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Note from the editor

Inequality: *A Piketty et al. Moment* in the Social Sciences

Olivier Godechot

What does the world success of Piketty's *Capital in the 21st Century* (2014) reveal? This book is not just one of the most astonishing bestsellers in the social sciences in recent years, but it may also signal an important shift in the way we consider inequality in economics and sociology, in the social sciences and the public arena, in political debate and day-to-day conversations.

Piketty warns the reader that the book should not be considered a one-off one-man opus. It is the sediment of a decade of collective research involving many scholars, including Anthony Atkinson, Emmanuel Saez, Gabriel Zucman, Gilles Postel-Vinay, and Facundo Alvaredo. It started at the turn of the millennium with publications about the evolution of income inequality during the course of a century in the United States and France (Piketty and Saez, 2001 & 2003;

Piketty, 2001 & 2003). Piketty and Saez's 2001 NBER Working Paper (published in 2003 in the *Quarterly Journal of Economics*) made the return of inequality in the United States visible to many through an extremely simple and striking U-shaped graph plotting the income share of the top one percent, which peaked at 20% in 1928, dropped after World War II, stabilized at a low of 8% in the early 1970s, and grew rapidly in the 1980s and 1990s, reaching 14% in 1998. This single graph helped to definitively bury Kuznets' (1955) optimistic argument of an inverted U-shaped evolution of inequality under capitalism: low before the Industrial Revolution, very high during it, and decreasing in the post-industrial era.

Although this simple message may seem revolutionary not only in political terms, but also as a groundbreaking scientific contribution, it was not that novel. The

data (US tax data), the method (top income shares), and the result (the return of income inequality) could already be found in previous works (Kuznets and Jenks, 1953; Feenberg and Poterba, 1993; Cutler and Katz, 1992). So how did *Piketty et al.*'s incremental innovations – e.g. greater historical depth, a more homogeneous dataset, a richer description of top incomes, a decomposition of the respective contribution of wages and property income – become a turning point in the social sciences? It is not merely because this research, which may only have been incrementally innovative in regard to our knowledge of the United States in 2001, led to the establishment of ignored facts when replicated in other countries – China, India, Germany, and so on – and led to the development of a unique comparative dataset on world income and wealth inequalities. It is also because it helped to shift the way we view inequality in the distribution of affluence, whether in terms of income, wages, or wealth.

The top one percent and statistical measures of inequality

Asking whether inequality has increased or decreased seems like a fairly simple question that should have a simple and unequivocal answer. However, it requires a statistical comparison of not just two distinct figures, but two full distributions. Answers might differ depending on the area one focuses on (most (top, middle, or bottom) and on the metric that one prioritizes in order to summarize a full distribution. This is the famous problem of Lorenz curves comparisons. The “Gini coefficient” (1921) is notoriously famous for being an all-encompassing measure that solves this comparison puzzle. As shown in Figure 1, it is one of the most used statistical measures of inequality, and it has enjoyed rapid growth, especially in the 1960s and 1970s. The fact that it could be implemented easily on limited samples of the population contributed to its adoption. However, it was criticized for not

being decomposable and not being fully in line with basic axioms of welfare theory (Atkinson, 1970), which led to alternative inequality coefficients (i.e. the Theil and Atkinson coefficients). Nevertheless, those coefficients share with the Gini the fact that they overlook the heterogeneity in the evolution of inequality at different levels of the income distribution.

In contrast, the *Piketty et al.* approach to inequality contributed to fully tackling the heterogeneity of the inequality evolutions: the rhythm of the evolution of the top one percent share might differ from that of the F95-99 or the F90-95. Without formulating any explicit criticisms of other measures, it provided the level of detail required to turn the quest for a per-

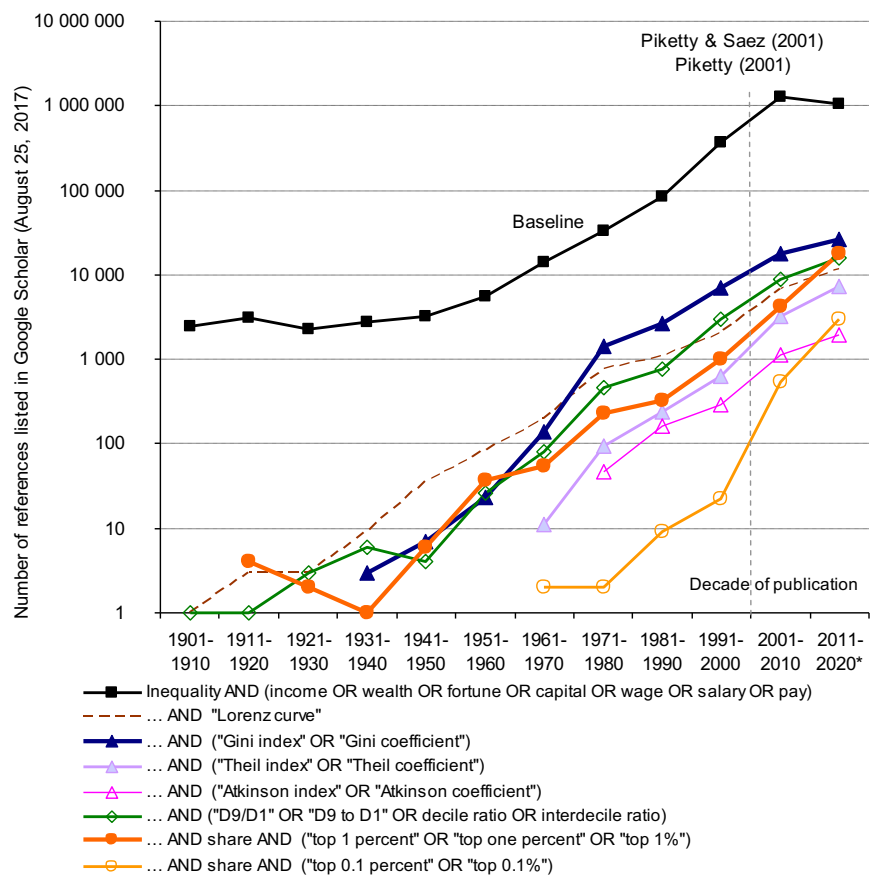


Figure 1. Statistical concepts used to approach inequality

Note and source: I collected the number of references published each decade that are listed on Google Scholar. I used the keywords *inequality AND (income OR wealth OR fortune OR capital OR wage OR salary OR pay)* as the baseline search, to which I added various statistical concepts on the measure of inequality. Hence, the Google Scholar search on the “top one percent share” concept was *inequality AND (income OR wealth OR fortune OR capital OR wage OR salary OR pay) AND share AND (“top 1 percent” OR “top one percent” OR “top 1%”)*.

Google Scholar is a publication database based on Google’s automated collecting of academic references in online scientific books and articles. It therefore comes with some limitations, including imprecise, undefined, and potentially inconsistent temporal coverage and some referencing errors, especially dating errors, especially for the earliest decades. However, besides being free of charge and easy to use, Google Scholar has the great advantage of providing a comprehensive list of publications, including books (Harzing and van der Val 2008). This graph has been included mainly to compare the evolution of the use of different measures of inequality. Absolute levels and evolutions where the number of references <10 should be treated with caution.

* The number of references for the full decade 2011–2020 has been estimated by multiplying the actual number of publications for 2011–August 2017 (6.5 years) by 1.54 (=10/6.5).

fect all-encompassing measure of inequality into an unnecessary illusion.

By focusing on the top of the affluence distribution, the *Piketty et al.* approach also uncovered a skewed world that had largely been ignored, especially when one used the more traditional inter-decile ratios: the gap separating the richest of the rich from the rest of the rich is as large as the one separating the rich from the rest of the population. Hence, the *Piketty et al.* approach not only popularized a “top one percent share” measure of inequality – which had already been abundantly used by Kuznets and Jenks (1953) – but even more importantly, it promoted a “top 0.1% share” (and “top 0.01% share”) measure of inequality that had been very rare before their work (Figure 1). Thanks to the level of detail provided by tax records and administrative datasets, it became possible to say something robust about the super-rich beyond unempirical Marxist concepts or anecdotal evidence provided by the press.

Finally, the great virtue of this approach lies in its proximity to ordinary people’s understanding of inequality. The sentence “In 20 years, the top one percent increased its share of the national income from 8% to 14%” is much more concrete and easier for non-statisticians to understand than “the Gini coefficient increased from 0.33 to 0.38”. The affluence share of the top one percent is much simpler to calculate than full distribution coefficients, and it reintroduces “real people” into the measure of inequality. The top one percent can be thought of as a group of persons that compete with other persons for the appropriation of the value created. This decomposition can easily fuel the view that “the misfortune of the little people makes the fortune of the great men” – a judgment which encapsulates the principles of exploitation according to Boltanski and Chiapello (2006, 375). This may be exactly why the 2011 #*Occupy!* movement was so prompt to adopt those statistical concepts as slogans, sometimes along with a *Piketty-Saez* graph on their T-shirts. “We are the 99%!” combined the universal “We the People” of the Founding Fathers of the United States Constitution with the “Us versus them!” (99% versus 1%) logic of many class struggles.

The top one percent and concepts of the upper classes

The concept of the top one percent (or the top 0.1% or 0.01%) identified by the *Piketty et al.* approach has increasingly become equated with a kind of social class.

This equation has not only been adopted by new social protests keen on naming an enemy, but also by the social sciences beyond economics, especially sociology and political science, as a complement to and even a substitute for traditional ways of designating the highest group in the affluence hierarchy. Figure 2 shows clearly that upper-percentile groups have become a challenge for class theories. Traditional notions such as “capitalists,” “bourgeoisie,” and “upper classes” were clearly dominant at the beginning of the twentieth century for thinking the gap between the top and the bottom (Figure 2). Their popularity declined sharply after the 1970s, however, and fractiles of affluence have been gaining ground in academic discourse ever since. Why do these statistical ranking measures have so much appeal?

On the one hand, the conceptual limits of fractiles are quite straightforward. The top one percent, especially the top one percent of the income hierarchy, mixes heterogeneous people in terms of their relationship to employment (employers and employees), position in the hierarchy (executives, managers, and experts, on the one hand, and those with no hierarchical power, such as rank-and-file traders, on the other), expertise (experts and, for example, professional athletes), type of income (wages and property income), age (retired people and active workers), and so on. Depending on the way income is defined and measured (i.e., individual or household income), the threshold is arbitrary and artificially creates a group within a continuum. People are not conscious of being part of this group, and others would not assign them to it without being told by social scientists what the relevant threshold is. In summary, we find neither objective nor subjective grounds for treating this statistical group as a solid social category.

On the other hand, despite the analytical clarity of various class schemes, their concrete application is not immune to the criticisms raised against the concept of the top one percent. In *Capital*, building on Smith and Ricardo’s labor theory of value, Marx puts the opposition between the capitalists, who own the means of production, and the proletariat, which possesses nothing but its labor power, at the heart of his class analysis (Marx [1867] 1887). Even in the late nineteenth century, the empirical application of this functional class analysis generated many doctrinal, theoretical, and political debates, especially regarding the class position of the growing intermediate strata of wage earners. Moreover, even class identification among capitalists, one of the core aspects of Erik Olin Wright’s class scheme (Wright 1997), faces many problems. Since class assignment rests on occupation,

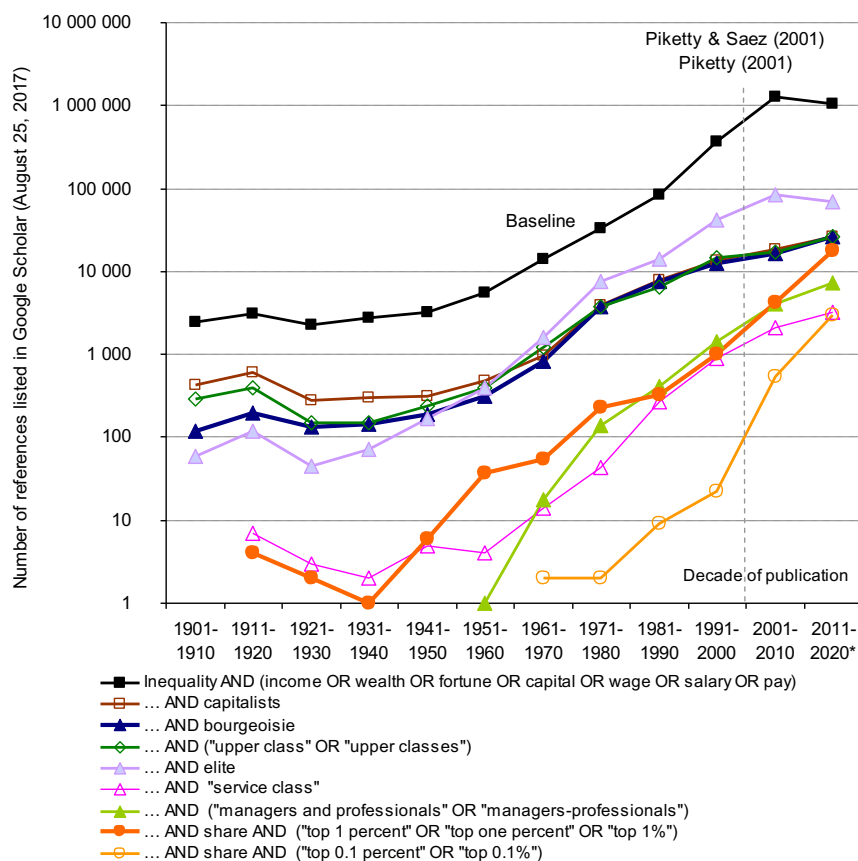


Figure 2. Categorical concepts used to approach inequality
 Note and source: I collected the number of references published each decade that are listed on Google Scholar. I used the keywords *inequality AND (income OR wealth OR fortune OR capital OR wage OR salary OR pay)* as the baseline search, to which I added various categorical concepts to designate the upper strata. Hence, the Google Scholar search for the bourgeoisie" was *inequality AND (income OR wealth OR fortune OR capital OR wage OR salary OR pay) AND share AND bourgeoisie*.
 I discuss the advantages and limitations of the Google Scholar database in the note in Figure 1.
 * The number of references for the full decade 2011–2020 has been estimated by multiplying the actual number of publications for 2011–August 2017 (6.5 years) by 1.54 (=10/6.5).

it is not well equipped to handle the twentieth century's separation of ownership and control – or wage earners' growing "capitalist" involvement through direct savings and pension funds. The delimitation of a group of entrepreneurs heading firms with more than 10 employees is at best a highly imperfect proxy for the capitalist class. Not only is this group becoming increasingly smaller, and therefore hardly useful for class analysis in most limited-size surveys, but it also consists mainly of entrepreneurs heading small and medium-sized firms. In the end, it represents mainly the upper fraction of the petty bourgeoisie.

