in the long-run for the following reason: it is the sole rate such that the agents are willing to rise their consumption at rate g, that is at the growth rate of the economy. If the return is higher, the agents prefer to postpone their consumption and accumulate more capital, which will decrease the rate of return; and if it is lower, they want to anticipate their consumption and borrow more, which will increase the rate of return.

stock keeps rising as fast as the size of the economy. This would correspond to a situation of excessive capital accumulation from a social and economic efficiency standpoint.[2]

So what is the relationship between r – g and wealth inequality? To answer this question, one needs to introduce extra ingredients into the basic model, so that inequality arises in the first place.[3] In the real world, many shocks to the wealth trajectories of families can contribute to making wealth distribution highly unequal (indeed, in every country and time period for which we have data, wealth distribution *within each age group* is substantially more unequal than income distribution, which is difficult to explain with standard life-cycle models of wealth accumulation). There are demographic shocks: some families have many children and have to split inheritances in many pieces, some have few; some parents die late, some die soon, and so on. There are also shocks to rates of return: some families make good investments, others go bankrupt. There are shocks to labor market outcomes: some earn high wages, others do not. There are differences in taste parameters that affect the level of saving: some families consume more than a fraction 1-g/r of their capital income, and may even consume the full capital value; others may reinvest more than a fraction g/r and have a strong taste for leaving bequests and perpetuating large fortunes.

A central property of this large class of models is that, for a given structure of shocks, the long-run magnitude of wealth inequality will tend to be magnified if the gap r – g is higher. In other words, wealth inequality will converge towards a finite level. The shocks will ensure that there is always some degree of downward and upward wealth mobility, so that wealth inequality remains bounded in the long run. But this finite inequality level will be a steeply rising function of the gap r – g. Intuitively, a higher gap between r and g works as an amplifier mechanism for wealth inequality, for a given variance of other shocks. In other words: a higher gap between r and g facilitates a sustained level of wealth inequality that is higher and more persistent over time (i.e. a higher gap r – g leads both to higher inequality ad lower mobility). Technically, it can be shown that if shocks take a multiplicative form, then the inequality of wealth converges toward a distribution that has a Pareto shape for top wealth holders (which is approximately the form observed in real world distributions, and which corresponds to relatively fat upper tails and large concentration of wealth at the very top), and that the inverted Pareto coefficient (an indicator of top-end inequality) is a steeply rising function of the gap r – g. The logic behind this well-known theoretical result (which was established by many authors using various structure of demographic and economic shocks; see in particular Stiglitz 1969) and this 'inequality amplification' impact of r – g is presented in Chapter 10 of my book.[4]

The important point is that in this class of models, relatively small changes in r – g can generate large changes in steady-state wealth inequality. Simple simulations of the model with binomial taste shocks, for instance, show that going from r – g = 2 percent to r – g = 3 percent is sufficient to move the inverted Pareto coefficient from b = 2.28 to b = 3.25. Taken literally, this corresponds to a shift from an economy with moderate wealth inequality – with a top 1 percent wealth share of around 20–30 percent, for instance, like present-day Europe or the United States – to an economy with very high wealth inequality with a top 1 percent wealth share of around 50–60 percent, like pre-World War 1 Europe.[5]

The micro-level evidence available on wealth dynamics confirms that the high gap between r and g is one of the central reasons why wealth concentration was so high during the 18th–19th centuries and up until World War 1 (see Chapter 10; and also Piketty, Postel-Vinay and Rosenthal 2006 and 2014). During the 20th century, a very unusual combination of events transformed the relation between r and g (large capital shocks during the 1914–1945 period, including destruction, nationalization, inflation; high growth dur-

[2] As is well known, r < g cannot happen in infinite-horizon models with no shock and perfect capital markets. This is because r < g would violate the transversality condition: the net present value of future resources would be infinite, so that rational agents would borrow infinite amounts in order to consume right away, until r rises above g. However, in models with other saving motives, such as finite-horizon overlapping generation models, it is possible to have r < g and excessive capital accumulation.

