

Sparse, Inconsistent and Unreliable: Tax Records and the *World Inequality Report 2018*

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ABSTRACT

This article reviews the *World Inequality Report 2018*, a large collaborative data project based on the work of Thomas Piketty and the late Anthony Atkinson, which critiques the entire literature of inequality measurement from survey data and purports to provide superior, unprecedented and reliable coverage of income and wealth inequalities over the entire world, based primarily on tax records. The article examines three major issues: the coverage provided by tax data in the world economy, the consistency of tax data with other sources of information on income inequality, and the peculiarities of tax-based measurement of inequality in the United States. Then a comparison is made with measures drawn from other forms of administrative data — specifically payroll records — which are generally more consistent with records of inequality measured in household surveys than are tax records. Following this, the article discusses the analysis of wealth and wealth inequality before offering a few closing remarks about policy.

INTRODUCTION

In *The World Inequality Report 2018*, Thomas Piketty and his colleagues have produced a new and wide-ranging exposition of their empirical work on income and wealth inequalities.¹ The Report showcases the exploration of measures and evidence started with *Top Incomes Global Perspective* (Atkinson and Piketty, 2010) and theorized in Piketty's (2014) epic *Capital in the Twenty-first Century*. The larger goal is to inform a 'deliberative

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1. Published in 2017, the credited co-authors of the *The World Inequality Report 2018* are Facundo Alvaredo, Lucas Chancel, Thomas Piketty, Emmanuel Saez and Gabriel Zucman.

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process' with 'more rigorous and transparent information on income and wealth' (p. 8.) than has been available to date:

To overcome current limitations, we rely on a ground-breaking methodology which combines in a systematic and transparent manner all data sources at our disposal: national income and wealth accounts (including, when possible, offshore wealth estimates); household income and wealth surveys; fiscal data coming from taxes on income; inheritance and wealth data (when they exist); and wealth rankings. (ibid.)

The text of the *World Inequality Report 2018* runs to nearly 300 pages, of which about half deal with income inequality and the rest with wealth inequality and with policy suggestions. The formatting is elegant, suffused with data-rich graphics, and the whole is backed by a beautiful website, replete with numbers and a treasury of background papers dealing with general methods and particular case studies. The authors state that these are the 'latest and most complete data' developed by 'more than a hundred' research fellows of the World Inequality Lab (ibid.).

The ground-breaking, systematic and transparent methodology on which the Report rests is largely the use of tax records — specifically income tax records — mined to show the income shares of tranches of the income-earning population: top 1 per cent, top 10 per cent, next 40 per cent and bottom 50 per cent are the usual divisions. These make up the World Inequality Database (WID, formerly WWID). These, Piketty and his colleagues argue, are more complete, comprehensive and comparable across countries and through time than the generally used alternative, which is household or person-based surveys.

The Report leads with 'new findings', among them that 'income inequality has increased in nearly all world regions in recent decades, but at different speeds' (p. 9). It elaborates that inequality is 'lowest in Europe and highest in the Middle East' and that the different speeds at which inequalities rise 'highlights the important roles that national policies and institutions play in shaping inequality' (ibid.). Many similar statements follow, including some strong causal assertions, such as that in the United States 'the income inequality trajectory observed . . . is largely due to massive educational inequalities, combined with a tax system that grew less progressive . . . since the 1980s' (p. 10).

Such claims are too numerous to check in full and some may appear superficial as well as not exactly new. That inequality has risen in countries and regions around the world is a commonplace, although, given the rise of average real incomes in China and to a lesser degree India (which, combined, account for about 35 per cent of the world population), it is not entirely clear that *world inequality* — inequality measured across the people of the world — has actually increased over recent decades. It is technically possible — and may be the case — that inequality increased in every region yet was stable or even declined at the level of the world as a whole. That said, the study of worldwide inequality between persons is more academic

than practical; one may reasonably argue that people care mainly about their own reference group, which is much more likely to be national, provincial or local than continental or global.

A more interesting claim lies in the focus on national institutions and policies, which is justified by the comment about ‘different speeds’. Differences in the behaviour of inequality over time are indeed evidence that national institutions matter. But if it should appear instead that movements of inequality are *correlated* across countries, that inequalities move in the same way in neighbouring countries or even across continental distances — that would lead toward a very different view. Namely, it would suggest that global forces tend to drive the movement of inequalities across countries, even if they do not work everywhere with the same force or at the same rate. We shall return to this issue, as work with different data strongly indicates that powerful global macroeconomic currents affect the movement of inequalities, especially in smaller countries and the developing world.