Similarly, many class schemes make an important distinction between managerial power and expertise (Wright 1997; Erikson and Goldthorpe, 2002). However, when they approach the wage-earning upper strata empirically, this distinction is less useful, especially with the flattening of hierarchies in the service sector. The sociocultural proximity of

managers and experts and the high level of mobility between the two groups have led both class theorists (Erikson and Goldthorpe, 2002; Bourdieu, 1984; Desrosières and Thévenot, 1988) and applied researchers to merge the two groups into a single category. Hence, we end up with a large "upper class" consisting of entrepreneurs, executives, managers, experts, professors, and public-sector officers that – depending on the precise boundaries, the country, and the period – accounts for 5% to 20% of the population. While this grouping can make sense in order to capture the symbolic (and conflicting) roles these people play in defining the values that dominate the social order (Bourdieu, 1984), it lacks the precision and homogeneity necessary to measure economic inequality.

Moreover, the size of this group has changed substantially over time. In France, managers and professionals (*Cadres et professions intellectuelles supérieures*) doubled in size in thirty years, from 8% of the employed labor force in 1982–1984 to 16% in 2012–2014. Measuring inequality through the differential of incomes and outcomes (promotions, social mobility, and so on) between this group and blue-collar workers requires some

way of normalizing the growing size of this elite. Odds ratios and log-linear models may do a reasonable job of solving this problem, but only under the questionable hypothesis of nominal stability: in such models, a manager in 1982 is *ipso facto* considered equivalent to a manager in 2012.

In contrast, the use of affluence fractiles is much simpler and provides much greater robustness to longitudinal and cross-country comparisons. Positing a categorical equivalence between the top one percent of two countries or two periods appears to be a much more reasonable first-order proxy than using social classifications that are ultimately derived from historically and nationally defined occupational groups (Thévenot and Desrosières 1988). Therefore, the use of stable fractions of a given hierarchy makes it much easier to determine whether the top and the bottom are diverging, converging, or staying about the same.

Moreover, while *Piketty et al.* began by focusing on the top of the income hierarchy, the approach has now diversified to include analyses of the top of the wage and wealth hierarchies as well. One could argue that the upper hundredth (and especially the upper thousandth) of wealth (Saez and Zucman, 2016) is a better proxy for “capitalists” than “entrepreneurs heading firms with 10 or more employees.” Along these lines, one could imagine a top one percent of the control hierarchy or a top one percent of cultural capital, making it possible to renew the tools for sound social, longitudinal, and cross-national comparisons.

The top one percent and economic sociology

My argument could lead us to conclude that this *Piketty et al.* moment constitutes a challenge and a ground-breaking shift primarily for class theorists and specialists in the measurement of inequality. At first glance, it would appear to pose less of a challenge to economic sociology, which focuses mainly on markets and their embeddedness in society, be it a network embeddedness, or a cultural, an institutional, or a paradigmatic one (Granovetter, 1985; Zelizer, 1997; Fligstein, 1990; Callon, 1998). However, showing the various ways concrete market exchanges may differ from the Walrasian model is directly linked to the issue of inequality. If equivalents are not exchanged for their equivalents by anonymous and powerless actors, then markets are no longer neutral. Inequality can be viewed both as a cause and a consequence of economic exchange. The link to distributional inequality is most obvious for research into labor markets: social

capital, the Matthew effect, exploitation, opportunity hoarding, organizational resources, and hold-up power challenge the traditional view of the labor market as a neutral alignment of human capital, productivity, and wages (Lin et al., 1981; DiPrete et al., 2010; Goldstein, 2012; Avent-holt and Tomaskovic-Devey, 2014; Godechot, 2017). Moreover, inequality does play a key role in some general economic sociologies of the market. For instance, the field approach to economic exchange developed by Bourdieu (2005) and Fligstein and McAdam (2012) puts the inequality of resources at the core of the model. Actors with the most resources enjoy higher returns within a field due to the legitimacy they have garnered to define the rules of the game. Among firms, strong status hierarchies also determine exchange opportunities, prices, and profit and shape the distribution of value added (Podolny, 2005). New forms of classification based on digital socio-technical devices (such as the FICO score) both challenge and amplify traditional hierarchies and invisibly exclude the poor and minorities from essential goods and services (Fourcade and Healy, 2013).

The *Piketty et al.* moment has created an opportunity to amplify economic sociology’s concern for inequality. First, it provides a battery of tools to robustly measure the long-term evolution of inequality in many countries. Second, those measures can in turn serve as either dependent or independent variables. To summarize, *Piketty et al.* have mainly measured inequality in various ways; the point now is to explain its origins and grapple with its consequences. Understanding concrete market mechanisms is vital to this process, and economic sociology can provide the means to do so.

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How Did the Great Recession Affect Income Inequality in Spain?

by Pierre Blavier

Introduction

One of the striking features of the Great Recession of 2008¹ has been the great increase in unemployment in some Southern European countries, including Greece and Italy – and particularly in Spain, where the unemployment rate, defined as the percentage of unemployed people in the active population (employed and unemployed), skyrocketed from 8% in 2008 to 25% in 2014, before dropping slightly to 22% in 2015 (Figure 1). This significant increase is found regardless of the definition of unemployment used (self-declaration or the ILO's – International Labour Organization's – definition²). The number of Spanish unemployed registered at the Employment Office rose from 2 million to 6 million between 2008 and 2014, whereas the population, at around 46 million, remained more or less constant. This short-term development is part of a long-term trend of increasing unemployment since the early 1970s in Spain and more broadly in other European countries.

Figure 1 also shows that high unemployment is not a new phenomenon in Spain, where it was above 20% in the mid-1980s (1984) and 1990s (1994). Yet this is the first time that unemployment has increased so dramatically, has exceeded 25%, and has stagnated at this level for years (2008–2015). It is also the first time that it applies to an activity rate of 75%, with the unemployed peaking at 20% of the whole working-age population in 2014, whereas the previous maximum, in 1994, was about 15%. This pattern clearly breaks the convergence trend of the Spanish economy that had prevailed until then. It also raises questions about the long-term sustainability of this situation in re-

gard to the pension system, unemployment benefits, the fertility rate, and inequality.

Indeed, this phenomenon raises questions about the impact of such a sudden and lasting increase in unemployment on income inequality. When Thomas Piketty came to Spain in January 2015, he said “The great source of inequality in Europe is unemployment,” and he recommended looking at the bottom of the income distribution.³ Intuitively, increasing unemployment is likely to be accompanied by increasing income inequality, although other mechanisms may mitigate this effect. For example, capital income may decrease in times of crisis, which should moderate the rise of inequality.

The actual effect of increasing unemployment on income inequality remains open in the literature. For example, Aaberge et al. (2000) found a striking disconnect between the surge in unemployment and aggregate inequality indicators in Scandinavian countries in the early 1990s. Other authors suggest that inequality and recessions are generally correlated (Heathcote et al. 2010; Krueger et al. 2010). Regarding the Great Recession of 2008, some empirical studies (see Giannitsis and Zografakis 2015 for Greece; Grusky et al. 2011 for the US) have indicated that inequality has indeed increased, although not as much as could have been expected.

Ferrer-i-Carbonnel et al. (2013) have shown that before the Great Recession, Spain was no exception to this empirical rule of thumb, as inequality did indeed increase during the recession of the early 1990s. Nevertheless, as Pijoan-Mas and Sánchez-Marcos (2010) have argued, “inequality in individual net labor earnings and household net disposable income [in Spain] has decreased” from 1985 to 2000. In their extension of this period to the beginning of the Great Recession (2010), Ferrer-i-Carbonnel et al. (2013) came to the same conclusion. This trend was mainly due to a rather surprising decrease in the tertiary education premium (Felgueroso et al. 2016), decreasing unemployment since the begin-

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ning of the 1990s, and the development of the welfare state since the 1980s (Moreno 2006; Guillen and Pavalini 2015; Palomera 2015), with improvements in retirement pensions (Natalid and Stamati 2014), for example, and in unemployment and layoff benefits (Gil Martín 2002). Otherwise, few studies exist for the most recent period and for the Great Recession. Apart from a recent

study by Goerlich (2016), the case of Spain is often left aside (Jenkins et al. 2013; Gornick and Jantti 2013; Kollmeyer 2013), even though the country remains one of the most unequal in Europe (see the OECD Income Distribution Database⁴).

This paper presents some empirical evidence to fill this gap. How has inequality evolved in Spain during the Great Recession? What mechanisms and hypotheses can explain this development?

To answer these questions, we investigate the development of income inequality in Spain in recent years in the context of the past few decades, using indicators reflecting both objective income inequality and households' subjective declarations. We note a worsening of income inequality during the Great Recession, but the overall effect is rather limited in historical perspective and in comparison with the increase in unemployment. After establishing this puzzling result, we conclude by discussing mechanisms that are likely to explain it.

Our analysis is based on standard surveys of living conditions, namely the European Union Statistics on Income and Living Conditions (EU-SILC, 2004–2014) and the earlier European Community Household Panel surveys (ECHP, 1994–2001),⁵ which are comprehensive annual micro-level datasets of income and living conditions at the individual and household level. Capital income is usually subject to a downward bias in such data,

but here the top income revenues do not seem to be underestimated significantly: the top 1% of households concentrate 7% of total income, which is in the same order of magnitude as the figure computed in the World Income Database based on Spanish national accounts.⁶ Wealth effects regarding housing prices are not considered here.

Looking at the facts: Standard aggregate indicators depict a real but moderate increase in income inequality

Spanish inequality has generated a limited amount of literature. For the past, this dearth of research seems to be mainly due to a lack of empirical data, as almost no data existed before 1985 (Pijoan-Mas and Sánchez-Marcos 2010; Ferrer-i-Carbonnel et al. 2013).⁷ Nonetheless, this trend continues for the years since then, even though we now have data for them (Jenkins et al. 2013; Gornick and Jantti 2013).

Some results regarding historical trends have been established, however. Concerning wealth distribution, Alvaredo (2008, 18–106) has shown that, contrary to what is commonly thought, the income share of the top decile decreased during the dictatorship. It has in-

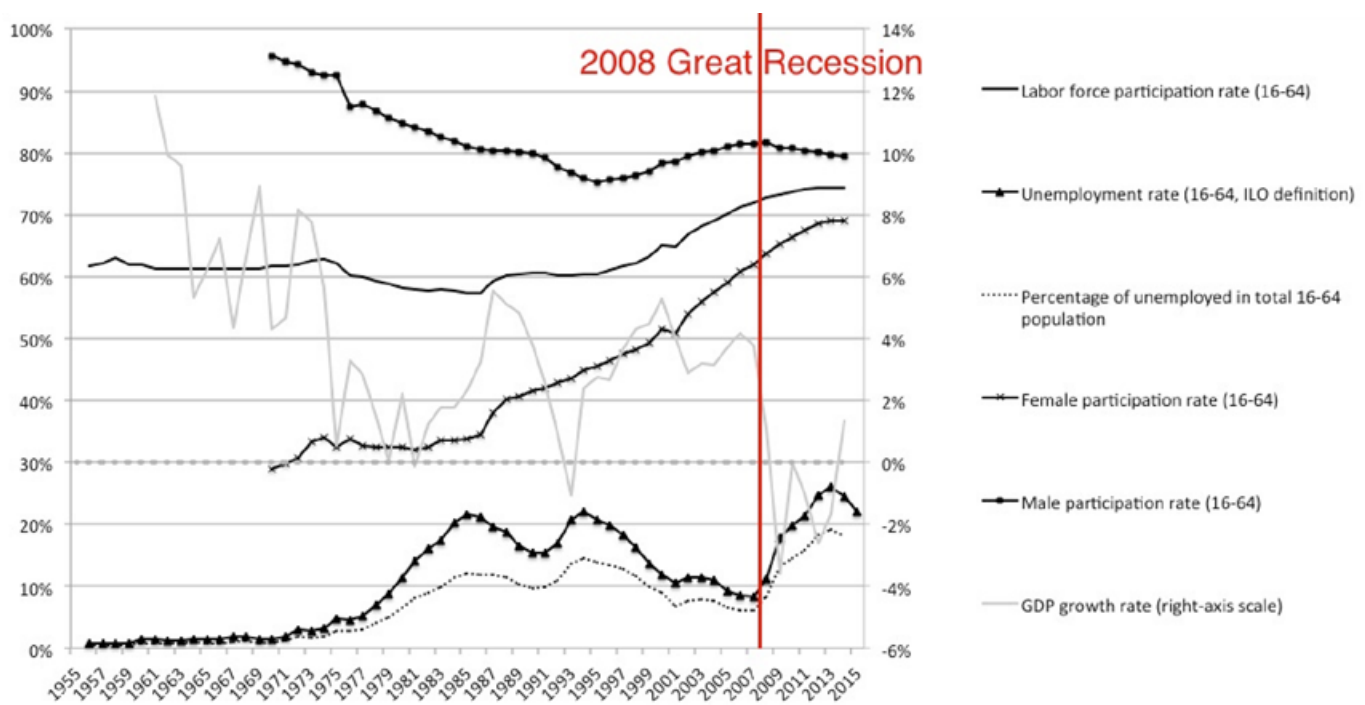


Figure 1. Harmonized development of growth and labor figures in Spain, 1956–2014

Source: OECD data, corrected for definitional changes.

Note: Real GDP growth is in gray and refers to the right vertical axis. All the other curves (in black) concern the labor market and refer to the left vertical axis. The activity rate is the percentage of the working-age population (15 to 64 years old, by international convention) that is active, either employed or unemployed. The unemployment rate is the percentage of unemployed individuals in the active population.