[3] In the dynastic model with no shock, there is no force generating inequality out of equality (or equality out of inequality), so any initial level of wealth inequality (including full equality) can be self-sustaining, as long as the modified golden rule is satisfied. It is worth noting, however, that the magnitude of the gap r – g has an impact on the steady-state inequality of consumption and welfare: if r – g is small then high-wealth dynasties need to reinvest a large fraction of their capital income, so that they do not consume much more than low wealth dynasties.

[4] For references to this literature on dynamic wealth accumulation models with random shocks, see the on-line appendix to Chapter 10 available at piketty.pse.ens.fr/capital21c. See also Piketty and Zucman (2015, Section 5.4).

[5] In the special case with binomial saving taste shocks with probability p, one can easily show that the inverted Pareto coefficient is given by $b = \log(1/p)/\log(1/\omega)$, with $\omega = s\, e^{(r-g)H}$ (s is the average saving taste, r and g are the annual rate of return and growth rate, and H is generation length) – see Piketty and Zucman (2015, Section 5.4) for simple calibrations. Atkinson, Piketty and Saez (2011, Figures 12–15) provide evidence on the long-run evolution of Pareto coefficients.

ing the reconstruction period and demographic transition). In the future, several forces may widen the r – g gap (particularly the slowdown in population growth, and rising global competition to attract capital) and higher wealth inequality. But ultimately which forces will prevail is relatively uncertain. In particular, this depends on the institutions and policies that will be adopted.

What can we learn from cross-country regressions on inequality and r – g ?

Let me now ask the following question: what can we learn from cross-country regressions between wealth inequality and r – g ? Let me first stress again that r – g is one of many different economic, social and political mechanisms that plays an important role in inequality dynamics. Therefore, it is important to control for these other factors if one wants to be able to isolate the impact of r – g.

From that viewpoint, the cross-country regressions presented in their paper by Fuest, Peichl and Waldenström (who find that a higher r – g gap seems to lead to higher wealth inequality) strike me as more sophisticated and potentially more convincing that the cross-country regressions that were recently presented by Acemoglu and Robinson. There are several reasons for this: Fuest-Peichl-Waldentrom explicitely use wealth inequality measures, they control for income inequality and other factors, and they introduce substantial time lags.

In particular, one central factor which makes the Acemoglu-Robinson regressions particularly unconvincing is that they regress income inequality (rather than wealth inequality) on r – g. This is most problematic, since income inequality is primarily determined by the inequality of labour income (which typically represents between two thirds and three quarters of total income), which as I noted above has nothing to do with r – g, and is determined by completely different factors (supply and demand for skills, educational institutions, labour market rules, corporate governance, etc.). It makes more sense to run such a regression with wealth inequality (controlling for labour income inequality), which is what Fuest-Peichl-Waldenström attempt to do. In addition, the process of intergenerational accumulation and the distribution of wealth is a very long-run process, so looking at cross-sectional regressions between income inequality and r – g (which is what Acemoglu and Robinson do, i.e. they regress income inequality at a given time t on the r – g gap at this same time t) is not very meaningful. Using 15-year time lags – the method used by Fuest-Peichl-Waldenström – looks potentially more promising. The fact that they find statistically significant effects going in the right direction (according to the theoretical model) also seems promising.

I should stress, however, that I am not sure whether there is a lot to learn at this stage from running explicit cross-country regressions between wealth inequality and r – g. In particular, it may well be necessary to introduce time lags over much longer time periods: the processes of wealth accumulation and transmission typically spans several generations, so it would perhaps be better to use the average r – g observed during the 30 or 50 years. The broad correlations between r – g and wealth inequality certainly seem to run in the right direction, both from a long run (18th–19th vs 20th centuries) and international (Europe vs United States) perspective. One problem with going beyond this observation is that there are relatively few countries with homogenous long-run series on wealth inequality, which makes it very difficult to run regressions. We are in the process of extending the 'World Top Incomes Database' (WTID) to a more ambitious 'World Wealth and Income Database' (W2ID) including a wealth distribution series for more countries, so this difficulty may be overcome in the future.[6] But given the data limitations and the time lag specification problems that we currently face, I feel somewhat sceptical about running cross-country regressions.

