Technical appendix of the book « Capital in the twenty-first century»
Appendix to chapter 10. Inequality of Capital Ownership
Addendum: Response to FT

Thomas Piketty, May 28 2014
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This is a response to the criticisms - which I interpret as requests for additional information - that were published in the Financial Times on May 23 2014 (see FT article here).[1] These criticisms only refer to the series reported in chapter 10 of my book "Capital in the 21st century", and not to the other figures and tables presented in the other chapters, so in what follows I will only refer to these series.

This response should be read jointly with the technical appendix to my book, and particularly with the appendix to chapter 10 (available here). The page numbers given below refer to the HUP edition of my book that was published in March 2014.

Let me start by saying that the reason why I put all excel files on line, including all the detailed excel formulas about data constructions and adjustments, is precisely because I want to promote an open and transparent debate about these important and sensitive measurement issues.

Let me also say that I certainly agree that available data sources on wealth inequality are much less systematic than what we have for income inequality. In fact, one of the main reasons why I am in favor of wealth taxation, international cooperation and automatic exchange of bank information is that this would be a way to develop more financial transparency and more reliable sources of information on wealth dynamics (even if the tax was charged at very low rates, which everybody could agree with).

For the time being, we have to do with what we have, that is, a very diverse and heterogeneous set of data sources on wealth: historical inheritance declarations and estate tax statistics, scarce property and wealth tax data; household surveys with self-reported data on wealth (with typically a lot of under-reporting at the top); Forbes-type wealth rankings (which certainly give a more realistic picture of very top wealth groups than wealth surveys, but which also raise significant methodological problems, to say the least). As I make clear in the book, in the on-line appendix, and

[1] See also the other two articles published by the FT on May 23 2014: here and there. See also my short reponse published here in the FT. Unfortunately I was given limited time to submit this response, so I could not address specific points; here is a longer response.

in the many technical papers on which this book relies, I have no doubt that my historical data series can be improved and will be improved in the future (this is why I put everything on line). In fact, the "World Top Incomes Database" (WTID) is set to become a "World Wealth and Income Database" in the coming years, and together with my colleagues we will put on-line updated estimates covering more countries. But I would be very surprised if any of the substantive conclusions about the long run evolution of wealth distributions was much affected by these improvements.

I welcome all criticisms and I am very happy that this book contributes to stimulate a global debate about these important issues. My problem with the FT criticisms is twofold. First, I did not find the FT criticism particularly constructive. The FT suggests that I made mistakes and errors in my computations, which is simply wrong, as I show below. The corrections proposed by the FT to my series (and with which I disagree) are for the most part relatively minor, and do not affect the long run evolutions and my overall analysis, contrarily to what the FT suggests. Next, the FT corrections that are somewhat more important are based upon methodological choices that are quite debatable (to say the least). In particular, the FT simply chooses to ignore the Saez-Zucman 2014 study, which indicates a higher rise in top wealth shares in the United States during recent decades than what I report in my book (if anything, my book underestimates the rise in wealth inequality). Regarding Britain, the FT seems to put a lot of trust in self-reported wealth survey data that notoriously underestimates wealth inequality.

I will start by giving an overview of the series on wealth inequality that I present in chapter 10 of my book. I will then respond to the specific points raised by the FT.

Overview of the series on wealth inequality reported in chapter 10

The long run series on wealth inequality provided in chapter 10 of my book deal with only four countries: France, Britain, Sweden, and the United States.

The series used to construct figures 10.1-10.6, replicated in the book on p.340-348 are available in table S10.1, as well as in the corresponding excel file.

These wealth inequality series deal with much fewer countries and are substantially more exploratory than the empirical material provided in other parts of the book: income and population growth in chapters 1-2; wealth-income ratios in chapters 3-6; income inequality series in chapters 7-9. This follows from the fact that available data sources on wealth inequality are much less systematic than data sources on growth, wealth-income ratios and income inequality. In particular, we do have yearly income declarations statistics for dozens of countries, but we do not have yearly wealth declarations statistics for most countries. So we have to do with the diverse set of sources that I described above.