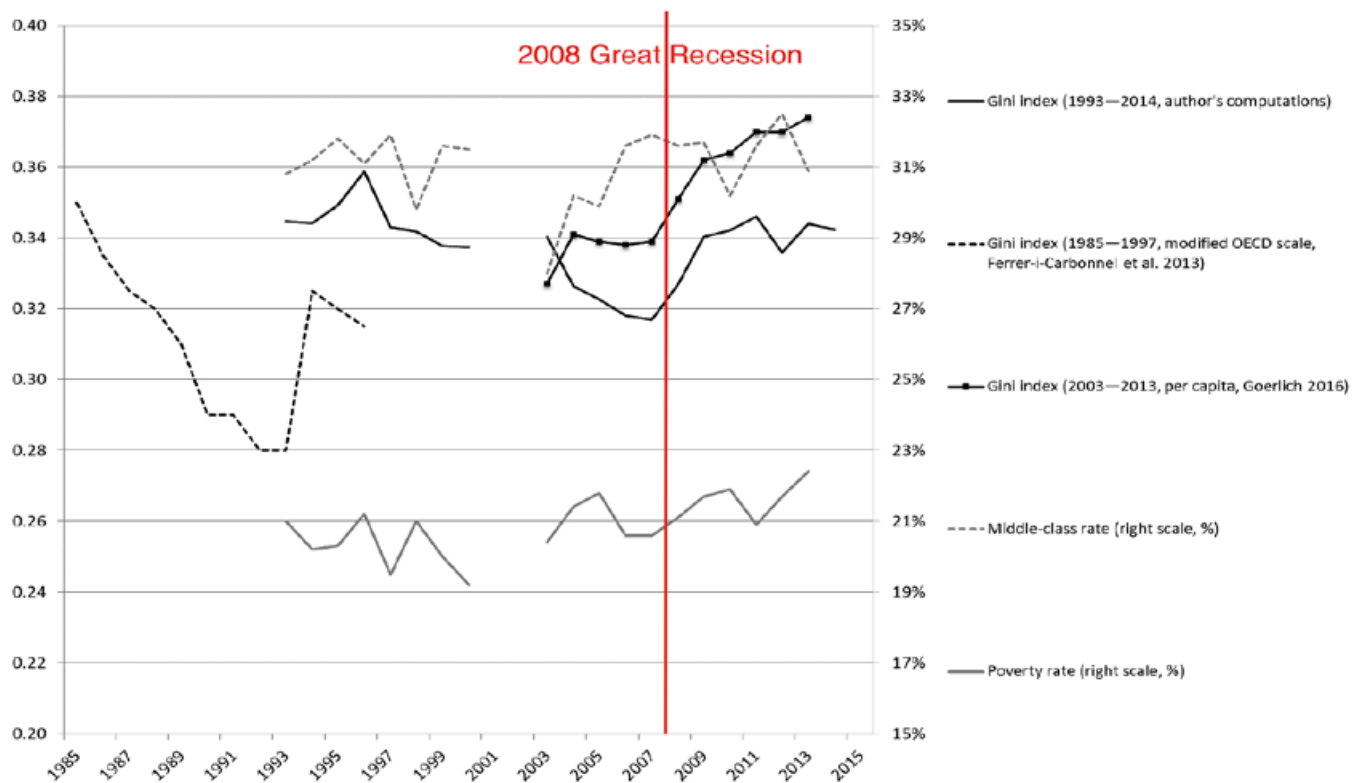


Figure 2. The development of different inequality indexes using different calculations, 1985–2014

Source: European Union Statistics on Income and Living Conditions (EU-SILC, 2004–2014) and European Community Household Panel surveys (ECHP, 1994–2001).

Note: The middle-class and poverty rates (in gray) refer to the right axis, and all other Gini indexes (in black) to the left one. For 2014, the Spanish Gini coefficient is 0.34 (modified OECD scale computed from household data). The 1985–1996 series corresponds to the calculations presented by Ferrer-i-Carbonnel et al. (2013) with modified OECD scale and consumption survey data. The discrepancy has already been noted by Pijoan-Mas and Sánchez-Marcos (2010) and Ferrer-i-Carbonnel et al. (2013, 14). The third measure is presented by Goerlich (2016, 53) at the individual level (and with a nominal deflator, but whose effect remains marginal since the inflation rate is very low), which explains why he finds a higher Gini index. All values are in real (constant) terms, indexed to the OECD Consumer Price Index (CPI) with 2010 as reference.

creased since the 1980s, following the same trend as in many other European countries. Alvaredo notes that the housing bubble may have limited this trend, as it benefited the middle class most.

One specific aspect of the situation in Spain is the sharp decline in the education premium since the beginning of the 1990s. Felgueroso et al. (2016) and Bernardi (2012) test various hypotheses for the correlation between education and earning power, but have only partly succeeded in explaining it. This development contrasts greatly with what has been noted, for example, in the United States, where education inequality is often cited as one reason for the current growth of inequality (Autor 2014). The situation in the United States is attributed to a lack of college graduates who are adequately employed, which fuels fierce competition to hire them and thus increases their wages (Hidalgo 2010). This explanation does not hold in Spain (or other countries, including France), where tertiary education has increased significantly since the 1980s.

Another specific aspect we observe is that Spain, like other Southern European countries that experienced

dictatorships and until recently were very rural (Greece, Portugal), is characterized by an underdeveloped welfare system in comparison with other European countries. This causes Spain to be one of the most unequal countries in Europe, with a very high Gini index, for example.

At first sight, it seems obvious that income inequality in Spain has increased during the Great Recession. We use the Gini index to investigate this view because it is a standard indicator of inequality, easy to compute, well known, and quite intuitive, and because it encompasses the whole distribution. The Theil index, which shares these characteristics with the Gini (with the exception that it may be less intuitive),⁸ yields similar results. Below, we use income deciles and ventiles to zoom in on specific aspects of income distribution in Spain, particularly at the bottom.

If we apply the Gini index to per capita disposable income,⁹ we find that this income has indeed increased significantly from 2008 to 2012 (Figure 2), a period during which there were two waves of increased unemployment in Spain (from 11% to 18% in 2008–2009 and from 20% to 26% in 2011–2012¹⁰). This trend is consistent

with the official figures of Spain's National Statistics Institute (Instituto Nacional de Estadística, INE)¹¹ as well as a recent report by Goerlich (2016, 24). This consistency is to be expected since both of these sources use the same data as we do (EU-SILC).

However, we would like to show that this observation is not so trivial, for several reasons. First, the fluctuations of the Gini index depend on numerous parameters:

1. The dataset used and the income sources it takes into account. Figure 2 shows that the Gini index may change depending on which dataset is used.
2. The scale of equivalence used: the square root of household members, the OECD scale, the OECD modified scale,¹² or no equivalence weights, i.e. per capita (each household member counts for one).
3. The use or non-use of the survey weights.
4. The statistical unit: household or individual.
5. The range/scope of the population (above 18 years old or not).

6. The use of a deflator to take into account the regional price index or calculation in nominal terms, although this parameter has little effect (Goerlich 2016).

The differences in these parameters explain why varying Gini index levels are found in the literature: it is common to find variations between different estimates, as can be seen in Figure 2. This suggests that using the percent changes of the Gini index as the result variable is not very reliable. It blurs the observation of inequality and makes it difficult to know what we are talking about, all the more so because the conventions employed are rarely specified. This basically suggests that more attention should be paid to the statistical source employed.¹³

A second point concerns the scope of time taken into account. Indeed, we have seen that inequality is once more on the rise, after the opposite trend had prevailed since the 1980s (Ferrer-i-Carbonnel et al. 2013; Alvaredo and Saez 2009; Pijoan-Mas and Sánchez-Marcos 2010; Bonhomme and Hospido 2012). But the change is hardly major, and inequality is not much great-

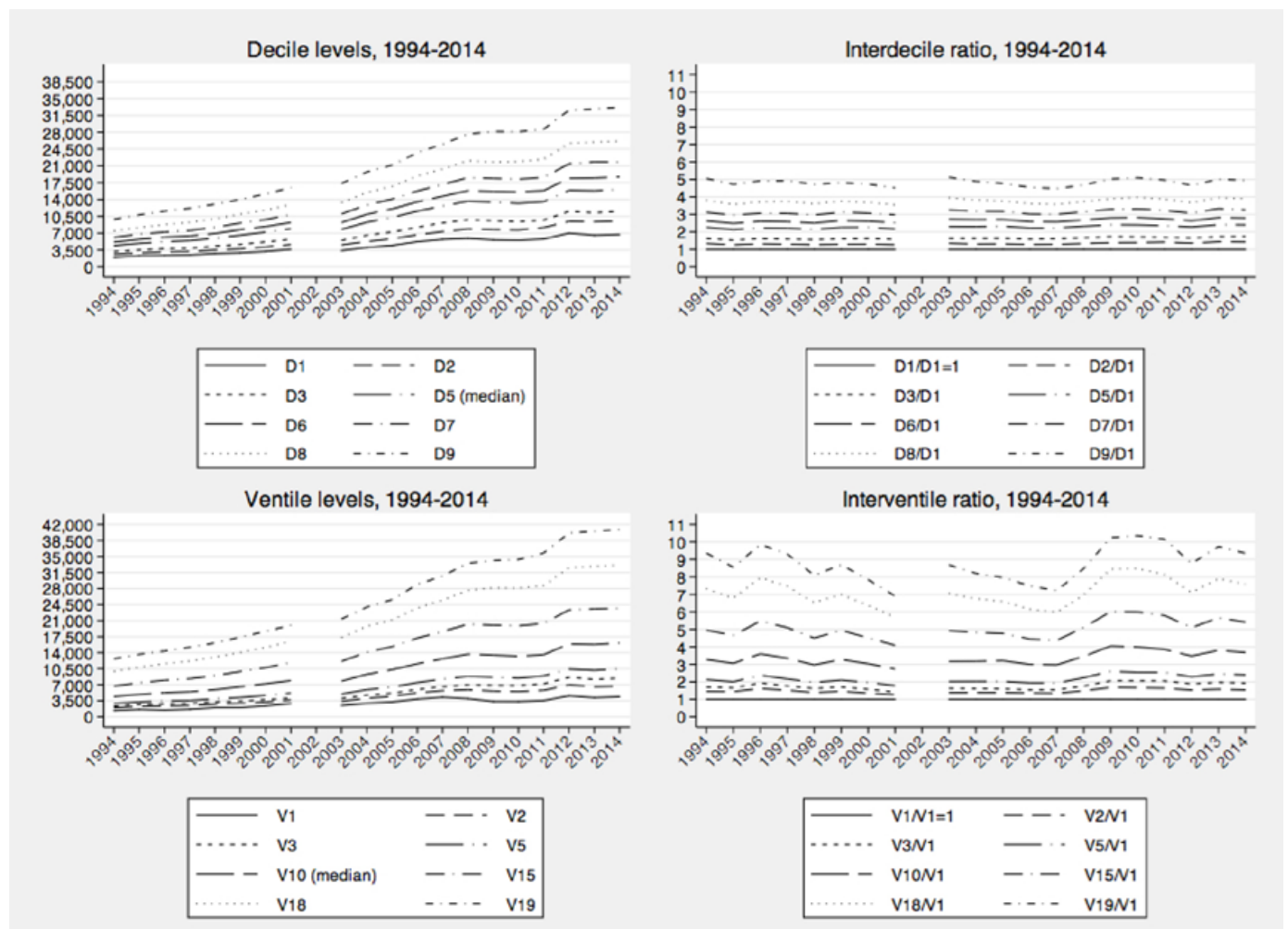


Figure 3. Real annual disposable income per consumption unit in Spain, 1994–2014: Development of interdecile and interventile levels and ratios
Source: ECHP, EU-SILC 1994–2014.

Note: In 2013 in Spain, the real annually disposable income per consumption unit (square root of the number of members in the household) of the tenth decile (D9) is 31,500 euros. This amounts to five times that of the first decile (D9/D1).¹⁴

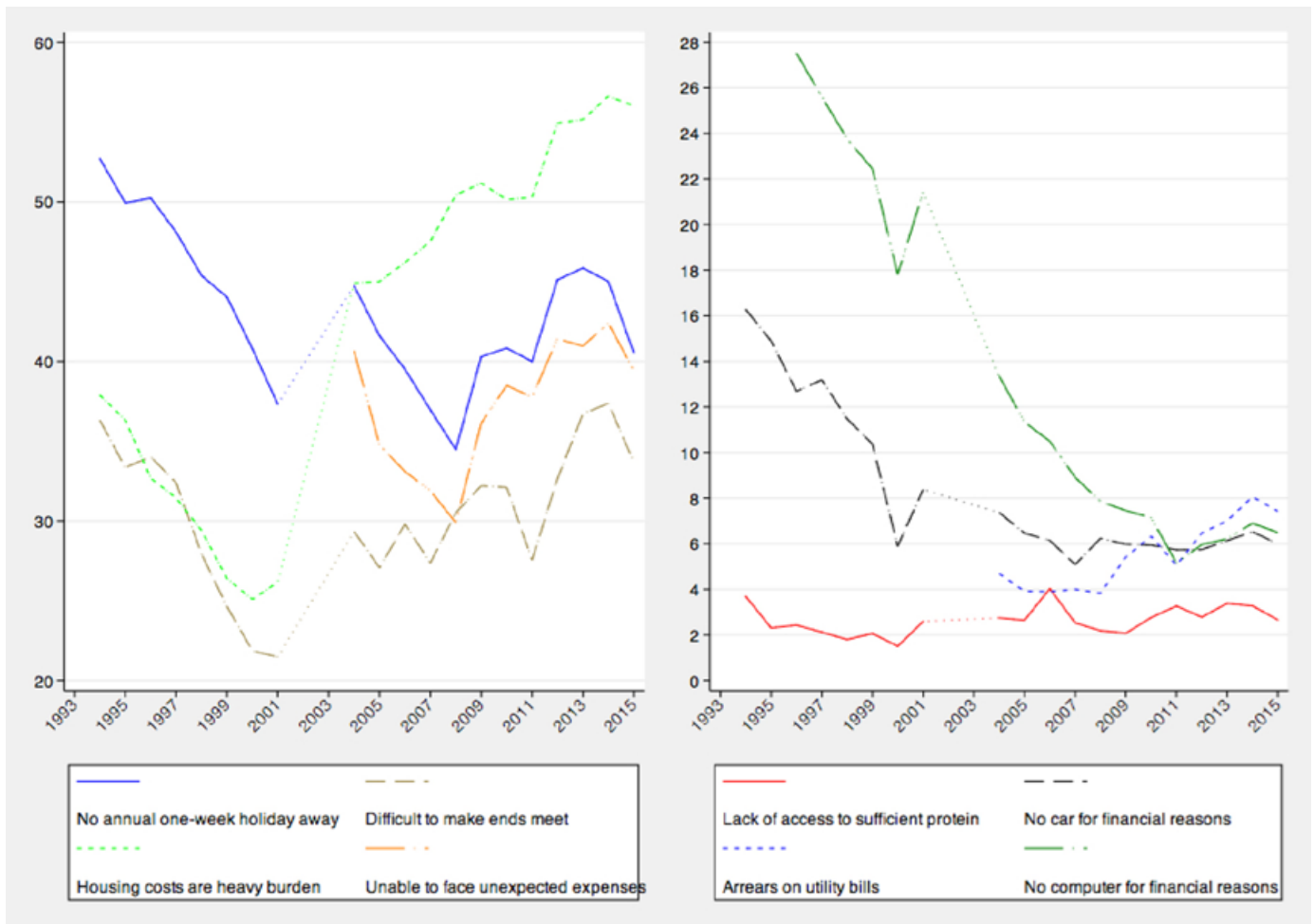


Figure 4. Economic hardships in Spain, 1994–2014 (in percent of households)

Source: ECHP & EU-SILC data.