Looking at simple statements about different regions, is it correct (for instance) to state that ‘inequalities are lowest in Europe’? By practically all measures, this appears true if one takes measures of individual countries — the national data record — and particularly if one focuses on the small egalitarian countries of Northern Europe, such as Scandinavia and the Baltics, as well as such post-socialist Eastern European states as the Czech Republic. But is it also correct if one measures inequality for Europe as a single integrated continental economy, that is, as the ‘European Union’? That would be to measure European inequality in the same way that one would do for the United States. To get that measure, one would have to take account of the differences in average income *between* European countries — of the gap between Romania and Sweden, or Germany and Portugal. To do that, it is necessary to integrate the various national data sources into a single European pool.² And for that, even within the single currency eurozone, separated national tax data will not serve.

More broadly, the authors claim that data drawn from tax sources are generally, if not invariably, better than the most commonly used alternative, household survey records. They write disparagingly of the ‘Gini index’, the inequality measure most prevalent in such surveys, which they find too ‘technical’ and not sufficiently intuitive. But they also object to survey methods: ‘The main problem with household surveys, however, is that they usually rely entirely on self-reported information about income and wealth. As a consequence, they misrepresent top income and wealth levels, and therefore overall inequality’ (p. 29).

This sweeping critique carries on for several pages, brushing aside a body of research comprising thousands of papers and millions of survey observations, including the work of the Luxembourg Income Studies, the

2. For early work on this issue, see Galbraith et al. (1999).

World Bank, Eurostat, the Economic Commission for Latin America and the United States Census Bureau, among scores of national data-collection agencies. It is a repudiation of what almost every previous researcher has done in this field over 50 years.

The critique has certain points of merit. It is true that while some countries have good and regular household surveys, many do not. The comparative and historical record is at best intermittent and biased towards wealthy and stable countries with the resources and interest to track the issue. Further, the problem of comparing survey results is compounded by differences in the concept being surveyed. Is it income or is it consumption? If income, is it before or after transfers and taxes? Is it income of households or of individuals? If households, what adjustments are made for household size? Variation in these details tends to complicate the use of survey data for comparative purposes. And, as the authors of the Report correctly point out, even where surveys are fairly consistent and regular, the rich are routinely under-sampled, thanks to ‘fat tails’ and to ‘top-coding’, and also to the natural reticence of the wealthy faced with probes into their affairs.

But are tax data really better? Where survey and tax measures both exist, and report different results, should one systematically prefer a measure based on taxes? The answer depends in part on the quality of the survey measures. But it also must depend *in part* on the quality, consistency, length and continuity of the national tax record, and in particular of the income tax record. Any serious attempt to evaluate the Report must therefore take a considered look at the measures the authors have compiled.

This review examines the question from three points of view: the coverage provided by tax data in the world economy, the consistency of tax data with other sources of information on income inequality, and the peculiarities of tax-based measurement of inequality in the United States. It goes on to make a comparison with measures drawn from other forms of administrative data — specifically payroll records — which are generally more consistent with records of inequality measured in household surveys than the tax records of the World Inequality Lab. I then discuss the analysis of wealth and wealth inequality before offering a few closing remarks about policy.

COVERAGE OF TAX DATA IN THE WORLD ECONOMY

What happens when you begin to examine the online data set underlying the *World Inequality Report 2018*? Click on Tanzania or Yemen or any country, and graphs of the top income shares appear, with the data points connected by smooth curves to show the movement over time. But then, run your cursor over those graphs, and you will notice that the record is not entirely complete. For Yemen there is just a single data point, for the top 1 per cent, from 2006 — and the resulting ‘time line’ is a flat line. For Tanzania, the series ends in 1970 — by now nearly a half century back. For Malaysia, it

is somewhat better: there are 32 data points over 61 years. Is this truly the *latest and most complete* record?

The longest series and perhaps the brightest jewel in the WID crown is Japan, for which Piketty and his colleagues present estimates of the top 1 per cent shares for 124 years, going back to the introduction of income tax in Japan in 1887. A paper by Moriguchi and Saez (2010) explains and backstops the estimates; it is actually a chapter from *Top Incomes: A Global Perspective*, edited by Atkinson and Piketty (2010). There is careful attention to the estimating procedure, to changes in the Japanese tax code and other relevant matters. The war and occupation are accounted for — it turns out that the top income share fell sharply during the war and that the occupation largely continued the wartime incomes policy rather than instituting new levelling measures. As Moriguchi and Saez (2010) explain, however, land reform and the wealth distribution are a different story.

And yet, when you click on the menu bar for other measures of income shares in Japan — the middle 40, the bottom 50 — or for any measure of wealth inequality — nothing comes up, except a box instructing you to choose some other statistic. Moreover, Japan is an insular nation, an advanced industrial country, a fastidious record keeper, and since 1945 a stable, nearly one-party state. If this is the best-case, what about the others? Is this wonderfully presented data package a bit of a Potemkin village?