In my view, a more promising approach – to this issue as well as many others – is a mixture of careful case studies and structural calibrations of theoretical models. Although we do not have many historical series on wealth inequality, they show a consistent pattern. Namely, we observe extremely high concentration of wealth in almost every European society in the 18th and 19th centuries, up until World War I. In particular, in France, Britain and Sweden, the top 10 percent wealth share accounted for about 90 percent of total wealth (including the top 1 percent wealth share around 60–70 percent) in the 19th century and at the very beginning of the 20th century. If anything, wealth inequality seems to have risen somewhat during the

[6] It is also worth noting that we generally do not have separate series for top labour income shares and top capital income shares, which is what would be needed to run this kind of regression. In other words Fuest-Peichl-Waldenström control for top income shares, while ideally one would only like to control for top labour income shares on the right-hand side of the wealth inequality vs r – g regression.

19th century and up until World War I – or perhaps to have stabilised at very high levels in around 1890–1910. Thus, despite major changes in the nature of wealth during the 19th century – agricultural land as a form of wealth is largely replaced by real estate, business assets and foreign investment – wealth inequality was as extreme in the modern industrial society of 1914 as it had been under France's *ancien regime* in 1789. The most convincing explanation for the very high wealth concentration in these pre-World War I European societies seems to be the very large r – g gap – that is, the gap between rates of return and growth rates during the 18th and 19th centuries. There was very little taxation or inflation up until 1914, so the gap (1-t)r – g was particularly high in pre-World War 1 societies, which in dynamic models of wealth accumulation with random shocks leads to very large wealth concentration. In contrast, following the large capital shocks of the 1914–1945 period – a time of physical destruction, periods of high inflation and taxation, and nationalizations – the after-tax, after-capital-losses rate of return fell precipitously below growth rates after World War I (see Chapter 10; Figure 10.9 compares the pre-tax pure rate of return with growth rate g, while Figures 10.10-10.11 show a post-tax, post-capital-losses rate of return).

I have already argued above that this interpretation of the evidence is further confirmed by the detailed individual-level data collected in French inheritance archives since the time of the French revolution (Piketty, Postel-Vinay and Rosenthal 2006 and 2014). In particular, we find in this research that the age-wealth profiles rise increasingly steeply at high ages in the 19th century and early 20th century (individuals aged 70 to 79 years old are, on average, a lot wealthier than individuals aged 60 to 69, those aged 80 to 89 are a lot wealthier than those aged 70 to 79, and so on), and that this can be well accounted for by a capitalisation effect and a high gap between (1-t)r and g. Indeed, it is very difficult to account for the observed dynamics of the age-wealth profile with a different mechanism (since there is limited labour income at high ages). This age-wealth pattern suddenly breaks down following the 1914–1945 capital shocks. The fact that wealth concentration in the United States was significantly lower than in Europe during the 19th century and up until World War I is also consistent with this model: growth rates were higher in the US economy, which was particularly due to higher population growth, thereby limiting the dynamic cumulative effects of the inequality amplification channel. There had also been less time for dynastic wealth concentration to arise in the US economy by the 19th century. This evidence is further reviewed in Chapters 10–11 of my book.

Data collection will continue, and new data will certainly allow for better empirical tests of structural models of wealth accumulation and distribution in the future. At this stage, however, the best evidence available suggests that r > g is an important part of the explanation for the very high and persistent level of wealth concentration observed in most societies in the 18th-19th centuries and until World War I. Although it is very difficult to predict how the gap r – g will change in the future, there are good reasons to believe that this mechanism might become relevant once again.

On the optimal progressive taxation of income, wealth and consumption

I now move to the issue of optimal taxation. The theory of capital taxation that I present in *Capital in the 21st Century* is largely based upon joint work with Emmanuel Saez (see in particular Piketty and Saez 2013). In this paper, we develop a model where inequality is fundamentally two-dimensional: individuals differ both in their labour earning potential and in their inherited wealth. Due to the underlying structure of demographic, productivity and taste shocks, these two dimensions are never perfectly correlated. As a result, the optimal tax policy is also two-dimensional: it involves a progressive tax on labour income and a progressive tax on inherited wealth. Specifically, we show that the long-run optimal tax rates on labour income and inheritance depend on the distributional parameters, the social welfare function, and the elasticities of labour earnings and capital bequests with respect to tax rates. The optimal tax rate on inheritance is always positive, except, of course, in the extreme case with an infinite elasticity of capital accumulation with respect to the net-of-tax rate of return (as posited implicitly in the benchmark dynastic model with infinite horizon and no shock). For realistic empirical values, we find that the optimal inheritance tax rate might be as high as 50–60 percent, or even higher for top bequests, in line with historical experience.[7]