Field: All households in the survey.

Note: In 1994, 52% of Spanish households could not pay for one week of vacation away per year. The jump between 2001 and 2004 is not interpretable because it is due to a change of datasets.

er than it has been in the past, as can be seen in Figure 2. It is always problematic to determine the extent to which a given change in inequality should be considered important, but in this case it is rather clear that income inequality has hardly responded to the explosion in unemployment (from 8% in 2007 to over 20% until 2016) experienced by Spain during the Great Recession.

Third, it should be noticed that the same trend is observed if other indexes are considered. Figure 2 reports the development of three standard inequality indicators for disposable income from 1994 to 2014: the Gini index (left scale); the poverty rate (60% of disposable income median); and the middle class, defined by Pressman (2007) as households receiving between 75% and 125% of the median income. Pressman's hypothesis of a growing destabilization of the middle class during recessions is not supported by the data. Nominal median income has stagnated but not decreased, so the lack of change in the poverty rate cannot be explained by its fluctuations, as some have argued.

Similar pattern with decile analysis

Another explanation for this surprising result may lie in the fact that the previous indicators are very aggregate ones, which may make specific trends within the income distribution, as well as effects restricted to some parts of it, invisible. To tackle this problem, we analyze the development of nominal disposable income deciles, ordered from the poorest (first decile) to the richest (last decile). This approach has clear limits (Piketty 2014, 266–68): deciles with very low numbers of observations are unreliable (e.g. capital income for the poorest deciles), and by definition deciles do not zoom in on specific parts of the income distribution (e.g. the top 1%). But here it is appropriate because we are not focusing on the extremes of the distribution (top or bottom 1%) and disposable income is consistent across the distribution even though the income sources vary (no large part of the distribu-

tion has zero disposable income, as can be the case for specific types of income derived from capital). Results are presented in Figure 3.

This approach results in remarkable stability between deciles. For example, as depicted by the top right graph in Figure 3, the ratio between D9 and D1 remains around 5 for the whole period. This means that all deciles have grown at an approximately similar rate, indicating that inequality remained constant if considered in a multiplicative way. To see any decrease in inequality, one has to look at the lowest ventiles (bottom two graphs of Figure 3): only the first ventile exhibits a significant decrease, thus triggering a clear rising interventile ratio with the top part of the distribution (bottom left graph of Figure 3) from 7 in 2008 to more than 10 in 2010.

Yet very different conclusions can be drawn if we consider interdecile income differences: in 1994, the difference between the ninth decile and the first was $D9-D1=15,000-3,000=12,000$ euros (per consumption unit), whereas in 2013 it was $30,000-6,000=24,000$ euros. Hence very different results are obtained depending on whether we consider inequality in terms of ratio or difference. To the best of our knowledge (Combessie 2011), there is no clear-cut reason to use one over the other, although nowadays the multiplicative method is more commonly used. Furthermore, it is also surprising to note that incomes have barely dropped at all. Indeed, incomes have decreased only slightly for the lowest deciles or ventiles, but these decreases can be considered very limited if viewed in the long run. Goerlich (2016, 29) also finds mean fluctuations by quintiles over different periods (2003–2007 and 2008–2013). He finds a decrease in incomes of -7% for the lowest quintile (0–20%) in 2008–2013, which is not very much of a difference compared with the trend in the long run. It seems appropriate, therefore, to speak of stagnation in the lowest quintile.

This result stands in sharp contrast to the trend in unemployment. It can easily be shown that unemployment has been concentrated in low-income groups. Unemployment benefits may also be at stake: their redistributive effect depends on the replacement rate and the share of the unemployed covered by the system. Both have been modified during the Great Recession. The replacement scheme has been lowered by between 10% and 20% by two austerity labor reform laws (2010, 2012). The cover rate has progressively declined, from more than 60% in 2011 to 50% in 2014, according to the official data of Spain's National Statistics Institute.

Reductions in working hours or wages have not played a great role in income developments. Only a small share of workers has been affected by such reductions, and the reductions for those who have been affect-

ed – such as the civil servants, who lost an extra month of salary beginning in 2012, whose wages were frozen, and who had a wage drop of 5% in 2010 due to the Zapatero reform – have not been substantial. Nor have there been any significant developments in wage inequalities.

A contrasting picture with economic hardship indicators

Some standard hardship indicators suggest that the Great Recession marks a clear turning point in terms of economic hardship, as can be seen in Figure 4.

The curves in Figure 4 are U-shaped: a decrease in hardships after the recession at the beginning of the 1990s, relative stagnation, and then a worsening since 2008. For example, difficulty facing unexpected expenses rose from 30% to more than 40%. In keeping with the housing bubble, housing indicators have increased the most. For example, arrears on utility bills increased from 4% to 8% in 2010. Particularly impressive is the upsurge in households that consider their housing costs a “heavy burden”: the percentage of households in this situation was already high before 2008 (about 45% in 2004), but increased almost continuously to over 55%, whereas it had decreased a great deal in the 1990s. This development suggests how important the housing component is in household budgets. The trends for other items such as a lack of access to sufficient protein and the inability to afford a car or computer are much less pronounced, partly as a result of the decreasing cost of these items over the last few decades.

Overall, the impact of the Great Recession is real, but it has still not brought Spain back to the levels at the beginning of the 1990s. And there is a sharp contrast between the trend in terms of hardship indicators (Figure 4) and the development of inequality presented above. How can this contrast be explained? Three hypotheses can be proposed. First, it can be attributed to the decline in income at the bottom of the distribution, as moderate as that might seem: in relative terms the decline for the bottom ventile is more important than for other ventiles. Second, it can be the result of declining income trajectories, which have increased considerably with the Great Recession as a result of unemployment. Finally, we might be observing a phenomenon Gollac described in another context (Gollac 1997). He noticed a sharp decline in how some professions, such as nursing, assessed their working conditions between surveys conducted in 1986 and in 1991. While no objective change in working conditions could account for this decline, impor-

tant social mobilization in those sectors may have led workers to realize how poor their working conditions were. In other words, the meaning of a questionnaire may depend heavily on the context and the state of the world observed through the survey, and not only on what it is expected to measure objectively. In our case, the hardship indicators may also reflect the politicization of the issue of living standards in Spain, in keeping with the framing promoted by the Platform for People Affected by Mortgages (PAH, *Plataforma de Afectados por la Hipoteca*) and other militant groups (including Podemos and various organizations of the unemployed): the Great Recession has not only affected living standards in Spain, but also the way the Spanish population perceives them. All these explanations are likely to be part of the answer and need further research.

Synthesis: Various hypotheses

Different mechanisms may be responsible for the paradoxical development presented here.¹⁵ First, a decrease in employment earnings can be compensated for by alternative sources of income, such as benefits deriving from social policy (automatic sta-

bilizers), returns on capital, or familial solidarity. Secondly, behavioral changes can play a role: people adapt their behavior to the economic context. This is especially true for young people, who may remain in their parents' home longer or study longer; for spouses, who may enter the labor market to compensate for their partner's lost job (added worker effect); and older unemployed, who may retire earlier than expected. Finally, it may be that those most affected by the Great Recession have emigrated, leading to a change in the census composition and thus to observational fallacies. Two different kinds of people are likely to have chosen this exit option to cope with the Great Recession: immigrants and unemployed Spanish youth. This possibility would be consistent with the stagnation of the total population in Spain, which has remained almost constant at 46 million since 2009.¹⁶

Finally, here we have considered only the case of Spain. In further research, it would be fruitful to compare developments in Spain with those in the Southern European countries hit hardest by the Great Recession (Greece, Italy, Portugal), and with those in the Northern part of the Continent. The distance between the Northern countries and a country like Spain has probably grown since 2008.

Acknowledgements

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Endnotes

- 1 Referred to here as the Great Recession.
- 2 According to the ILO's 1982 definition of unemployment, unemployed persons are between 16 and 64 years of age and meet three criteria: they have not worked in the previous week; they are available to accept a job within the next two weeks; and they have actively looked for a job in the last month.
- 3 Spanish national newspaper *El País*, January 11, 2015.
- 4 <http://www.oecd.org/social/income-distribution-database.html>
- 5 This survey does not offer sufficient precision regarding income, but it inspired the EU-SILC and yields coherent results.
- 6 As explained in the methodological note found at <http://wid.world/>.
- 7 Some data does exist, but it is either hardly reliable (consumption surveys) or do not document incomes (e.g. the Spanish and European Labor Force Survey, LFS, that began in 1976).
- 8 The Theil index can also be broken down into various parts, but based on groups and not on income sources, unlike the Gini index.
- 9 Household disposable income consists mainly of the wages of all household members, self-employed and capital incomes, and inter-household and welfare transfers including unemployment benefits and retirement pensions minus social insurance contributions and taxes on income and wealth.
- 10 Source: Encuesta de la Población Activa (EPA) Survey, Instituto Nacional de la Estadística (National Statistics Institute of Spain).
- 11 See <http://www.ine.es/jaxiT3/Tabla.htm?t=9966>.
- 12 The OECD modified scale assigns a value of 1 to the first household member, 0.5 to each additional adult, and 0.3 to each child.
- 13 Piketty's success is partly explained by the carefulness of his data and the fact that he makes the data he uses publicly available.
- 14 Taking the ratio relative to the median gives same results.
- 15 These mechanisms are tested empirically in my doctoral thesis.
- 16 Source: Spanish census. Obviously this trend is also due to other factors, including aging.

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The Comparative Organizational Inequality Network: Toward an Economic Sociology of Inequality

Donald Tomaskovic-Devey

with Nina Bandelj, Irene Boeckmann, István Boza, David Cort, Dustin Avent-Holt, Olivier Godechot, Gergely Hajdu, Martin Hällsten, Lasse Folke Henriksen, Andrea Hense, Are-Skeie Hermansen, Joon Han, Feng Hou, Jiwook Jung, Aleksandra Kanjuo-Mrčela, Joseph King, Naomi Kodama, Alena Krizkova, Zoltán Lippényi, Silvia Maja Melzer, Eunmi Mun, Andrew Penner, Trond Petersen, Andreja Poje, Anthony Rainey, Mirna Safi, and Zaibu Tufail

An odd thing about economic sociologists is that most of us do not spend much time studying economic inequality. Market transactions, institutions, status processes, and cultural production in markets are all fair game, but who wins and who loses, not so much.

Imagine for a moment you took some of the basic ideas from economic sociology and began to ask distributional questions about who wins and who loses in economic transactions. We can quickly embrace the obvious: an economic sociology of inequality would not begin with assumptions about market competition or efficiency, nor would it expect capital investments – human or physical – to yield equivalent returns independent of the relational context of exchange. An economic sociology of inequality would instead wonder about the degree of embedded versus exploitable relationships, and perhaps the valence of categorical, cultural, status-related, and institutional influences on

these exchanges. In short, it would take inequality distributions to be both dynamic and relational.

But that might be the limits of a conventional economic sociology, which is not quite far enough to get to a useful economic inequality agenda. To do this, we would have to take another step and focus on the actors that benefit or lose in economic exchanges. Since the key actors for both the accumulation of surplus and its distribution are organizations, we would need to focus on the organizational nexus around which product market exchanges happen and within which labor market transactions are enacted.

One reason that economic sociologists have paid little attention to economic inequality is that almost all distributional data available in the social sciences have been collected within a status attainment (sociological) or human capital (economics), individual actor, framework. These data are typically neither relational nor organizational. The lack of data has probably also slowed the development of an authentically relational theory of market inequalities, since there was nowhere to go with it – empirically, at least. Fear of crossing the invisible barriers between economic sociology and stratification sociology and between sociology and economics may have played a role as well.

If you are with us so far, then we have some exciting news to share with economic sociology, particularly European economic sociology. Times have changed and there is now an emerging economic sociology model of relational inequality and a treasure trove of administrative data on organizations and their employees. In some countries these data extend to owners and capital income as well.

Relational inequality theory (RIT, Avent-Holt and Tomaskovic-Devey 2014) proposes that organizations *pool income* and then distribute it to stakeholders. *Closure* (versus incorporation) and *exploitation* (versus embeddedness in Granovetter's sense) are the key mechanisms producing distributions both between and within firms. Exploitation includes such things as wage discrimination, wage theft, monopoly and network rent-seeking, and any transfer of income or other organizational resources based on power and status inequalities between actors. Closure implies various forms of exclusion and monopolization of resources, drawing firm boundaries to externalize costs and internalize profits, and refusal to hire or trade. In a relational inequality framework, the salience and valence of these mechanisms as well as tradeoffs between them are produced by *claims-making processes*, which in turn are heavily tied to relational processes of categorization, linked status distinctions, and inhabited institutions (see for example Bandelj 2016; Godechot 2017;

Roscigno and Wilson 2014; Tilly 1999; Wilson and Roscigno 2014; Zelizer 2012). These processes are, of course, all embedded – in Polyani’s sense of institutional embeddedness.