The authors of the Report claim to have studied ‘more than 70’ countries with data on ‘distribution’, meaning on income shares measured from tax data. My count from the website is 66, but of these 12 are multiple entries or varying with units of observation: there are five listings for the United States, three for France, and so forth, plus a combined region (‘World’), so the total of distinct countries for the top 1 per cent share (the most common measure) is 54. For other statistics — say the ‘bottom 50’ the count is less: just 21 countries of which 14 (for some reason) are in the Middle East,³ and only one each from East Asia, Eastern Europe, South America or sub-Saharan Africa. In the full data set (including the top 1 per cent shares) there are no countries from Central America or the Caribbean or the former Soviet Republics except Russia. Among large countries, Mexico, Nigeria, Pakistan, Ukraine and Vietnam are unrecorded. In the case of Nigeria, which is prominently listed on the website, a click on the link reveals a 99.9 fiscal share measure that runs from 1954 to 1959. Nigeria became independent in 1960.⁴

One source of selection bias — as Atkinson (2015) notes in his paper on Malaysia — is the British colonial practice of imposing income tax, which probably accounts for the availability of some tax data in up to 17 countries

3. I count Turkey as Middle East. Most of the entries for the Gulf States have just one or two observations.

4. Similarly, data for Ghana and Zambia end before independence; those for Zimbabwe end in 1984.

that were at one time under British rule (Australia, Canada, Egypt, Ghana, Iraq, Ireland, Kenya, Malaysia, New Zealand, Nigeria, Palestine, Singapore, South Africa, Uganda, Yemen, Zambia, Zimbabwe), as well as the United Kingdom. Overall, the WID has the top 1 per cent shares for 16 countries with six or fewer years, for 24 with 18 or fewer, and just 20 with 40 years of observation or above. The other source of selection bias is the same as for household surveys: there is more information on rich countries. Overall, there are 1,954 independent country/year observations in the WID; of these over 1,100 are from the member states of the Organization for Economic Cooperation and Development (OECD).

Similar limitations affect the library of research papers that Piketty and his colleagues present to document their measures. At first glance, the research record appears ultra-vast. Click on Malaysia, go to the library, and *two* papers by the eminent, late Anthony Atkinson come up. They are, however, the same paper, merely two versions issued at different dates (Atkinson, 2013, 2015). The tactic of repeatedly listing the same paper occurs frequently. Overall, there are 235 separate article listings, but the count falls to 139 when exact duplicates are removed and to only 104 when variants are also excluded. For the Middle East, data for practically the entire region are drawn from a single paper, listed in the library more than a dozen times.

COMPARISONS TO OTHER DATABASES

Any claim that a new data set is better than *all* the alternatives — the ‘gold standard’, as people unfamiliar with gold standards sometimes say — must be evaluated against those actual alternatives. Apart from delivering a general critique of household surveys and of the Gini coefficient — in principle, neither better nor worse than other inequality measures — the authors have shied away from this task. So we do not find here a systematic comparison of top-share rankings with, say, Gini coefficients by year and country, and we cannot know, from this analysis, whether the new data set from the World Inequality Lab (WIL) challenges previous understandings in a systematic or important way.

A widely respected effort to develop comparable cross-country estimates of household income inequality has been underway for years at the Luxembourg Income Studies (LIS),⁵ using a method that combines micro-data sets from national surveys and carefully brings definitions of income and of households into line with each other. LIS also offers a limited data set on wealth inequality. The LIS data may be taken as benchmarks; their limitation is that the survey data sets on which they are based exist mainly in wealthy countries, and the LIS estimates are limited to a relatively short

5. See www.lisdatacenter.org/

span of years. Limited comparison is therefore possible, both with LIS and with other data sets.⁶ Galbraith and Halbach (2016) have carried out this exercise in a separate paper, finding that in important cases the WID is not very consistent with other reputable sources, including LIS, the OECD, the Economic Commission for Latin America and the Caribbean (ECLAC), Eurostat and our own Estimated Household Income Inequality (EHII) data set. Better or worse? It depends on who you choose to believe, but in important cases the WID is the outlier. In social science research this is *sometimes* the mark of genius. But only sometimes.

The Report makes a strong — and in my view, valid — case for *supplementing* household income surveys with administrative data. This is in part on the ground that administrative records are collected consistently and routinely, and therefore enable researchers to fill in an otherwise sparse, spotty and often inconsistent survey record. The authors are careful to acknowledge — in numerous places in the Report and supporting papers — the limitations of tax records specifically and the need for care in extrapolating tax data over the whole population; this is a modelling exercise that requires diverse strategies depending on circumstances in each country. Yet they seem unaware of the existence of administrative data *apart* from tax records, which may cast additional light on the evolution of inequalities worldwide.