I believe that the data we have on wealth inequality is sufficient to reach a number of conclusions. Namely, wealth inequality was extremely high and rising in European countries during the 19th century and up until World War 1 (with a top 10% wealth share around 90% of total wealth in 1910), then declined until the 1960s-1970s (down to about 50-60% for the top 10% wealth share); and finally increased moderately since the 1980s-1990s. In the United States, wealth inequality was less extreme than in Europe until World War 1, but it was less strongly affected by the 20th century shocks, and in recent decades it rose more strongly than in Europe. Both in Europe and in the United States, wealth inequality is less extreme than what it was in Europe on the eve on World War 1.

I believe that the data that we have is sufficient to reach these conclusions, but that it is insufficient to go much beyond that. In particular, our ability to measure the most recent trends in wealth inequality is limited, partly due to the huge rise in cross border financial assets and offshore wealth. According to Forbes-type wealth rankings, the very top of the world wealth distribution has been rising about three times faster than average wealth at the global level over the 1987-2013 period (see chapter 12 of my book, in particular Table 12.1. The growth rate of top global wealth, 1987-2013). This seems to be clear evidence than wealth inequality is rising, partly because the rate of return to very large portfolios is higher than the growth rate. This interpretation is consistent with what I find with the returns to large university endowments (see Table 12.2. The return on the capital endowments of US universities, 1980-2010). But we do not really know whether this holds only at the very very top or for bigger groups (say, above 10 millions $ and not only above 1 billion $). Let me make very clear that I do not believe that r>g is the only force that determines the dynamics of wealth inequality. There are many other important forces that could in principle drive wealth inequality in other directions. The main message coming from my book is not that there should always be a deterministic trend toward ever rising inequality (I do not believe in this); the main message is that we need more democratic transparency about wealth dynamics, so that we are able to adjust our institutions and policies to whatever we observe.

I now consider each of the four countries one by one and respond to the specific points raised by the FT. I start with Sweden (the first country for which the FT expresses concerns), and then move to France, the United States, and finally to Britain (arguably the country with the biggest data problems) and to the European average.

Sweden (see figure 10.4 here**)**

The FT does not point out any significant disagreement regarding Sweden. Their corrected figure looks virtually identical to mine (see their figure on Sweden here).

The FT argues however that my choice of years from raw data sources is not entirely clear. For instance, they point out that raw data for year "1908" for year "1910", year "1935" for year "1930", and so on. These issues are already explained in the book and in the technical appendix, but they probably need to be clarified. Generally speaking, when I present series on wealth-income ratios and wealth inequality (and also for some figures on income inequality), I usually choose to present decennial averages rather than yearly series. This is because wealth series often display a lot of short-run volatility (in particular due to sharp movements in asset prices). So in order to focus the attention on long-run evolutions, it is better to abstract from these short-run movements and show decennial averages. See for instance the wealth-income series presented in chapter 5: contrast figure 5.1 and figure 5.5. When full yearly series are available, the way decennial averages are computed in the book is the following: "1900" usually refers to the average "1900-1909", and so on. This is further explained in the technical paper "Capital is back..." (Piketty-Zucman QJE 2014) available here.

In the case of the wealth inequality series reported in chapter 10, the raw series are usually not available on annual basis, so I compute decennial averages on the basis of the closest years available. This is clearly explained in the chapter 10 excel file (see sheet "TS10.1"). For instance, "1870" is computed as the average for years

"1873-1877", "1910" as the average "1907-1908", and so on. These choices can be discussed and improved, but they are reasonably transparent (they are explicitly mentioned in the excel table, which apparently the FT did not notice), and as one can check they have negligible impact on long run evolutions.