In effect, what we do in this work is to extend the 'sufficient statistics' approach to the study of capital taxa-

[7] See Piketty and Saez (2013a), Figure 1–2 and Table 1. It is worth noting that the optimal inheritance tax rate can also be expressed as an increasing function of the gap r – g.

tion. The general idea behind this approach is to express those optimal tax formulas in terms of estimable 'sufficient statistics' including behavioural elasticities, distributional parameters, and social preferences for redistribution. Those formulas are designed to be robust to the underlying primitives of the model and capture the key equity-efficiency trade-off in a transparent way. This approach has been fruitfully used in the analysis of optimal labour income taxation (for a recent survey, see Piketty and Saez 2013b). We follow a similar route and show that the equity-efficiency trade-off logic also applies to the taxation of inheritance. This approach successfully brings together many of the existing scattered results from the literature.

Next, if we introduce capital market imperfections into our basic inheritance tax model, then we find that one needs to supplement inheritance taxes with the annual taxation of wealth and capital income. Intuitively, in the presence of idiosyncratic shocks to future rates of return, it is impossible to know the lifetime capitalised value of an asset at the time of inheritance, and it is optimal to split the tax burden between these different tax instruments. For instance, assume I received from my family an apartment in Paris worth 100,000 euros back in 1975. In order to compute the optimal inheritance tax rate, one would need to know the lifetime capitalized value of this asset. But of course, back in 1975, nobody could have guessed that this asset would be worth millions of euros in 2015, or estimated the annual income flows generated by this asset between 1975 and 2015. In such a model, one can show that it is optimal to use a combination of inheritance taxation and annual taxation of property values and capital income flows (Piketty and Saez 2013a).

One difficulty, however, is that optimal tax formulas soon become relatively complicated and difficult to calibrate. More specifically, the optimal split between annual taxes on wealth stock and annual taxes on capital income flows depends on the elasticity of rates of return with respect to taxation (i.e. the extent to which observed rates of return are sensitive to individual effort and portfolio decisions, as opposed to idiosyncratic, uninsurable shocks). Naturally, intertemporal substitution elasticities also play a role. Substantial additional research is necessary before we can provide a realistic, complete calibration of the optimal capital tax system (which involves a mixture of progressive taxes on inheritance, annual wealth holdings and annual capital income flows).

In my book, I propose a simple rule-of-thumb for thinking about optimal wealth tax rates. Namely, one should adapt the tax rates to the observed speed at which the different wealth groups are rising over time. For instance, if top wealth holders are rising at 6–7 percent per year in real terms (as compared to 1–2 percent per year for average wealth), as suggested by Forbes-type wealth rankings (as well as by recent research by Saez and Zucman 2014), and if one aims to stabilise the level of wealth concentration, then one might need to apply top wealth tax rates as large as 5 percent per year, and possibly higher (see Chapter 15; see also Chapter 12, Tables 12.1–12.2). Needless to say, the implications would be very different if top wealth holders were rising at the same speed as average wealth. One of the main conclusions of my research is indeed that there is substantial uncertainty about how far income and wealth inequality might rise in the 21st century, and that we need more financial transparency and better information about income and wealth dynamics, so that we can adapt our policies and institutions to a changing environment. This might require better international fiscal coordination, which is difficult, but by no means impossible (Zucman 2014). I will return to this issue below.