For over 20 years, the University of Texas Inequality Project (UTIP) — a much smaller and more modest operation than the WIL, consisting at any given time of a few PhD and Master's students under this author's supervision — has been producing measures of inequality based mostly on administrative payroll records, that is, total payrolls and employment measured across industrial, sectoral or geographic categories. The metric is the between-groups component of Theil's T statistic, a standard ('generalized entropy') inequality measure based on the theory of information developed initially by Claude Shannon (1948). In 74 papers to date, the UTIP team has shown that inequality measured across categories is an effective instrument for tracking the overall *evolution* of a distribution over time.⁷ It has also shown that a consistent category scheme — a standardized industrial classification, say — provides a reasonable basis for comparing *levels* of inequality between regions or countries. And it has demonstrated that payroll data — even when limited to a narrow part of the earning population, such as workers in manufacturing establishments — are closely related in statistical terms to measures of household income inequality. Unlike the data generated

6. A much larger data set is available from UNU-WIDER, but it is bibliographic in nature, rather than being constructed to provide measures of any single consistent inequality concept. See Ferreira and Lustig (2015).

7. The working papers and data sets may be found at: <http://utip.lbj.utexas.edu>, including references to journal publications. There are also four books: Galbraith (1998, 2012, 2016) and Galbraith and Berner (2001).

by the WIL, the UTIP work tends to reinforce and validate the best survey evidence, such as LIS, while filling in gaps in the coverage available from surveys.

From these findings, the UTIP team has produced a set of direct measures of industrial pay inequality based on the UN Industrial Development Organization's (UNIDO) Industrial Statistics, called UTIP-UNIDO, and a derivative EHII data set that uses a simple pair of regression coefficients to translate the industrial pay inequality measures into a Gini coefficient for the whole population. Both data sets now have over 4,000 country/year observations covering about 150 countries for the period 1963–2014 in the most recent updates. This is about twice the WID coverage, in nearly three times as many countries, and in a more compact time frame.⁸

In a series of papers the UTIP team has shown that the EHII measures correspond closely in most cases to available surveys of the same concept, while providing for a more dense and consistent set of measures, over more countries, than any other source (Galbraith and Kum, 2005; Galbraith et al., 2016a; Galbraith et al., 2016b). The data sets have been in wide use for over a decade, and acknowledged as a source data set for the widely used Standardized World Income Inequality Database (SWIID) of Frederick Solt.⁹

The *World Inequality Report 2018* states: ‘the 10 percent income shares then increased in all regions between 1980 and 2016’ (p. 42). Although this overlooks the noticeable decline in inequality which also appears in the top-share data for the period after 2000, it is a statement that accurately reflects the UTIP findings for the years 1980–2000 — which were reported on almost two decades prior.¹⁰ That said, how they can identify a turning point is not very clear, given that 1980 is the ‘date from which data become available for a large enough number of countries to allow a sound analysis of global dynamics’ (p. 41). And the subsidiary finding that inequality *within countries* continued to rise after 2000 — indeed accelerated — is inconsistent with national data from a wide range of countries that are not in the WID.¹¹

Yet to arrive at their 2018 ‘finding’, the WIL researchers were obliged ‘to distribute [a] quarter of global income to the third of the

8. UTIP-UNIDO and EHII are the largest data sets on world inequality that employ a single consistent concept (gross household income inequality in the case of EHII) and that (unlike the Standardized World Income Inequality Database, SWIID) use no interpolation across either years or countries. The only interpolations in the construction of the data set involve the bridging of reporting gaps for particular industries in the underlying Industrial Statistics. For details, see Galbraith et al. (2017).

9. See Frederick Solt's Standardized World Income Inequality Database at: <http://fsolt.org/swiid/>

10. To quote: ‘many countries compressed their wage structures in the 1970s but most saw rising inequality in the 1980s’ (Galbraith and Berner, 2001: 175).

11. Further evidence on declining inequalities globally after 2000 is to be found in Hammar and Waldenström (2017) — another new cross-country database that is not cited in the *World Inequality Report 2018*.

global population for which there is currently no consistent income inequality data available' (p. 47). The imputations are unnecessary, for there do exist consistent income inequality data for much (not quite all) of the Third World. For instance, Piketty and his colleagues maintain that 'there is no consistent income inequality data for sub-Saharan Africa'. But EHII offers 485 consistent country-year gross income inequality observations for 24 countries in that region.¹²