The FT also suggests that I made a transcription error by using the estimate for 1908 for the top 1% wealth share (namely, 53.8% of total wealth) for year 1920 (instead of the correct raw estimate for that year, namely 51.5% of total wealth). In fact, this adjustment was intended to correct for the fact that there is a break in a data sources in 1908: pre-1908 series use estate tax data, while post-1908 use wealth tax data, resulting into somewhat lower top wealth (as exemplified by year 1908, for which both data sources co-exist; see Waldenstrom 2009, Table 3.A1, p.120-121). This is standard practice, but I agree that this adjustment should have been made more explicit in the technical appendix and excel file.[2] In any case, whatever adjustment one chooses to make to deal with this break in series is again going to have a negligible impact on long-run patterns.

France (see figure 10.1 and figure 10.2 here)

The FT does not point out any significant disagreement regarding France. Their corrected figure looks virtually identical to mine (see their figure on France here).

The FT argues however that no explanation is given for some of the data construction. Namely, the FT claims the following: "The original source reports data relative to the distribution of wealth among the dead. In order to obtain the distribution of wealth across the living, Prof Piketty augments the share of the top 10 per cent of the dead by 1 per cent and the wealth share of the top 1 per cent by 5 per cent. An adjustment of this sort is standard practice in this type of calculations to correct for the fact that those who die are not representative of the living population. Prof. Piketty does not explain why the adjustment is usually constant. But in one year, 1910, it is not constant and the adjustment scale rises to 2 per cent and 8 per cent respectively. There is no explanation."

This is a surprising statement, because all necessary explanations are actually given in the technical research paper on which these series are based (see Piketty-Postel-

[2] Also note that the raw series display a decline in top 1% wealth share between 1908 and 1920, but a sharp rise in the share of the next 9% (resulting into a significant increase in the top 10% share). This does not look entirely plausible and might also be due to a break in raw data sources (unless this is due to sharp short-run variations in the relative price of assets held by these different wealth groups).

Vinay-Rosenthal AER 2006) and in the chapter 10 excel file (see sheet "TS10.1DetailsFR"). Namely, the PPVR AER 2006 paper includes detailed, year-by-year estimates of how differential mortality affects wealth inequality among the living, and finds that the ratio between top wealth shares among the living and top wealth shares among decedents rises at the end of the 19th century and in the early 20th century. Intuitively, this is because differential mortality effects seem to become stronger around that time (namely, life expectancy rises quite fast among top wealth holders, but much less so for the rest of the population). One can see this explicitly in table A4 of the working paper version of the PPVR AER 2006 article; this is explicitly reproduced in chapter 10 excel file (see sheet "TS10.1DetailsFR", table A4 (2), ratios for top 1% shares). More recent research has also confirmed the changing pattern of differential mortality around that time. See in particular the appendix tables to Piketty-Postel-Vinay-Rosenthal EEH 2014. Differential mortality is a complex issue, and we do not have perfect answers; but we do our best to address this issue in the most transparent way. In particular, we put on line on this web site the large micro files that we have collected in French inheritance archives, so that everybody can reproduce our computations and use this data for their own research. We are currently collecting additional micro files in Parisian and provincial archives, and we will put new data files and updated estimates in the future.

What it find somewhat puzzling in this controversy is the following: (i) the FT journalists evidently did not read carefully the technical research papers and excel files that I have put on-line; (ii) whatever adjustment one makes to correct for differential mortality (and I certainly agree that there are uncertainties left regarding this complex and important issue), it should be clear to everyone that this really has a relatively small impact on the long-run trends in wealth inequality. This looks a little bit like criticism for the sake of criticism.

United States (see figure 10.5)

The FT does point out more substantial disagreements regarding the United States. Their corrected figure actually looks very close to mine regarding the long run evolution, but not for the recent decades, where the FT considers that I overestimate somewhat the rise in wealth inequality (see their figure on United States here). The FT also expresses concerns about some of the adjustments that are made for earlier periods, although they have little impact on the overall patterns.