Although this theoretical background is roughly compatible with the long empirical record of human capital studies, it also provides predictions as to the organizational contexts in which earnings are more or less likely to be tied to individual and collective productivity, it foregrounds multiple status-based claims on organizational resources, and it theorizes variation in both workplace wage/price setting practices and institutional context. For example, the composition of the labor force and its categorical boundaries contribute considerably to the local balance of power within workplaces, both among employees and between employers and employees (Tomaskovic-Devey et al. 2017).

There is growing empirical evidence that the relative status composition of workplaces (e.g., gender, race, education, permanent versus temporary contract, etc.) explains variation in occupational wage gaps, bullying and sexual harassment among workers, formal versus informal merit evaluation practices, the relative autonomy of workers in the labor process, gender wage gaps, immigrant-native wage gaps, and sex and race discrimination. Previous RIT research has also demonstrated that the influence of particular categorical distinctions varies as a function of national labor market institutions, the formalization of personnel policy, managerial accountability, local versus centralized wage setting, product market competition, team versus hierarchical labor process organization, organizational orientation toward merit-based compensation, and pay-for-performance systems (see review in Tomaskovic-Devey 2014).

Concurrently, many national governments, particularly in Europe, are now making high quality administrative data available to the research community. The use of these data, collected by state agencies for social security and taxation purposes, is typically restricted (for confidentiality reasons). Past research, primarily in economics, has used such Linked Employer-Employee Data (LEED) to investigate the relative importance of individual and workplace wage setting, mainly finding more workplace variation in wage setting than theoretically anticipated in a human capital framework (e.g. Lazear and Shaw 2009). This literature also shows that 40% or more of the total national variation in wages was attributable to similarly skilled

workers working in different firms. Current research using LEED data has also revealed that rising national inequality is often tightly coupled with rising inequality between workplaces. Recent studies in economics have shown that up to two-thirds of rising inequality in the US (Barth et al. 2016), Germany (Card et al. 2013), and Sweden (Skans et al. 2009) has been produced by rising between-workplace inequalities.

The Comparative Organizational Inequality Network (COIN) is putting both RIT and LEED data to work to build an economic sociology of inequality. We

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are a network of twenty-seven (at last count) social scientists with access to administrative data linking employees and their workplaces over the last twenty or so years for thirteen (at last count) countries. COIN is a loose collective, comprised of scholars in sociology, industrial relations, business, and economics. Our theoretical center of gravity is closest to relational inequality theory and economic sociology, but we have not yet imposed any orthodoxy tests. Among the thirteen countries, nine are in Europe (Czechia, Denmark, France, Germany, Hungary, the Netherlands, Norway, Slovenia, and Sweden). The other four countries are in Asia (Japan, South Korea) and North America (Canada, the US). We have meetings twice a year (so far in London, Bielefeld [twice], Prague, and Ljubljana; next up is Paris in January 2018) to build trust and reciprocity, coordinate research protocols, and generate joint projects. COIN members now have many projects under development, including ones investigating the organizational and national variation in occupational, gender, and citizenship inequalities; firm employment volatility; the role of large firms in inequality generation; and the organizational production of national inequalities.

To give you a small taste of what we are up to, we will briefly present three sets of (*very provisional*) empirical results from COIN signature papers. Each of these “signature” papers is still under construction; we hope to add more countries and more dynamic analyses as they

develop. Our “signature papers” attempt to use as many countries as possible from the COIN network in order to make significant empirical contributions to contemporary social science, while both developing scientific protocols and legitimizing the COIN enterprise.

Gender inequality variation

The gender stratification literature has focused on occupational, workplace, and job segregation as the key drivers of the gender wage gap. These have been treated as both powerful and generic mechanisms in that literature (e.g. Petersen and Morgan 1995; Petersen, Penner, and Høgsnes 2014). These studies have been concerned more with the size and segregation sources of national gender pay gaps than with their organizational and national variation. One of the first COIN papers, with leadership provided by Andrew Penner (UC Irvine), has taken this segregation logic and extended it to a comparative framework, asking if this closure mechanism is the same across countries and if within-job gender pay gaps are always small. Not surprisingly, we are finding that the answer is no to both questions. National gender regimes vary, but we are also finding that the erosion of the closure mechanism makes room for the development of new, more intimate exploitation mechanisms. Figures 1 and 2 produce some provisional estimates.

In Figure 1, we see substantial variation in the gender earnings gap, which is only 5% in Sweden after education, age, and part-time status are controlled (blue – population bars), rising to 40% in Japan. In Sweden, segregation mechanisms have a limited effect. In France, the dominant segregation mechanism happens at the job level. In the Netherlands, it is a combination of establishment and job segregation; in Hunga-

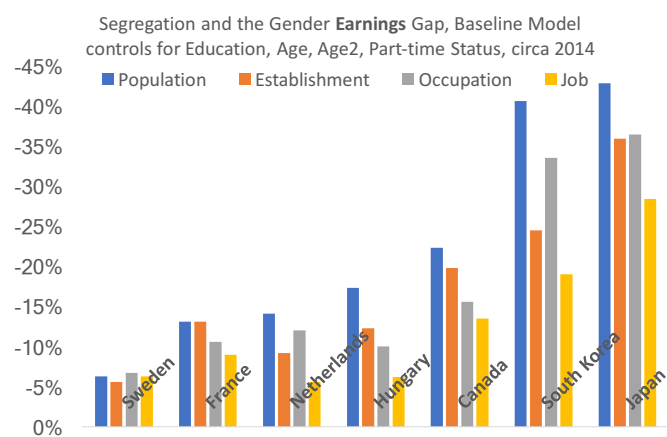


Figure 1. Country variation in levels and segregation mechanisms producing the gender earnings gap (in %)

ry and Canada, the results indicate that all three segregation mechanisms work together; while in South Korea and Japan, most of the gender earnings gap is produced within jobs.

The mechanisms producing gender inequalities are not constant, but vary with national institutional contexts. In Sweden, the country with the smallest gender earnings inequalities, essentially all of the (small) gender pay gap appears to be produced within jobs within workplaces (see Figure 2). Only in the middle gender inequality countries (Netherlands and Hungary) is most of the pay gap produced by segregation processes (when measured as job within workplace segregation). Next steps are to expand the list of countries included and to interrogate how these gaps and mechanisms are changing over time and vary between organizations within countries.

National inequality trends

We are also studying the workplace generation of national earnings inequalities trends across many countries. We focus on decomposing earnings inequalities into those that are being produced by employee-employer exchanges within workplaces and those that are being produced by exchange relationships between organizations. Economists have already discovered for Germany (Card et al. 2014), Sweden (Skans et al. 2010) and the U.S. (Barth et al. 2016), that *between* workplace inequality is growing more rapidly in those countries than *within* workplace inequalities (although in all three the within workplace component is larger). Economic sociologists have pointed out that the disintegration of the vertically integrated mega firm (Davis 2013) through outsourcing, subcontracting, and various forms of labor externalization and exchange part-

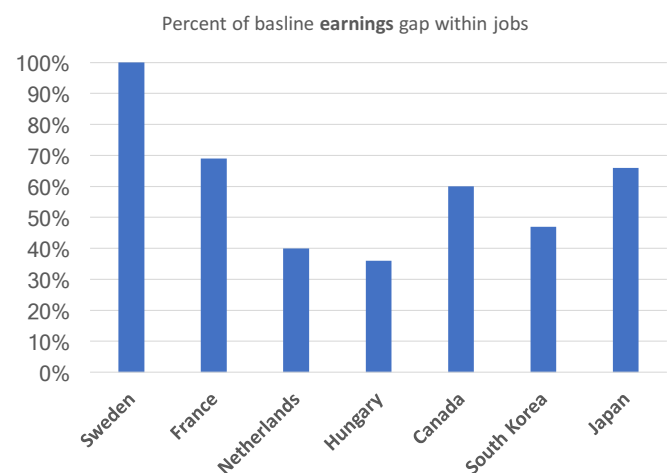


Figure 2. Percent of original baseline gender earnings gap which remains within jobs

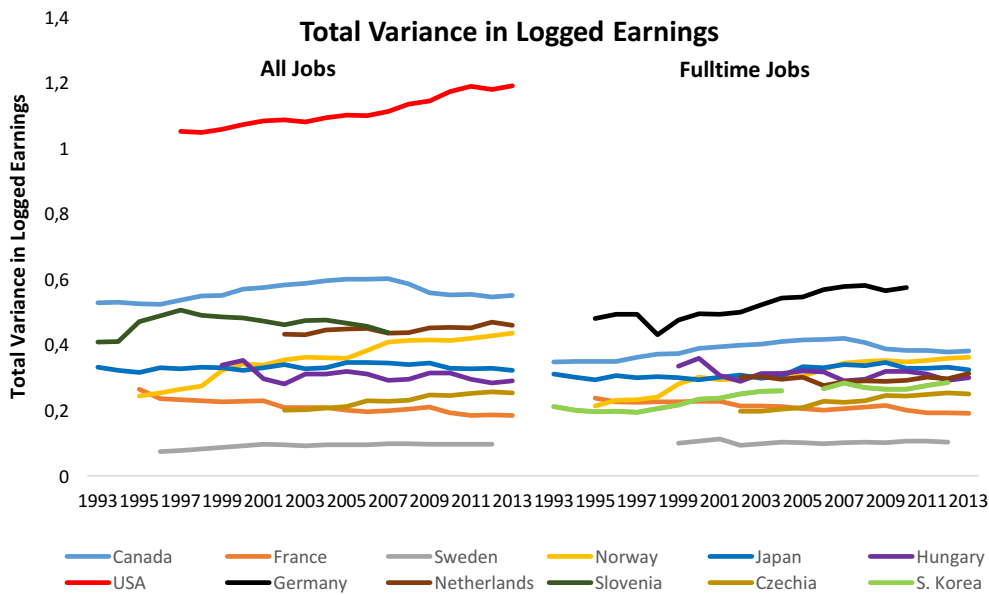


Figure 3. Trends in the between-workplace variance component for all jobs and for full-time person job matches. Note: Slovenia appears only in the all-job figure, Germany and South Korea only in the full-time figure.

ner exploitation have been growing in tandem (Gereffi 1996; Weil 2014; Whitford 2005). We bring these two insights together and are exploring the degree to which this between firm inequality growth is universal (it is not) and the degree to which it is influenced by national labor market and social welfare institutions (a lot). Donald Tomaskovic-Devey (University of Massachusetts) is taking the lead on this paper.

Figure 3 provides the trends in the *between-workplace* variance component. All of the countries, except Slovenia and Hungary, show clear patterns of rising between-workplace inequalities, although the timing and steepness of change vary considerably. Hungary has a stably high between-firm inequality component, while Slovenia’s between-firm share is in the middle of the distribution and declining (although this time series stops in 2007). Each of the other countries saw a substantial rise in the between-workplace variance component, ranging between 3% (Norway) and 8% (Netherlands). The trends are largely similar for all jobs and full-time only jobs, with two exceptions. In Japan, between workplace inequality

for full-time employees is rising more steeply, and in the Netherlands the change is more dramatic in the all-job sample. The latter difference is attributable to the very large share of part-time employment in the Netherlands.

Trying to explain this cross-country variation in rising between-firm inequality, we turned to the comparative political economy literature. We rank each country on the degree to which worker protections have weakened during the neoliberal period. Figure 4 provides some provisional estimates on the degree to which

change in employment protections is associated with rising between-workplace inequalities. The theoretical logic behind this analysis is that strong pro-employee labor market institutions reduce the labor-saving inequality consequences of outsourcing, subcontracting, and the rise of market-dominating firms. There is a clear negative relationship. Countries with weakened labor protections see greater growth in between-firm earnings inequalities. When we looked only at the private sector for available countries, we found even

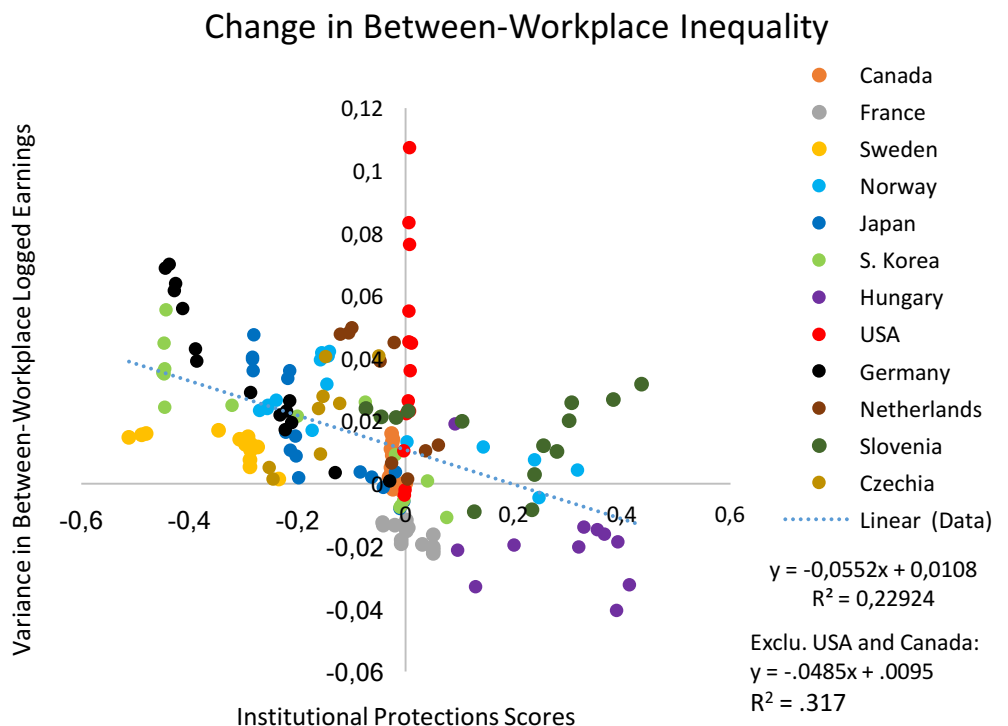


Figure 4. The relationship between changes in institutional employment protections and changes in between-workplace job earnings inequalities for all jobs