The *World Inequality Report 2018* reports measures of inequality aggregated across major regions, notably 'Europe' and 'the World'. A technical note by Chancel and Gethin (2017) explains how this is done. Gross income distributions are inferred for entire regions using data for a handful of countries. Thus data for the UK, Germany and France serve for all of Europe, China and India serve for Asia, and Brazil stands in for South America. Each group share is then scaled to the national income of each country, weighted either by Purchasing Power Parity or by market exchange rates. Gaps between years are interpolated. The resulting estimates are beyond heroic, and based on far less information than is actually available, since actual country-year inequality measures available from UTIP or the SWIID or many national sources are not used, or even cited.¹³

A considerable part of the Report is taken up with a handful of national or regional cases: the United States, France, Russia, Germany, India, the Middle East, Brazil and South Africa. We deal with the US case below. France stands out as a case where the top 10 per cent tax share hasn't changed since the early 1980s and is today *below* that of 1960. That is, the authors seem to believe that Gaullist France, under the Monnet Plans, with state-owned industries and banks and comprehensive credit allocation (Galbraith et al., 1981), was *less* egalitarian than today's citadel of Parisian wealth and National Front disaffection. The claim is quite implausible. France as a citadel of stable egalitarianism would, in that case, also be quite the exception in Europe — except for the fact that the authors, lacking data for other countries, use their French calculations as a partial template for all of Europe.

For India, the WID reports 90 years of top 1 per cent shares going back to the 1922 institution of income tax in British India, and 63 years of data on the other shares. A paper by Chancel and Piketty (2017) explains the heroic meld of sources and techniques required to estimate lower income shares for a country where only 6 per cent of earners file income tax. Despite the complications, the results are consistent with earlier estimates for India, which show a major turn towards rising inequality with the 1992 neoliberal 'reforms' (Galbraith et al., 2004b). These tax-based estimates are indeed better than the *consumption* surveys that the Indian government

12. As with all UTIP data, these are freely available. A detailed evaluation of the quality of the African data is available from the author on request.

13. Galbraith et al. (2016a) reference and classify hundreds of surveys, in addition to the EHII estimates.

routinely reports to the World Bank, which (as consumption surveys will) omit accumulated savings from the inequality measure. They do not, however, reveal anything new.

On China, the *World Inequality Report 2018* shows the large rise in inequality reported by household surveys, and reports that the top shares stabilized after 2006. Here their information tracks the UTIP findings of Zhang (2016). Top share data on Russia show the dramatic jump that occurred with the collapse of the USSR and stabilization after 1998; these findings track work from 15 years previously (Galbraith et al., 2004a) as well as later surveys. For Brazil, evaluation of tax records suggests that levels of inequality are higher than surveys show, but the tax records confirm declining inequality in Brazil since the mid-1990s. This finding was previously inferred from payroll records by Galbraith et al. (2007), and confirmed from surveys by López-Calva and Lustig (2010). An advantage of the earlier work is that it also covers other countries in Latin America, for which no independent records appear in the *World Inequality Report 2018*. Despite their argument for the specific importance of national institutions, and the existence of an entire research institution — ECLAC — with ample archives of source data on the region, the WIL team simply assumes that the rest of South America is like Brazil.

In general, where tax records confirm what is already known from other sources, the work of this Report is a valuable contribution. But the reader is not helped by claims that the findings are novel or that they supersede previous work on the same countries and regions, or that tax records produce better comparisons than surveys, let alone the neglected contribution of payroll records. And in a data set as rich and full of claims to scholarly precedence as this one, is it too much to ask for citations to earlier research?

THE PECULIAR CASE OF THE UNITED STATES

The unit of observation in tax data, characteristically, is the tax filer or tax unit. Taxes are filed by individuals or married couples, and it appears that in the second case the *World Inequality Report 2018* authors usually, although not always, just split the income equally between spouses ('equal-split adults'). It seems likely that in most countries the tax filer corresponds roughly to the head of household, but this is not a safe assumption in all cases. Using this measure, in the Report and in the graphics on the website, Piketty and his team discuss and illustrate what they term the 'collapse' in the income share of the 'bottom 50 percent' in the United States.

Was there a collapse of the income share of the bottom 50 per cent of Americans? It's a very strong claim and — given the prestige of one of its authors — one certain to take hold in the popular imagination and

in the press coverage of this report. It will therefore resonate through American politics for many years. If true, it is also a dramatic indictment of the household survey record, in which, according to numerous reputable papers (e.g. Caminada and Wang, 2011; Heathcote et al., 2010), there has been practically no change in the distribution of post-tax, post-transfer household incomes since the early 1990s, thanks to the workings of automatic stabilizers such as the progressive income tax and the Earned Income Tax Credit. And *per contra*, if false, it can stand as a critical test of the approach taken generally by the World Inequality Lab. Indeed it is a critical test of the general claim for the superiority of tax records in the analysis of income distributions.