It is worth noting that an alternative to progressive taxation of inheritance and wealth that is often referred to in the public debate is the progressive consumption tax (see e.g. Gates 2014). This, however, is a highly imperfect substitute. Firstly, meritocratic values imply that one might want to tax inherited wealth more than self-made wealth, which is impossible to do with a consumption tax alone. Next, the very notion of consumption is not very well defined for top wealth holders: personal consumption in the form of food or clothes is bound to account for a tiny fraction of the consumption of individuals with large fortunes, who usually spend most of their resources on purchasing influence, prestige and power. When the Koch brothers spend money on political campaigns, should this be counted as part of their consumption? When billionaires use their corporate jets, should this be included in consumption? A progressive tax on net wealth seems more desirable than a progressive consumption tax for two reasons: firstly because net wealth is easier to define, measure and monitor than consumption, and secondly, because it is better indicator of the ability of wealthy taxpayers to pay taxes and to contribute to the common good (see Chapter 15).

Is it enough to have a progressive tax on immovable property?

In their paper, Fuest, Peichl and Waldenström argue that the progressive taxation of immovable property (real estate) might be a desirable policy for the future (particularly in Europe), but that the progressive taxation of net wealth (including financial assets and liabilities, and not only real estate assets) is impossible to implement and should be discarded. The basic argument is that real estate assets are impossible to dissimulate, while financial assets are difficult to monitor, particularly in a world of free capital flows. While I certainly agree that progressive taxation of immovable property is a lot easier to implement and might well be used more intensively by governments in the future (as illustrated, for instance, by the recent and bipartisan British move towards higher transaction taxes on real estate properties worth over 1 million British pounds),[8] I am not convinced that we should discard the idea of a comprehensive progressive tax on net wealth.

Firstly, it is possible to develop a system of automatic transmission of information about cross-border financial assets between international banking institutions and tax administrations. This is technically well within the reach of what the tax administrations of developed countries could do if there was a political will to do it (this is already what has been done within each county for a long time). In a way, this movement towards the international automatic transmission of information has already started to happen, and gathered impetus after the enactment of US sanctions against non-cooperative Swiss banks. Of course, there is still a long way to go. But there is ample evidence showing that it is possible to implement higher cross-border financial transparency in the not-too-distant future (Zucman 2014).

Next, assuming we can develop automatic information transmission systems for financial assets, there is really no sound economic rationale for taxing real estate assets more than financial assets. In practice, financial assets are nothing but claims on real assets, particularly on business assets (buildings, machinery, equipment, patents, etc.). All capital assets are useful, whether they are used to produce housing services or business services. There is no economic reason in general why the tax system should favour certain assets over others. Of course, this is not saying that sector-specific policies toward capital accumulation are never justified: in some cases, one certainly needs to change regulations regarding land use, construction permits, or R&D incentives. But when it comes to taxation, it is usually preferable to have a tax code that it is neutral with respect to the different asset categories.

Finally, the last reason why it would be a mistake – in my view – to discard progressive taxes on net wealth is a simple political economy argument. Middle class households tend to own a very large fraction of their wealth portfolio in the form of real estate, while high-wealth households typically own a much bigger fraction in the form of financial assets. By exempting financial assets and by taxing solely real assets, one is in effect introducing some strong regressivity component into the wealth tax system, which might be difficult to explain to the public, and particularly to the middle class electorate. A recent illustration is the attempt by the Monti government to introduce a property tax in 2012. The implicit tax rate was about 0.8 percent on real estate assets, and only 0.1 percent on financial assets (with many exemptions). In effect, someone with a few hundred thousand euros in real estate wealth was paying a much higher tax rate than someone with several million euros in financial wealth. This arguably contributed to the unpopularity of the tax and to its final repeal. Fiscal consent requires a minimal feeling of tax fairness.

Capital-income ratios vs capital shares: towards a multi-sector approach

One of the important findings of my research is that capital-income ratios $\beta = K/Y$ (where K is the market value of the sum of all capital assets, net of debt, and Y is national income) and capital shares $\alpha = Y_K/Y$ (where Y_K is the sum of all capital income flows: rent, profit, dividend, interest, etc.) tend to move together in the long run, particularly in recent decades, where both have been rising. In the standard one-good model of capital accumulation with perfect competition, the only way to explain why β and α move together is to assume that the capital-labour elasticity of substitution σ that is somewhat larger than one (which could