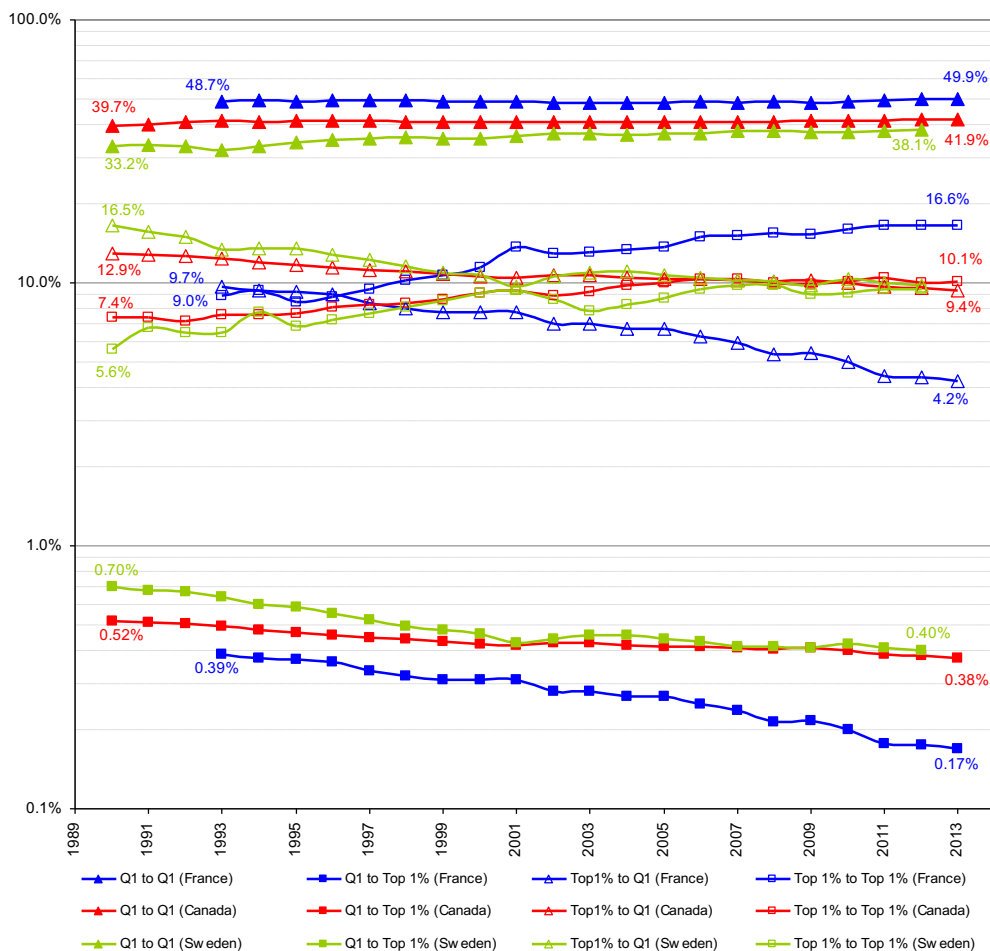


Figure 5. Workplace exposure of bottom quartile and top 1% to each other in Canada, France, and Sweden
 Note: The top 1% and the bottom quartile are wage groups defined at the national level. The workplace is the establishment, except for Canada, where information on the establishment was not available. We used instead the intersection of firms and Canadian Provinces. The larger size of size of units in Canada could attenuate the estimated level and the evolution of segregation.

stronger versions of the relationships shown in Figure 4. Reductions in institutional protections lead to higher between-workplace inequality in the private sector. Explained variance doubles.

Segregation at work

A third comparative project focusing on segregation at work has just started under the leadership of Olivier Godchot (Sciences Po). It aims to assess the magnitude and the evolution of segregation at work between diverse groups - not only the classical groups for which segregation is usually monitored, such as migrants/non-migrants, females/males, but also occupations, educational levels, age groups, and more crucially, wage groups. Are employees increasingly working in the same unit with people who are more similar to them?

Fresh results for Canada, France, and Sweden show remarkable trends for wage groups (Figure 5). The top 1% of earners in all three countries are separating

more and more from employees at the bottom of the earnings hierarchy. This evolution is particularly striking for France. In 1993, France's top 1% worked in establishments where 10% of their coworkers belonged to the lowest national wage quartile. By 2013, only 4% of their coworkers belonged to the bottom wage quartile. Top 1% exposure to the bottom quartile - and reciprocally, the latter's exposure to the former - were respectively divided by 2.4 and 2.3 (odds ratio). At the same time the top 1% isolation (exposure to itself) doubled. These trends at the workplace towards an airtight separation of the most affluent workers from the bottom of the wage hierarchy are less dramatic in Sweden and Canada, but nevertheless remain quite pronounced. The reciprocal exposures of these groups were divided by 1.8 in Sweden and 1.4 in Canada.

While one may think that underlying these separation trends are mechanisms of assortative matching of workers by levels of productivity (Kremer 1993), these preliminary results show that wage-assortative matching is extremely powerful at the very top of the wage distribution, while less pronounced or even reversed at the bottom. In fact, the three lowest quartiles of wage earners are increasingly exposed to one another in all three countries. Not all categorical status boundaries follow this trend towards more segregation that we see among wage groups. Relative isolation of migrants at work increased by a factor of 1.4 in France and 1.2 in Sweden but decreased by a factor of 1.5 in Canada. Women's relative isolation at work remains stable overall in all three countries.

The next step for this workplace segregation project is to establish trends among a larger set of countries (potentially up to 13 countries within the present COIN research group). This could help us examine whether this powerful trend towards an increasing separation of top earners at work is a general phenomenon or is limited to some countries with specific features. Sector, geo-

The international Comparative Organizational Inequality Network (COIN) asks:

- What factors drive overall income inequality within and between workplaces?
- How do workplaces exacerbate or mitigate the impact of individual distinctions, such as education level, gender, or immigrant status?
- How do inequality-generating mechanisms vary as a function of institutional context?

The network

Dustin Avent-Holt (Augusta University), Nina Bandelj (University of California, Irvine), Irene Boeckmann (University of Toronto), István Boza (Central European University), David Cort (University of Massachusetts, Amherst), Olivier Godechot (Sciences Po), Gergely Hajdu (Central European University), Martin Hällsten (Stockholm University), Joon Han (Yonsei University), Lasse Folke Henriksen (Copenhagen Business School), Andrea Hense (University of Goettingen), Are-Skeie Hermansen (University of Oslo), Feng Hou (Statistics Canada), Jiwook Jung (University of Illinois, Urbana-Champaign), Aleksandra Kanjuo-Mrčela (University of Ljubljana), Joseph King (U.S. Bureau of the Census), Naomi Kodama (Hitotsubashi University), Alena Krizkova (Institute of Sociology of the Czech Academy of Sciences), Zoltán Lippényi (Utrecht University), Silvia Maja Melzer (Bielefeld University), Eunmi Mun (University of Illinois, Urbana-Champaign), Andrew Penner (University of California, Irvine), Trond Petersen (University of California, Berkeley), Andreja Poje (Association of Free Trade Unions of Slovenia), Anthony Rainey (University of Massachusetts, Amherst), Mirna Safi (Sciences Po), Donald Tomaskovic-Devey (University of Massachusetts, Amherst), Zaibu Tufail (University of California, Irvine)

COIN website

www.umass.edu/coin

graphical location, and size of workplace decompositions of this phenomenon can help us to understand the origin of this top 1% segregation process. Financialization, clustering of lucrative activities in global cities, and outsourcing stand as the main suspects.

But the second goal of future workplace segregation analyses is to focus on the consequences of this decline in top-to-bottom interactions at work. The clustering of high profile activities in some specific establishments located in a limited set of urban areas, or in special districts of those urban areas, also impacts the probability of interaction in neighborhoods. Indeed, we also find – as shown previously for the US (Reardon and Bischoff 2011) and France (Godechot 2013) – an increasing economic residential segregation. The residential isolation (captured at the municipality level) of the top 1% increased by a factor of 1.4 in France, 1.3 in Sweden, and 1.2 in Canada. In contrast to the literature on urban segregation, which implicitly

blames the rich for deliberately avoiding the poor for schooling, status, and security purposes, we propose that the increasing residential isolation of the rich has a great deal to do with a powerful increase in workplace segregation. This helps us to bring macro socio-economic factors into our understanding of the rich's growing isolation.

A decline in objective probabilities of interaction between the rich and the poor might also have an impact on the way the two groups view each other, and finally, how they view themselves.

On the one hand, the rich, working and living in a world of wealth, do not have to think about poor workers as relevant human beings whose economic and moral claims – especially direct claims on the way the firm's value added should be shared among workers – must be taken into account. The disappearance of the poor from their interactional sphere might remove the rich from any moral constraints on wage setting, enhance status competition among them, and in return increase wage inequality at the top.

On the other hand, poor workers have less and less access to a world where they could have some interactions with the elites, whether these be of a conflict and class struggle type or of a docile and paternalistic type. Increasingly, the poor may be aware of the rich's existence only from the media, which celebrates their style and lifestyle and easily reinforces the poor's impression that they are invisible and left behind. This increase in segregation processes could therefore help us understand some recent trends in Western democracies such as the decline of political trust and the rise of populism.

The economic sociology of inequality

On the whole, the Comparative Organizational Inequality Network group is excited to use its theoretical and empirical expertise to contribute to a broadly comparative economic sociology – one that addresses central questions about how social forces shape economic inequality, as well as how economic inequality trends influence social and political outcomes. The future is not ours to predict, but the present suggests that economic sociology produces valuable tools for studying economic inequalities. We hope that the work of the COIN group will provide some paths that lead toward that future. If you are interested in what we are up to, please feel free to drop us an email!

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The Sociology and Economics of Wealth Inequality: Two Worlds Apart

by Philipp Korom

Introduction

In the 1930s at the latest, economists and sociologists parted ways. It is well documented, for example, that the Harvard sociologist Talcott Parsons, the leading theorist of the 1950s and 1960s, agreed with eminent economists of his time on an almost jurisdictional division of the social sciences: Economists should study *value* and the economy; sociologists should research *values* and the relations in which economies are embedded (Stark 2009, 7). Disciplines were thus demarcated in terms of distinctive objects of analysis. Pecuniary, market-related, business phenomena, or, more broadly, the production and distribution of wealth were to be studied by economists only. Such disciplinary lines of demarcation had a major impact: economic topics may have been the bedrock of classical sociology, but the relevance of the economy in sociology has been dwindling since the two disciplines diverged (Daoud and Kohl 2016). It is noteworthy, however, that there were always some social scientists who transcended these disciplinary boundaries, the most

obvious example being the so-called “new economic sociology” (Dobbin 2004) spearheaded by social scientists such as Mark Granovetter or Viviana Zelizer. Research on wealth¹ (inequality) is another, often neglected, example of shared interests between economists and sociologists.

Pioneering work on wealth (inequality) was conducted exclusively by economists who were granted access to state-administered data and/or conducted government research. George K. Holmes and John S. Lord (1896), for example, authored a US government report on “Farms and Homes: Proprietorship and indebtedness in the United States.” Perhaps the most influential early study on top wealth-holders in the US was conducted by Robert Lampman (1962), economic advisor to the President John F. Kennedy’s Council of Economic Advisors and intellectual architect of the war on poverty. Economists were hired to manage surveys such as the *U.S. Survey of Consumer Finances* (SCF) that have become the main data source for wealth research. Sociologists confined themselves to the role of data consumers.

From the 1980s onwards, though, some sociologists started to argue that their colleagues would do well “to give this work [wealth research] critical attention, accepting the economists’ advances and extending what is presented here by elaborating the causes and effects of past and present patterns of wealth concentration” (Hout 1982, 657). The challenge for sociology, Hout continued, was to assimilate economist’s findings and “to assemble the empirical pieces into a coherent theory of the role of capital accumulation and wealth holding in social stratification” (ibid. 658). It took a while for sociologists to come up with theories about asset-based inequality (Keister 2000; Spilerman, Lewin-Epstein and Semyonov 1993). Today, a consid-

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erable number of sociologists specialize in wealth research, some experts even administer wealth surveys, and four literature reviews on wealth research have been published in the high-impact journal *Annual Re-*

view of Sociology (Keister 2014; Keister and Moller 2000; Killewald, Pfeffer and Schachner 2017; Spilerman 2000). Clearly, wealth research is not marginalized in sociology any more.

While much of the literature looks into the future and asks how wealth research could enrich existing sociological perspectives (Savage 2014), this article looks back. By analyzing between 150 and 200 pertinent publications in each discipline, it tries to establish the dominant discipline-specific lines of inquiry in economics and sociology. On the basis of citation networks, it identifies publications that are at the very core of the research field in each discipline. Questions addressed are: Are there dominant research paradigms, methods, and data sources in each discipline? How do economists and sociologists actually approach the topic of wealth inequality?

I will conclude that the two investigated literatures do not talk to each other. If, as Piketty (2017) has suggested, nothing short of a reconciliation between economics and the other social sciences can generate new insights into what drives wealth accumulation and inequality, then today's research practices need to be turned upside down.

Building a text corpus and visualizing the sociology and the economics of wealth inequality

In any field of scholarship, writers make judgments as to who has written about what using which methods. Their judgments are reflected in their citing practices. Even if there are good reasons to argue that all citations should not be treated equally,² previous studies demonstrate that simple co-citation analysis (who cites whom?) make it possible to visualize (sub)disciplines. Network analysis tools make it easy for us to find structure since key writers show commonalities in how they judge the subject matter, methodology, and intellectual style of other writers. Consensus on eminent authors and works is not gained “by getting the people around a table to agree. It is [rather] defined behaviorally, as the citing practices of many writers, and it is gained unobtrusively, through access to the citation data of the Institute for Scientific Information (ISI)” (White and McCain 1998, 328–29).