A new paper by Stephen Rose (2018) has taken up this issue, making reference also to the important work of Auten and Splinter (2017). Rose makes the elementary, but critical, point that in the United States there are 122 million households as normally defined but 232 million tax filers (this datum is reported in the WID website as the US ‘population’ — an obvious error). There are thus at least 110 million tax filers living in households with multiple tax filers — and no doubt more, since some households are not required to file income tax. Rose reports that the average market income reported by this group of secondary filers is just US\$ 6,636. Who are they? Obviously, for the most part second earners, children living with their parents, young people pooling resources, or people living off wealth and modest incomes in the form of dividends, interest and capital gains. When government services and the value of owner-occupied housing are included in the incomes of this group, as Rose (2018: 4) notes, ‘there was no room in the bottom 50 per cent for workers with annual earnings over \$30,000’. Is the prevalence of these low earners among tax filers a sign of collapse? Of course not. The factoid tells us nothing about the US beyond the prevalence of separated tax filings, an obvious artefact of efficient tax administration, the mind-boggling thoroughness of US income reporting and the complexities of the US code. To confect a ‘dramatic collapse’ out of this is either wilfully attention seeking or absurdly naive.¹⁴ Let me say it again, and clearly: tax filers are not households. No collapse in the incomes of the bottom 50 per cent of American households has occurred.

There are other problems with the now famous Piketty-Saez time-series on the top 1 per cent income share in the US, long pre-dating its use in the *World Inequality Report 2018*. This series peaks at 20.8 per cent in 2012, practically as high as the 21.2 per cent recorded for 1929. As a general description of US social conditions — for which it is often cited — this is preposterous and always has been. In 1929 there was no Social Security, no Medicare or Medicaid, no Unemployment Insurance, Food Stamps, Temporary Assistance for Needy Families, no

14. Another important issue concerns the way Piketty and his colleagues allocate the value of government services to private individuals; for details see Rose (2018).

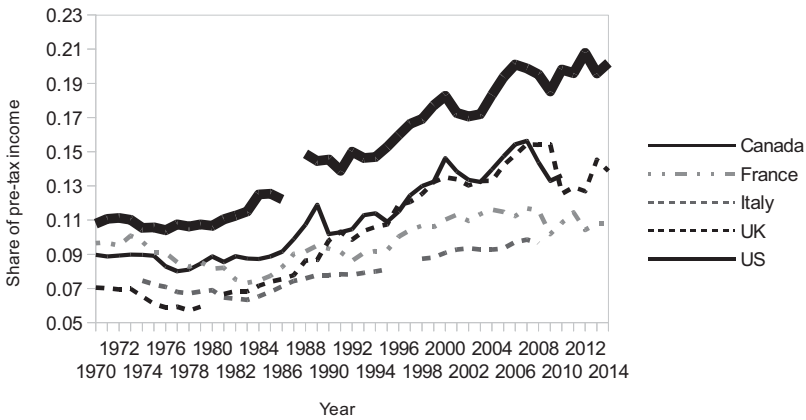
Earned Income Tax Credit, no National Labor Relations Board or minimum wage, practically no 30-year mortgages or deposit insurance. The life expectancy at birth for American men in 1929 was 58.1 years — roughly the same as in the demographic catastrophe that followed the collapse of the USSR.

Another serious difficulty crops up in the 1980s. According to the WIL, the US top 1 per cent share in the income tax data experienced a jump of 2.7 percentage points (12.2–14.9 per cent) in just two years over 1986–88. It is the largest two-year jump in the entire post-war record. But what happened in the US economy during those two years that could account for this? Not very much. There was no slump, no great boom, no inflationary surge, no oil shock — nothing that could account for this. Until, of course, you remember that the Tax Reform Act of 1986 (TRA86) was *designed* to require broader reporting of taxable income by top earners. The very goal of the TRA86 was to achieve revenue-neutral tax reform for the top incomes — a lower rate on a broader taxable base, achieved by eliminating exemptions and deductions that had previously reduced the reported top taxable incomes. Yet to anyone examining this datum without specific knowledge of this history, it looks like a social counter-revolution. This point illustrates the pitfalls of relying on tax data when the rules that define income are changed — those rules are tax rules.

The obvious data break occasioned by TRA86 is the entire source of the claim that inequality increases in the US are exceptional. Figure 1 illustrates this comparison. Remove the data break, and the US curves lie along those of the UK and Canada, just for starters. Whether the US top share should be counted as higher by the amount of the data break all along, and whether the true figures for the UK and Canada should be similarly adjusted, are matters that can be debated; it could be that the higher share for the US is nothing more than the artefact of a more diligent and effective tax code, and the availability of (say) Guernsey and Jersey, not to mention Geneva and Zurich, to top earners in the UK. But if the US ‘trajectory’ in inequality is not exceptional compared to the UK or Canada, the claim that exceptional inequality in US education is at fault for exceptional increases in inequality — a claim cited at the start of this review and a mainstay of mainstream wisdom on inequality — also falls down.