[8] A 5-percent tax rate on sales of property worth over 1 million British pounds was introduced in 2011, and a 7-percent tax rate on sales of property worth over 2 million British pounds in 2012. It would probably have been preferable to implement such a change without notches (see Best and Kleven 2015), and with an annual property tax, rather than a transaction tax. The point here, however, is that this might illustrate a more general move towards the more progressive taxation of immovable property (note that the reform was launched under a Labour government and pursued under a Conservative government).

be interpreted as the rise of robots and other capital-intensive technologies).[9]

Let me make clear, however, that this is not my preferred interpretation of the evidence. Maybe robots and high capital-labour substitution will be important in the future, but for the moment, the important capital-intensive sectors are more traditional sectors like real estate and energy. I believe that the right model to think about rising capital-income ratios and capital shares in recent decades is a multi-sector model of capital accumulation, with substantial movements in *relative* prices, and with important variations in bargaining power over time (see *Capital in the 21st Century,* Chapters 3–6). Large upward or downward movements of real estate prices have played an important role in the evolution of aggregate capital values in recent decades, as they did during the first half of the 20th centuries. This can, in turn, be accounted for by a complex mixture of institutional and technological forces, including 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 (see Chapters 3–6; and also Piketty and Zucman 2014). In practice, intersectoral elasticities of substitution combining supply and demand forces can often be much higher than within-sector elasticities (see e.g. Karababounis and Neiman (2014) on the role played by the declining relative price of equipment).[10] This multidimensional nature of capital creates substantial additional uncertainties regarding the future evolution of inequality, as illustrated by the examples of housing and oil prices. In my view, this reinforces the need for increased democratic transparency relating to income and wealth dynamics.

More generally, the main reason why my book is relatively long is because I try to offer a fairly detailed, multidimensional history of capital and its metamorphosis. Capital ownership takes many different historical forms, and each of them involves different forms of property relations and social conflict, which must be analysed as such. Throughout my book, I attempt to analyse the diversity of the forms taken by capital assets and the problems raised by property relations and market valorisations 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.

For example, 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, and also the most extreme form of intergenerational transmission of debt (Chapter 4). A similar theme also becomes evident when I examine the lower stock market capitalization of German companies compared 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, nongovernment organizations, and others) somewhat more than in other countries (Chapter 5). This power-sharing apparently is not detrimental to their productive efficiency and exporting performance, which illustrates the fact that the market and social values of capital can often differ.

Other examples involve real estate capital (which was already mentioned above) and natural resource wealth – like oil. The issue of oil capital and its world distribution is rooted in the power relations and military protections that go with it (particularly in the Middle East), as well in the implications for the financial investment strategies followed by the corresponding sovereign wealth funds (discussed in Chapter 12).

The institutional analysis of property relations and capital assets also has international and public-sector dimensions. 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 a recurring theme of the book (Chapters 1–5, 12, 15 and 16). I analyse the very large magnitude of the net foreign assets positions reached by Britain and France at the height of their colonial empires, and compare them to today's net positions of China, Japan or Germany. I repeatedly stress that international property relations – the fact that economic actors in some countries own significant claims on real and financial assets in other countries – can be par-

[9] With $Y= F(K,L) = [aK^{(\sigma-1)/\sigma} +(1-a)L^{(\sigma-1)/\sigma}]^{\sigma/(\sigma-1)}$, the marginal productivity of capital is given by $r = F_K = a\,(Y/K)^{1/\sigma} = a\beta^{-1/\sigma}$, and the capital share is given by $\alpha = r\beta = a\beta^{(\sigma-1)/\sigma}$. See Piketty and Zucman (2014 and 2015).

[10] As argued by van Schaik in his paper, introducing different vintages of capital can also contribute to a better understanding of why capital-income ratios and capital shares tend to move together.

ticularly complicated to regulate in a peaceful manner. This was certainly true during the colonisation and decolonisation period. Issues of international property relations could erupt again in the future. The difficulty of dealing with extreme internal and external inequality certainly helps to explain the high political instability that has long plagued the development process in Latin American and African countries.