I have used the *Web of Science* (WoS), the portal most widely used to search different ISI citation bases, to compile citation records. Critics note that WoS has the following downsides: it covers mainly English-lan-

guage journal articles, is limited to citations from journals indexed in the ISI database, does not count citations from books and other non-ISI sources, and has citing errors, such as inconsistencies in the use of initials (Yang and Meho 2007). I will demonstrate how the first three of these deficits can be remedied.

I began by searching for key publications on wealth inequality in both disciplines. In order to select a nearly equal number of articles in each discipline, I used slightly different search commands for each discipline in WoS (I conducted both searches at the end of May 2017):

For economics:

title: wealth AND topic: inequality
refined by
document types: article AND WoS
categories: economics
timespan: 1990-2017
indexes: SSCI

Result: 251 articles

For sociology:

topic: wealth AND inequality
refined by
document types: article AND WoS
categories: sociology
timespan: 1990-2017
indexes: SSCI

Result: 255 articles

Why did I use distinct search strategies for each discipline? By entering “topic terms,” I could search for “wealth” and “inequality” in the title only but also in the abstract. For sociology, this research strategy enabled me to identify articles that discuss the unequal distribution of assets but contain, for example, only keywords such as “inheritance” in the title. For economics, such a search procedure turned out to be too coarse-grained, since the word “wealth” is part of the standard vocabulary of economics. Searching for “wealth” in the title helped to achieve better search results.

The *biographical data* of the identified articles were imported to *CitNetExplorer* (van Eck and Waltman 2014), a software for analyzing *direct citation networks*. The CiteNetExplorer option “include non-matching reference” makes it possible to include major books or book chapters that are not in the WoS database but appear in the references of identified articles. Another limitation of the WoS is, however, impossible to overcome. The most recent publications are not considered at all since they have probably not been cited yet by other authors in the field.

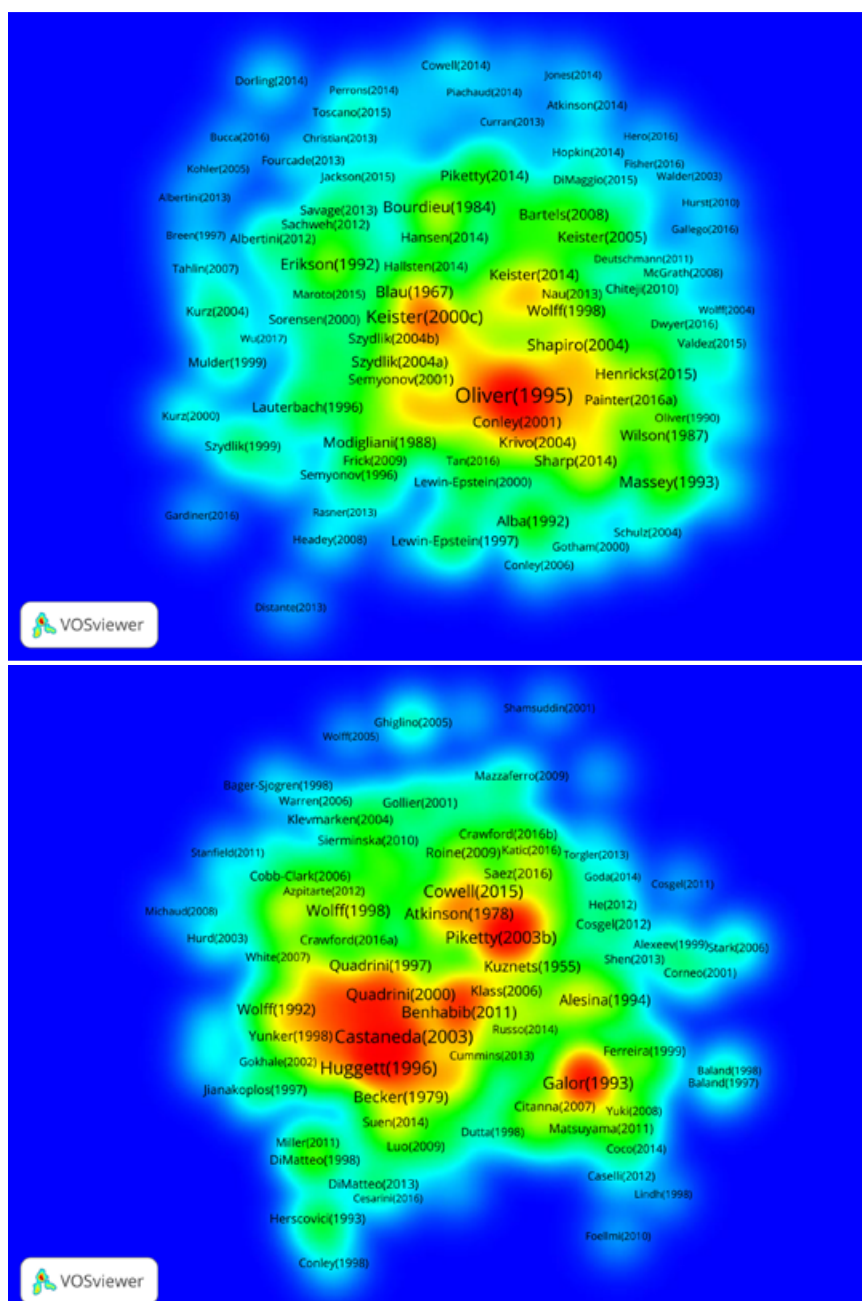


Figure 1. Density visualization for sociology (upper panel) and economics (lower panel)

Citation network data were also exported to *VOSviewer* (van Eck and Waltman 2010), a software tool that identifies the most cited works and their related publications using a distance-based approach (see below).

To eliminate publications that are only poorly representative of wealth research, I decided to select only works that belong to the main component of each discipline-specific network.³ I thus deleted all “isolates” from the network graph.

Figure 1 shows the discipline-specific networks. The network in sociology is based on 149 vertices (items) and 597 arcs; in economics, on 220 vertices (items) and 925 arcs. The labels of all vertices contain only the first author and the date of publication. The

label ‘Oliver(1995)’ stands, for example, for the monograph *Black Wealth, White Wealth* published by Melvin L. Oliver and Thomas M. Shapiro in 1995. All the labels, which are essentially abbreviations, are listed in Tables A1 and A2 in the Online Appendix, which can be downloaded from the EconSoc website.⁴

In the density view of the two networks displayed in Figure 1, the color of a point is determined based on the item density, which in turn depends on two factors: the number of neighboring items and the weights of these items. The larger the number of neighboring items and the smaller the distances between these items and the point of interest, the higher the item density (van Eck and Waltman 2010, 533). In the chosen color scheme, red corresponds with the highest item density and blue with the lowest item density.

If one compares the upper with the lower panel in Figure 1, it becomes apparent that the sociology of wealth inequality centers around a single density core while the economics of wealth inequality is fragmented.

In sociology, the core consists of often-cited works on the racial wealth gap (‘Conley 2001’, ‘Krivo 2004’, ‘Oliver 1995’, ‘Shapiro 2004’) and more general studies on wealth inequality (‘Keister 2000c’), inheritance (‘Semyonov 2001’, ‘Szydlak 2004a’) and economic elites (‘Nau 2013’, ‘Keister 2014’). It is impor-

tant to note that three publications by economists (‘Modigliani 1988’, ‘Wolff 1998’, ‘Piketty 2014’) have high density scores. Somewhat paradoxically, a landmark study by Blau and Duncan (1967), which posits that the structure of occupations (and not household wealth) is the main foundation of social stratification turns out to be the main historical reference in the field. By examining which publications garner most citations (“indegrees”) within the network (see Table 1), we can easily see which books and articles on the racial wealth divide are at the very core of wealth inequality research in sociology.

In economics, literature testing different economic models to examine the implications of unequal wealth distributions (‘Benhabib 2001’, ‘Castaneda

Table 1. Indegree centrality in the field of sociology**Indegree Work**

34	Oliver, Melvin L., and Thomas M. Shapiro. 1995. <i>Black Wealth, White Wealth: A New Perspective on Racial Inequality</i> . New York, NY: Routledge.
26	Conley, Dalton. 1999. <i>Being Black, Living in the Red: Race, Wealth, and Social Policy in America</i> . Berkeley; Los Angeles: University of California Press.
26	Keister, Lisa A., and Stephanie Moller. 2000. "Wealth Inequality in the United States." <i>Annual Review of Sociology</i> 26 (1): 63–81.
21	Keister, Lisa A. 2000. <i>Wealth in America: Trends in Wealth Inequality</i> . Cambridge; New York: Cambridge University Press.
21	Spilerman, Seymour. 2000. "Wealth and Stratification Processes." <i>Annual Review of Sociology</i> 26: 497–524.
17	Bourdieu, Pierre. 1984. <i>Distinction: A Social Critique of the Judgement of Taste</i> . Cambridge, Mass.: Harvard University Press.
15	Blau, Peter M., and Otis D. Duncan. 1967. <i>The American Occupational Structure</i> . New York: John Wiley & Sons.
14	Erikson, Robert, and John H. Goldthorpe. 1992. <i>The Constant Flux: A Study of Class Mobility in Industrial Societies</i> . Oxford: Oxford University Press.
14	Massey, Douglas S., and Nancy A. Denton. 1993. <i>American Apartheid: Segregation and the Making of the Underclass</i> . Cambridge, Mass.: Harvard University Press.
14	Shapiro, Thomas M. 2004. <i>The Hidden Cost of Being African American: How Wealth Perpetuates Inequality</i> . New York: Oxford University Press.
12	Keister, Lisa A. 2000. "Race and Wealth Inequality: The Impact of Racial Differences in Asset Ownership on the Distribution of Household Wealth." <i>Social Science Research</i> 29 (4): 477–502.
11	Wilson, William Julius. 1987. <i>The Truly Disadvantaged</i> . Chicago: Chicago University Press.
11	Alba, Richard D., and John R. Logan. 1992. "Assimilation and Stratification in the Homeownership Patterns of Racial and Ethnic Groups." <i>International Migration Review</i> 26 (4): 1314–41.
11	Wolff, Edward N. 1998. "Recent Trends in the Size Distribution of Household Wealth." <i>Journal of Economic Perspectives</i> 12 (3): 131–50.
11	Krivo, Lauren J., and Robert L. Kaufman. 2004. "Housing and Wealth Inequality: Racial-Ethnic Differences in Home Equity in the United States." <i>Demography</i> 41 (3): 585–605.
11	Keister, Lisa A. 2005. <i>Getting Rich: America's New Rich and How They Got That Way</i> . Cambridge; New York: Cambridge University Press.
11	Piketty, Thomas. 2014. <i>Capital in the Twenty-First Century</i> . Cambridge, Mass.: The Belknap Press of Harvard University Press.

Note: In a citation network, the indegree of a node is simply the number of citations that it has received from other nodes.

2003', 'Huggett 1996', 'Quadrini 1997') constitutes a separate density center. In another strand of literature discussing the interrelation between inequality and aggregate economic activity, we can observe similar high-density scores ('Galor 1993'). A third isolated

center of density emerges around publications by Anthony Atkinson and Thomas Piketty on the historical distribution of income and wealth. The intellectual predecessor most cited in this field of research is the economic historian Simon Kuznets, who was the first

Table 2. Indegree centrality in the field of economics**Indegree Work**

24	Piketty, Thomas. 2003. "Income Inequality in France, 1901–1998." <i>Journal of Political Economy</i> 111 (5): 1004–42.
23	Galor, Oded, and Joseph Zeira. 1993. "Income Distribution and Macroeconomics." <i>The Review of Economic Studies</i> 60 (1): 35.
22	Huggett, Mark. 1996. "Wealth Distribution in Life-Cycle Economies." <i>Journal of Monetary Economics</i> 38 (3): 469–94.
22	Castañeda, Ana, Javier Díaz-Giménez, and José-Víctor Ríos-Rull. 2003. "Accounting for the U.S. Earnings and Wealth Inequality." <i>Journal of Political Economy</i> 111 (4): 818–57.
20	Aghion, Philippe, and Patrick Bolton. 1997. "A Theory of Trickle-Down Growth and Development." <i>The Review of Economic Studies</i> 64 (2): 151–72.
20	Piketty, Thomas, and Gabriel Zucman. 2014. "Capital Is Back: Wealth-Income Ratios in Rich Countries 1700–2010." <i>The Quarterly Journal of Economics</i> 129 (3): 1255–1310.
19	Atkinson, A. B., and A. J. Harrison. 1978. <i>Distribution of Personal Wealth in Britain</i> . Cambridge ; New York: Cambridge University Press.
19	Banerjee, Abhijit V., and Andrew F. Newman. 1993. "Occupational Choice and the Process of Development." <i>The Journal of Political Economy</i> 101 (2): 274–98.
18	Aiyagari, S. Rao. 1994. "Uninsured Idiosyncratic Risk and Aggregate Saving." <i>The Quarterly Journal of Economics</i> 109 (3): 659–84.
16	Krusell, Per, and Anthony A. Smith, Jr. 1998. "Income and Wealth Heterogeneity in the Macroeconomy." <i>Journal of Political Economy</i> 106 (5): 867–96.
16	Quadrini, Vincenzo. 2000. "Entrepreneurship, Saving, and Social Mobility." <i>Review of Economic Dynamics</i> 3 (1): 1–40.
16	Piketty, Thomas. 1997. "The Dynamics of the Wealth Distribution and the Interest Rate with Credit Rationing." <i>The Review of Economic Studies</i> 64 (2): 173–89.
16	Kopczuk, Wojciech, and Emmanuel Saez. 2004. "Top Wealth Shares in the United States, 1916–2000: Evidence from Estate Tax Returns." <i>National Tax Journal</i> 2 (2): 445–87.
15	Becker, Gary S., and Nigel Tomes. 1979. "An Equilibrium Theory of the Distribution of Income and Intergenerational Mobility." <i>Journal of Political Economy</i> 87 (6): 1153–89.
15	Kotlikoff, Laurence J., and Lawrence H. Summers. 1981. "The Role of Intergenerational Transfers in Aggregate Capital Accumulation." <i>Journal of Political Economy</i> 89 (4): 706–32.
15	Modigliani, Franco. 1988. "The Role of Intergenerational Transfers and Life Cycle Saving in the Accumulation of Wealth." <i>Journal of Economic Perspectives</i> 2 (2): 15–40.
15	Wolff, Edward N. 1998. "Recent Trends in the Size Distribution of Household Wealth." <i>Journal of Economic Perspectives</i> 12 (3): 131–50.