As I explain in the book (chapter 10, p.347) and in the technical appendix to chapter 10 (available here), there are very large uncertainties regarding US historical sources on wealth inequality, and I certainly agree that the series that are provided in the book can be improved. I try to combine in the most consistent manner the information coming from estate tax statistics (which unfortunately only cover the top few percents of the distribution, and not the entire population like in France) and the information coming from household wealth surveys (fortunately the SCF is known to be of higher quality than most other wealth surveys). In particular, the estimate for year 1970 tries to combine the estimates available for top 10% and top 1% wealth shares for years 1960 and 1980 and the evolution of very top wealth shares between 1960, 1970 and 1980. This has little impact on the overall long-run pattern, but I agree that this is relatively uncertain, and that this could have been explained more clearly.

I should stress however that the more recent and more reliable estimates that were recently produced by Emmanuel Saez (Berkeley) and Gabriel Zucman (LSE) confirm the pattern that I find. See Saez-Zucman 2014. For the recent decades, they actually find a larger rise of top 10% wealth shares and especially top 1% and top 0.1% wealth shares than what I report in my book. So, if anything, my book tends to underestimate the recent rise in US wealth inequality (contrarily to what the FT suggests).

This important work was done after my book was written, so unfortunately I could not use it for my book. Saez and Zucman use much more systematic data than I used in my book, especially for the recent period. Also their series are constructed using a completely different data source and methodology (namely, the capitalization method using capital income flows and income statements by asset class). Now that this work is available, the Saez-Zucman series (which unfortunately the FT article seems to ignore) should be used as reference series for wealth inequality in the United States. In a recent survey chapter that will be published in the Handbook of Income Distribution (HID), we choose to use the Saez-Zucman series (rather than the series reported in my book) in order to describe the long-run evolution of US wealth inequality. See Piketty-Zucman 2014 (see in particular supplementary figure S3.5, p.91 for a comparison between the two series; as one can see, they look very similar).[3]

[3] Note that this HID chapter also includes novel series about the evolution of the share of inheritance in total wealth accumulation. These new series use a different methodology and complement those reported in chapter 11 of my book.

Britain (see figure 10.3)

The FT does point out substantial disagreements regarding the recent evolution in Britain. Their corrected figure actually looks very close to mine regarding the long run evolution, but not for the recent decades, where the FT considers that there was no rise at all in wealth inequality, and possibly a decline, whereas I report a rise (see their figure on Britain here). The biggest disagreement comes from the latest data point (c.2010): the FT considers that the right estimate for the top 10% wealth share is around 44% of total wealth (this comes from a recent household survey based upon self-reported data, namely the “wealth and assets survey”, which I believe underestimates top wealth groups significantly; see below); whereas I report an estimate with a top 10% wealth share around 71% (this comes from more reliable estate tax statistics). This is a very large difference indeed.

Let me make clear that although I think my estimate is more reliable and rests on better methodological choices, I also believe that this large gap reflects major uncertainties and limitations in our collective ability to measure recent evolution of wealth inequality in developed countries, particularly in Britain. As I explain above, I believe this is a major challenge for our statistical and democratic institutions.

The estimates that I report for wealth inequality in Britain rely primarily on the very careful estimates that were established by Atkinson-Harrison 1978 and Atkinson et al 1989 using estate tax statistics from the 1920s to the 1980s. I updated these series for the 1990-2010 period using official HMRC data that are also based upon estate tax records. I find a rising inequality trend, although a more modest one than for the United States. I think this is the most reasonable estimate one can obtain given available data, but this certainly should be improved in the future.