Public capital, which depends on the changing patterns and complex political histories of public investment and deficit trajectories, nationalisation and privatisation policies, also plays a critical role in the book (especially Chapters 3 and 4). I emphasize the sharp dissimilarities in country experiences (contrasting in particular the cases of Britain and France in the 18th and 19th centuries), as well as the commonalities (such as the historically large level of public capital in the postwar period, and the significant decline in recent decades, in rich countries as well as in Russia or China, with important implications for the distribution of private wealth and the rise of new forms of oligarchs).

Given the specific and context-heavy discussion of these multidimensional factors, does it still make sense to speak of 'capital' as a single category? 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 about this basic multidimensional reality of assets and corresponding property relations. As rightly argued by van Treek in his paper, the notions of an aggregate capital stock and of an aggregate production function $Y = F(K,L)$ are highly abstract concepts. From time to time I refer to them in my analysis. But I certainly do not believe that such grossly oversimplified concepts can provide an adequate description of the production structure and the state of property and social relations for any society. At different points in the book, I attempt to show that this abstract language can be useful for some purposes, but only if one does not exaggerate its meaning. 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 18th-19th centuries and until World War I. I believe that this finding is interesting in itself. But 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.

Some lessons from the history of public debt

Let me conclude by mentioning some of the lessons that can be drawn from the history of public debt as presented in my book (see especially Chapters 3–4 and 15), and which in my view can fruitfully inform some of the current debates about the Eurozone public debt crisis. If we take a broad comparative perspective, we find a large number of high public debt episodes, and a wide diversity of solutions that were adopted to deal with such situations. In my view, it is particularly fruitful to contrast the case of 19th century Britain with that of 20th century Germany or France.

Following the Napoleonic Wars, British public debt reached very high levels of well above 200 percent of GDP. This is an interesting example, because the successive British governments during the 19th century decided to gradually reduce this large public debt by slowly accumulating primary budget surpluses. There was no exceptional measure, there was no debt restructuring, and there was no inflation. If anything, consumer price inflation was slightly negative on average in Britain between 1815 and 1914, a little bit like the Eurozone in early 2014–2015. On average, the primary budget surplus was between 2 percent and 3 percent of GDP in Britain throughout the 1815–1914 period (which was mostly used to finance interest payments). The good news is that it worked, in the sense that public debt was finally reduced to very low levels around 1900–1910 (about 30 percent of GDP). The bad news is that it took a very long time: an entire century, during which the British taxpayers were putting more resources to repay public bond holders than they were investing in their entire education system. One may argue this was the best strategy to invest in the country's future.

This is an interesting example, because there is a serious risk that Eurozone countries might follow the same strategy today. This is particularly ironic, given

that the Eurozone was largely conceived by two countries, Germany and France, who never repaid their public debt during the 20th century. In 1945, both countries had accumulated enormous public debt (around 200 percent of GDP). By 1950–1955, this large public debt had disappeared (about 20–30 percent of GDP).

Of course, this did not happen because enormous budget surpluses were run between these two dates: this occurred thanks to a series of exceptional measures, and in particular due to high inflation. The fact that public debt was quickly reduced to negligible levels certainly had a positive impact on the ability of German and French governments to invest in reconstruction and postwar growth. Had they adopted the same strategy as the British government during the 19th century, it would have taken many decades to reduce such a large public debt. More specifically, both Germany and France would have had fewer resources to invest in public infrastructure, education or health in the 1950s, 1960s and 1970s. Of course, there were also costs associated with postwar inflation: many lower class and middle class households lost a large part of their savings, which probably helps to explain today's fear of inflation in Germany, as well as in France.

It is worth stressing, however, that other exceptional policy measures played a role in reducing postwar public debt (particularly debt restructuring in the case of Germany, and exceptional progressive tax on large private wealth in the case of France), and could play an even bigger role in the future. Such policy measures make it possible to avoid the negative distributional impact of inflation. In particular, the progressive wealth tax can be viewed as a civilized, progressive form of inflation. The ideal policy mix is certainly difficult to find, and there is clearly no easy solution to reducing a large public debt. My general point is simply that there is a wide diversity of policy tools that can potentially be used, and that the mere accumulation of budget surpluses in a zero-inflation environment is a strategy that can take a very, very long time. Historical amnesia is never the right solution.

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