Moreover, and finally, note again the relatively unchanging figures for France or Italy in Figure 1. Do they reflect the reality of those countries over 50 years? Or merely the deliberate inefficiency of their tax systems when it comes to going after the very rich? No one familiar with the functioning of income tax in Italy or the evolution of real estate prices in Paris is likely to be in doubt. The fact is, tax data are only as good as the practices of tax collectors — and whatever the relative demerits of the US tax code, the Internal Revenue Service in the US has a reputation for effective enforcement that is not to be found in Italy or France.

Figure 1. Top 1 Per Cent Shares for Five Countries, from the World Inequality Database



Note: For the US there is a clear data break in 1987 due to the Tax Reform Act of 1986; when this is taken into account, the US is not an exceptional case when compared to Canada or the UK.

Source: <https://wid.world/data/> (accessed 10 November 2018).

WEALTH INEQUALITY AND WEALTH-INCOME RATIOS

The second half of the *World Inequality Report 2018* turns from inequality of incomes to that of wealth. So little is known about the distribution of wealth at the world level that the authors spend several opening pages on disclaimers. For instance, they write: ‘It is also important to keep in mind that the very different notions of private property and public property can have very different meanings, depending on the country or period considered’ (p. 157). This well-stated dictum brings back to mind that *income* is an artefact of tax law — a proposition to which the WIL team pays at best lip service. *Property* is contextual to an even greater degree, an artefact of property law and the various rights of entitlement and tenancy that are peculiar to each nation state and that vary over changing political regimes. The task of comparison is therefore not easy, and the quality of comparison will depend on the capacity of the research team to render commensurable what is diverse and idiosyncratic by its very nature.

For most of history and most of the world this is simply not possible. Piketty and his team therefore concentrate on a side issue so far as the assessment of *inequalities* of wealth is concerned. The side issue, to which they devote nearly 40 pages, is the calculation and comparison of a concept of ‘national wealth’ and the calculation of a ‘wealth-income ratio’ for a selection of countries over time. Their method is to value private assets — mainly financial assets and housing at market prices — subtracting private debts to arrive at net private wealth. They then value public assets according

to a scheme they do not describe in detail in the text, and subtract public debts so as to arrive at net public wealth. The sum of these two concepts is their notion of ‘national wealth’.

This procedure does not withstand scrutiny. To begin, what is the valuation of public capital assets? It cannot be made in market terms, obviously — public capital is not exchanged on any market. If one chooses instead the prices initially paid — say US\$ 24 for the island of Manhattan or whatever Seward paid for Alaska in 1867 — good luck. Does the value placed on the Arctic National Wildlife Refuge fluctuate with the price of oil? There is no sign of it in this study. So public assets are treated as having low and relatively stable values, while private assets are treated as being worth what the private market, at any given moment, appears to decide.

There are (to be sure) circumstances and purposes that can justify ‘mark-to-market’ accounting. But the assessment of the aggregate wealth of a country is not one of them, and indeed this choice of technique is peculiar to the point of bizarre. The market capitalization of a business firm is the number of shares outstanding, multiplied by the price at which a handful of them can be sold at any given time. This is not the price at which the entire lot could be liquidated. Nor is it the present value of future returns to the company’s underlying physical and human assets, except by accident. As a component of the private wealth of the US, the market capitalization of US-owned corporate equities is not a sensible measure, however much it may figure in the well-advertised private wealth of celebrity billionaires, as recorded by magazines like *Forbes*.

In accounting terms, the asset of the shareholder is a liability to the corporation, and apart from the valuation of the underlying physical assets its net contribution to national wealth is precisely zero. The fact that we have no good alternative valuation of the physical assets is no excuse; to use the market cap is to endorse the bold fiction of the ‘efficient markets hypothesis’ — which, 10 years after the great financial crisis, should be done by no one in their right mind.

The most egregious accounting issue in calculating national wealth is the Report’s treatment of national debt. This the authors simply subtract from their measure of public assets to achieve the remarkable result that the net public wealth of the richest capitalist countries on the planet *approximates zero* in most cases. They write: ‘This situation does not mean that rich countries have become poor; it is their governments which have become poor’ (p. 279). By the WIL standard, the US government, along with those of Japan and the UK, are actually bankrupt, while Germany and France are nearly so. (Russia and China, by contrast, are solvent on this measure; in the case of Russia, this is no doubt thanks in part to the world-shaking default of 1998.) But if the governments of the US, the UK and Japan are all bankrupt, one might ask, how is it that they can borrow unlimited sums on private capital markets at zero interest rates?