What is troubling about the FT methodological choices is that they use the estimates based upon estate tax statistics for the older decades (until the 1980s), and then they shift to the survey based estimates for the more recent period. This is problematic because we know that in every country wealth surveys tend to underestimate top wealth shares as compared to estimates based upon administrative fiscal data. Therefore such a methodological choice is bound to bias the results in the direction of declining inequality. For instance, as I note in the technical appendix to chapter 10 (available here), the recent wealth surveys undertaken by INSEE in 2004-2010 in France indicate a top decile share just above 50% of the total wealth, whereas fiscal data (inheritance and wealth tax) suggest a top decile share above 60% of the total wealth. The gap seems particularly large for the case of Britain, which could reflect the fact that the “wealth and assets survey” seems particularly bad at measuring the top part of the wealth distribution of the UK. Indeed, according to the latest report by the Office of national statistics (ONS), the response rate for this survey was only 64% in 2010-2012; this is an improvement as compared to the response rate of 55% that was observed during the 2006-2008 wave of the same survey (see ONS 2014, Table 7.1); but it is pretty clear that with such a low response rate, it is hard to claim that one can adequately measure wealth inequality, particularly at the top of the distribution. Also note that a 44% wealth share for the top 10% (and a 12.5% wealth share for the top 1%, according to the FT) would mean that Britain is currently one the most egalitarian countries in history in terms of wealth distribution; in particular this would mean that Britain is a lot more equal that Sweden, and in fact a lot more equal than what Sweden as ever been (including in the 1980s). This does not look particularly plausible.

Of course the estate records based estimates also raise significant methodological concerns, and I do not claim that the resulting estimates are perfectly reliable. In particular, they might also underestimate top wealth levels (because top wealth holders sometime escape the estate tax through sophisticated trust funds or offshore assets). But they definitely seem more plausible than the estimates based upon self-reported survey data.

Note also that in recent years more and more scholars and statisticians have started to recognize the limitations of household wealth surveys and to upgrade the top segments of survey based wealth distributions using other sources. For instance, a recent study undertaken at the research department of the ECB attempts to upgrade in a systematic manner the top tail of the wealth surveys undertaken in Eurozone countries by using the Pareto coefficients that one can estimate using Forbes rankings and other lists of very high wealth individuals in each country. The results indicate that this can lead to very large increases (more than 10 percentage points) in top wealth shares (see Vermeulen 2014). In the United States, although the SCF wealth survey is generally regarded as a very high quality wealth survey, there has been some important work trying to upgrade the top tail by using Forbes ranking and estate tax data (see Johnson-Shreiber 2006 and Raub-Johnson-Newcomb 2010). This is definitely something that should be done for the British “wealth and assets survey”.

Regarding the 19th century estimates, the FT expresses concerns with the way I compute the top wealth shares for Britain in 1810 and 1870. Namely, I borrow the top 1% wealth shares estimates from Lindert (54.9% and 61.1%, respectively), and I assume that the next 9% shares shifted from 28% to 26%. Lindert does report a lower estimate for the next 9% share (about 16%). However this would indicate a relatively unusual pattern of Pareto coefficients within the top 10% of the distribution (as compared both to the French 19th century inheritance data, which is a lot more comprehensive than the British probate data, and to the British estate tax statistics for 1911-1913). Given that the probate records used by Lindert seem to provide a better coverage of the top 1% than of the next 9%, I use Pareto interpolation techniques to estimate the next 9% share. This is an issue that should have been explained more clearly and that would definitely deserve further research. This has a limited impact for the long run patterns analyzed here (the pre-World War 1 rise in wealth inequality would be even larger without this adjustment).

European average (see figure 10.6)

Finally, the FT also expresses the following concern: the European average series, which I computed by making a simple arithmetic series between France, Britain and Sweden, should have been computed using population weighted averages. I do agree that population (or GDP) weighted averages are generally superior to simple arithmetic averages. However I should stress that it really does not make much of a difference here, because all three European countries that I use follow fairly similar long run patterns. Namely, all three countries display high and rising top wealth shares during the 19th century and up until World War 1 (with about 90% of total wealth for the top 10% around 1910); then a sharp decline until the 1960s-1970s (with top 10% wealth shares down to 50-60%); and finally a modest rise since the 1980s-1990s. So whether one weights the three countries with equal weights or according to population or GDP does not make a big difference. But in case Britain did follow a markedly different pattern than the other countries in recent decades (with a decline in wealth inequality rather than a rise), then putting more weight on Britain than on Sweden becomes a significant issue. So we are back to the previous question: what happened to wealth inequality in Britain in recent decades? The FT seems to believe it has become more equal; however the way they use self-reported wealth survey data is not convincing. This is nevertheless an interesting debate for the future, and we should all agree that we know too little about it.