Piketty and his colleagues seem to have forgotten what Adam Smith (1776/2007) knew, that money is ‘a particular Branch of the General Stock’ (Book 2, Ch. 2). Public debt is a private asset — indeed it is the entire sum and substance of ‘net private wealth’ since all other private assets have corresponding private liabilities, which cancel each other exactly. Even when the debt of the US, the UK or Japan is held by foreign nationals, the true burden of that debt on the nation as a whole is the resource cost of servicing it in national currency units, which is precisely zero. Only countries that borrow and must repay in currencies they cannot issue have a public debt liability that matters, and which must be serviced from net exports. The US and the other issuers of reserve asset currencies and bonds and bills are not in this position.

The result of strange accountancy is that the WIL researchers arrive at some very strange inferences. They attribute the sharp drop in ‘national wealth’ relative to income observed in measures for Germany, France, the UK and the US in the second decade of the last century to ‘war destruction’ — even though World War I was not fought on the territories of the US, the UK or Germany (and there was no aerial bombardment to speak of). Obviously, the true source of the falling ratio is the vast rise in wartime incomes, while capital markets were closed and so capital asset prices could not rise.¹⁵

On a more modest scale, the privatization of council housing in the UK improves the wealth distribution in the WIL accounting, because rising house prices in Greater London dilute the otherwise highly concentrated financial wealth of the British elites. If this seems a conjurers’ trick — it is. The houses did not materially change. And what is a private asset to a new owner is just a prohibitive barrier to a prospective occupant, skewing the internal distribution of wealth but not adding to national wealth in any sensible way. More dramatically, the WIL team treats the spectacular speculative real estate bubbles in Japan and Spain as rising (and then falling) national wealth–income ratios. If this is the case, then obviously a rising ‘national wealth’ has no welfare significance at all, and indeed should be avoided as a harbinger of inevitable crash and disaster.

By the time we reach page 197 and the analysis of global wealth inequalities, the reader may not have the energy to realize that there is ‘no there there’. After a few prefatory remarks, the entire section rests on four countries: France, Spain, the UK and US. These chapters are not without interest. The authors identify declining net housing wealth, due to a rising ratio of mortgage debt to home values, as a major driver of increased wealth inequalities in these countries. Perhaps so. They do not, however, count public pensions, health insurance and other social insurance programmes as private wealth — although the private value of these benefits in rich countries, including the US, is, alongside housing, a major wealth-equivalent for

15. This is pointed out in Galbraith’s (2014) review of Piketty’s (2014) *Capital in the Twenty-first Century*.

the middle class. But the larger point is that there is so little information on wealth inequalities of any type that the section scarcely bears inclusion in what is supposed to be a ‘World Inequality Report’.

CONCLUDING THOUGHTS ON POLICY

I am likewise disinclined to comment on the final section, ‘Tackling Global Inequality’, with its half-hearted nods to milquetoast measures such as education and job training and its thin hint at the need for a ‘global financial register’, among other pipe dreams. Evidently our inequality problem can be solved only by world government and universal surveillance, along with a more efficient labour market.

The larger truth, easily overlooked here, is that the rise in global inequality from 1980 to 2000 was the by-product of a reactionary global financial regime, directed largely from Washington, New York and London. With sufficiently dense and consistent data, we can see this from the turning points and from the temporal and spatial sequence of rising inequalities, first in the countries hit hardest by the world debt crisis, then in the collapse of the socialist block, and finally in Asia.¹⁶ And the modest reduction in global inequality and poverty, particularly after 2000, can be traced first and foremost to those countries that defied the regime, as well as to the change in financial conditions that occurred following the end of the information technology boom in the West. This progress has now ended; we are back to the conditions that generate rising inequality, and the need for comprehensive stabilizing control over global finance is as urgent as it ever was.

Recent gains against global inequality are due in part to the rise of China — and to the productive powers of that country, not its real estate prices — as well as to the recovery of Russia from its 1990s calamity. There has also been the South American Summer that began with the election of progressive governments in Brazil, Argentina, Chile, Ecuador and other countries,¹⁷ and continued over about 20 years, along with the rise of average incomes in India despite crushing inequalities within that country. Many of these stories, especially those emanating from smaller countries, are not dealt with in the *World Inequality Report 2018*. That is not because data do not exist; they do. Rather, it is because Thomas Piketty and his colleagues choose to base their work on techniques and records that render many of those countries and their progress — and their struggles — largely invisible.

16. We can see it also in the close, contemporaneous relationship for many countries between changes in pay and income inequality and changes in exchange rates, an area of ongoing research.

17. Alas, the South American Summer has now ended, with the return of right-wing governments and systematic attacks on the social progress of the past two decades.

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