Note: In a citation network, the indegree of a node is simply the number of citations that it has received from other nodes.

to embark on the task of collecting historical data on the distribution of income and wealth. There is not a single non-economist in the network.

The diversity of wealth research in economics also becomes apparent in the list of the most cited works (see Table 2). The specializations of the most cited authors range from macroeconomics (J. Zeira) and inequality research (T. Piketty, T. Atkinsons) to development economics (A. Banerjee) and growth theory (P. Aghion).

How do sociologists and economists approach the topic of wealth inequality?

Theoretically the topic of wealth inequality can be approached in multiple ways. Content analysis, however, reveals that in each discipline a few topics dominate the research agenda in the discipline-specific field. What is even more striking is the insight that each discipline tends to follow a single methodological approach.

In sociology, about three quarters of all the works analyzed are empirical investigations based overwhelmingly on survey data. Most of these studies use regression models. In economics, a wide range of mathematical models is used to show how specific mechanisms work by isolating them from each other. Interestingly, only about 40% of all the articles applying economic models draw on empirical evidence to, for example, cal-

ibrate the model's parameter or improve the mapping between these models and the real world (see Table 3).

Regarding the dominant topics, racial and ethnic wealth disparities are by far the most frequently documented topics in sociology. Without exception, the works analyzed observe that the black–white wealth gap in the USA or the ethnic disparities in Israel or Germany are substantial. There is little to suggest that these gaps have been shrinking over time. Two other topics focused on by sociological scholarship are (intergenerational) wealth transfers due to cumulative advantages accrued via inheritance and housing wealth due to the importance of the benefits (and costs) associated with homeownership. Other frequent topics are wealth and social class, the intergenerational transmission of wealth, and the rich.

In general, there is little overlap in topics between sociology and economics. Taken at face value, economists also analyze the importance of intergenerational links, inheritance, and societal elites. But if we dig deeper into the text material, we discover distinct disciplinary perspectives even on these few common themes. Economists tend to model intergenerational links, analyze the top holders of wealth or test power laws (“Pareto’s principle,” “Zipf’s law”) on the basis of “rich lists,” while sociologists measure the importance of family background and adopt a broader concept of elites.

The interrelations between growth and the distribution of wealth are analyzed solely by economists. Typical research questions are: Does wealth inequality matter for growth? Is the wealth accumulation by the rich good for the poor? Does the unequal distribution of economic resources offer a (partial) explanation for cross-country differences in economic output? Taxes and (redistributive) policies are also commonly discussed in the literature. The possible redistributive effects of different tax-subsidy schemes are tested, and governmental transfer policies are incorporated in stylized models. Finally, about 11% of all articles either analyze historical trends over time or existing levels of wealth inequality in past societies.

Interestingly, the proportion of empirical analyses is higher in sociology (78%) than in economics (64%). Nearly half of all sociological articles report insights that are US-centric and cannot be generalized. In contrast, the economic literature has a more cross-national and global scope.

Table 3. Approaches to wealth inequality in sociology and economics

	Sociology		Economics	
Topics				
Racial and ethnic wealth disparities	35	(23.6%)	17	(7.7%)
Wealth transfers/inheritance	25	(16.9%)	50	(22.6%)
Homeownership/housing	22	(14.9%)	9	(4.1%)
Social class	18	(12.2%)		
Intergenerational links/parental background	14	(9.5%)	20	(9%)
Elite/rich/1%	10	(6.8%)	23	(10.4%)
Growth and distribution of wealth			36	(16.3%)
Taxes			34	(15.4%)
Historical analyses			25	(11.3%)
Policies			22	(10.0%)
Entrepreneurship			12	(5.4%)
Pareto distribution			11	(5.0%)
Data				
Number of empirical studies	116	(78.4%)	141	(63.8%)
Empirical studies based on survey data	93	(80.2%)	89	(40.3%)
Methodological approach				
Empirical analyses based on regression framework	74	(63.8%)		
Economic models			92	(41.6%)
Economic models and empirical analyses			26	(11.8%)
Country analyzed				
USA-specific	71	(48.0%)	77	(34.8%)
Total number of publications analyzed	148		221	

Conclusion

Wealth inequality is one of the societal megatrends that has made significant inroads into many social science disciplines, including economics and sociology. If we were to characterize the current status of wealth inequality research (and provide a critique of it), we would have to say that it is fragmented across main social science disciplines, and that there is little evidence of integrative or collaborative efforts.

This article has aimed to identify publications about wealth inequality in each discipline on the basis of co-citation analysis, and it has sought to determine common approaches and topics. It has become apparent that what applies to poverty research holds true for research on wealth inequality as well: “If we ask academics why poor people are poor ... different disciplines will answer ... in their own unique ways: each with certain kinds of data, certain methods, certain habits of thinking about the problem” (Abbott 2001, 142). In sociology, there is a strong and lasting tradition of investigating racial and ethnic wealth disparities, and nearly all insights gained are derived from survey data. In economics, the consequences and the causes of wealth inequality are key issues, and redistributive policies are commonly discussed. The economic literature is mostly technical and full of model-based theories. It is perhaps not exaggerated to assert that economic models are incomprehensible to the non-economist, especially because of the complex mathematics involved and the heavy jargon (“balanced growth path,” “agents maximize their utilities,” “overlapping generations model with intragenerational heterogeneity”).

Wealth research in sociology is more monothematic. Its focus is on disparities between different social groups (e.g. immigrants vs. natives, black vs. white wealth). In economics, research on wealth inequality is

done by authors working in very different fields of specialization. While sociologists refer to the work of economists, economists ignore other disciplines’ work, which suggests that they think of themselves as dominating the (largely invisible) pecking order among wealth researchers (Fourcade, Ollion and Algan 2015).

Each discipline seems to have basic working assumptions that constitute a consensus around which a dominating research paradigm develops. In sociology, it is possible to make an academic career out of specializing in survey research and applying regression frameworks to investigate wealth disparities between different social groups. In economics, research judged to be at the frontiers of knowledge merely models policy influences on the distribution of wealth without any subsequent empirical analysis. Such specializations are clearly purchased at the cost of an excessive narrowing of focus.

Where do we go from here? There are many professional incentives to stay within disciplinary confines. Sociologists and economists surround themselves with like-minded, similarly trained colleagues and depend heavily on peer review that is almost exclusively from within their own discipline and field of interest. If we are to believe that disciplines should overcome their differences in order to advance knowledge on wealth inequality (Piketty 2014), then the existing incentive structures have to be changed. Journal editors, directors of research centers, and department heads could reward sociologists whose work does not eliminate context from history by means of cross-sectional survey data but instead explores historical trends in wealth inequality. And peer reviewers could ask economists to evaluate each and every model on the basis of empirical evidence. Since new thinking of this kind is unlikely, we are well advised to take the best from both worlds.

Endnotes

- 1 Wealth consists of assets that can be “owned and exchanged on some market” (Piketty 2014, 46), and thus includes, among other things, property, financial assets, and professional capital (i.e. patents), but not human capital, which cannot be traded.
- 2 Citations may have different functions. “Ceremonial” citations only loosely relate to other’s work and should therefore be differentiated from “substantive” citations. Ceremonial citations have a “perfunctory” function: the elevated author of the cited source is used to boost the authority of one’s work (substantive citations indicate one’s intellectual precursors) (see Bornmann and Daniel 2008).

- 3 The main component retains all the nodes and relations among nodes that are part of the largest component of a graph. Components are defined as sets of points that are linked to one another through continuous chains of connection.
- 4 http://econsoc.mpifg.de/archive/supplements/econsoc_19-1_Korom_appendix.pdf

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Avoiding and Protesting Taxes: Wealthy People and Tax Consent

by **Camille Herlin-Giret**

Class conflict is, first and foremost, a struggle over the appropriation of work, production, property, and taxes.

James C. Scott (1987, 450)

Many authors have pointed out the significant role of tax systems in containing wealth inequalities. According to Thomas Piketty (2013), the two world wars partially undermined old fortunes. But he argues that the decrease in inequalities he observed after the Second World War was also a result of setting up highly progressive tax systems in several countries. Indeed, it is during and after the two world wars that many governments adopted the highest income tax rates of the twentieth century (Scheve and Stasavage 2016). Progressive wealth taxation can thus be considered “the greatest threat to the fortune” of wealthy families (Beckert et al. 2015, 22). To explain the endurance of family wealth, these authors argue, three dimensions need to be considered: wealth managers’ activities, tax avoidance, and long-term control over family companies (ibid.). Here, I will examine a small part of this broad research program by focusing on the issue of consent to the wealth tax in France.

Since 1981, the French tax system has included a progressive wealth tax, although it was eliminated in 1986 and reintroduced in 1988. Today, assets worth more than 1.3 million euros that are not considered professional or artistic assets are subject to the wealth tax. Even though tax dissent is a hotly debated topic that is often brought up in connection with

wealthy people, little is known about it except for what we find in a few book-length studies. For example, Isaac Martin (2013) recounts the history of five mobilizations against taxation of the top one percent in the United States. Interestingly, the wealthy were not the only ones who became involved in these movements, and one of Martin’s aims is to understand exactly why the middle class also protests against taxes on high incomes or wealth. Kenneth Scheve and David Stasavage (2016) explain changes in top income and inheritance tax rates as resulting from changes in political support for taxing the rich. Brooke Harrington (2016) focuses on wealth-management activities and examines tax dodging through the work of specialists on behalf of their clients. As can be seen from this brief review, the few books dealing with wealthy people and tax consent are not directly focused on the latter. Instead, they are focused on support for tax dissent among the rich, tax avoidance, or tax evasion. More broadly, the significant literature on tax consent often seems to focus indirectly on “citizens’” tax consent. Trust in the state, fear of a tax reassessment (Levi and Braitwhaite 1998), the government’s ability to raise taxes (Lieberman 2009), and tax rates (Daunton 2007) are treated as codes for discussing tax consent among “citizens” or tax-avoidance strategies.

A starting point of this paper, then, is to put wealthy people at the heart of the issue in order to examine their attitudes towards taxation more directly. To get a better idea of what is meant by “trust” or “distrust” of the state, I focus on wealthy people’s ordinary tax-dodging practices rather than questioning their claims about tax fairness. This approach makes it possible to investigate other forms of resistance to taxation than those studied when looking at technical setups constructed by wealth managers. It also enables me to

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discuss Hirschman’s well-known trilogy – exit, voice, and loyalty (1970) – regarding tax consent. In regard to France’s wealth tax, what forms do tax dissent and consent take? After discussing the significance of tax exile,

I go on to show that wealthy people are actually more likely to use invisible and petty forms of resistance to taxation than conspicuous ones: it is less stigmatizing to undervalue assets a little bit when filling a wealth tax form than to leave a country for good, and it is also less of a commitment to place money offshore than to leave oneself. By exploring these phenomena, the article presents an in-depth study of how wealthy people and wealth managers handle the boundary between compliance and noncompliance. To the extent that wealth managers are included in the analysis, it is to point out how they legitimate tax dodging among their clients. I will show that tax dodging may be accompanied by calls for, and even claims of, tax civism.

This paper is based on interviews I conducted with wealth tax payers (29) and wealth managers (37) and on an analysis of the archives of the French monthly magazine *Gestion de fortune* (which could be translated as “Wealth Management”), published between 1991 (the first issue) and 2014. First, I will argue that tax exile, which has been constructed as a major way to avoid taxation in France, can be considered a political construction that prevents tax increases for the rich. Second, I will highlight a more ordinary form of resistance to taxation – the undervaluation of assets – showing that “some protests occur without the use of voice” (Agrikoliansky and Collovald 2014, 10) (all translations from the French are by the author). Finally, I will analyze how wealth managers contribute to the legitimation of tax evasion, thus blurring the boundary between legal and illegal tax dodging.

The persuasive fiction of tax exile

Tax exile among the wealthy is one of the most media-friendly forms of resistance to taxation. The expression “tax exile” is not neutral. It suggests that people have left the country under strong political pressure, and that this pressure is mostly a result of the tax burden. Many newspaper articles and parliamentary reports about tax exile point to the high number of executives, managers, top earners, and wealthy people leaving France for countries with lower tax rates. However, we never know the motive behind these departures. Journalists and politicians usually assume that fiscal rules have pushed the wealthy out, but nothing indicates that this is true. For example, even though the income tax rate is not lower in the United Kingdom or Belgium than in France, executives’ departures are often explained as resulting from the fiscal burden of remaining in France.

Economic theories have contributed significantly to the idea that top earners’ departures result from the tax rate. According to the well-known Laffer curve, when tax rates reach a certain – mysterious – level, government revenue decreases because taxpayers change their behavior, “voting with their feet” (Tiebout 1956) in their attempt to avoid taxes. Arthur Laffer used the social movement to limit property taxation in California in 1978 (“Proposition 13”) to argue that his curve was not merely an abstraction. Even though the Laffer curve is not empirically based and has been contested, it has a strong influence on economists, politicians, and journalists. In particular, it has popularized the idea that taxing the rich is economically dangerous or counterproductive because it increases the risk of tax exile (Trannoy 2010; Sterdyniak 2015). Even the small amount of empirically based economic research on the matter, which is quite critical of abstract models, does not question the implicit hypothesis of fiscally motivated departures.¹ Let us turn now to the often-made connection between wealthy people’s departures and the tax rate.

First of all, executives, managers, and top earners are increasingly used to having international careers and moving from one country to another. While the expression “tax exile” suggests that the departure is fiscally forced, sociological research focusing on the circulation of elites reveals that departures instead result first and foremost from social pressure. Indeed, mobility has become a social norm among the upper classes. Anne-Catherine Wagner (1998) has demonstrated that stays abroad are seen as nearly obligatory stages on the road to social success: “Living abroad decreases the number of work competitors, increases the range of choices, and makes it possible to obtain a higher status than in one’s native country” (145). In other words, wealthy people who leave France are surely also motivated by reasons other than taxation, and it seems hard to believe that they are simply reacting to an increase in tax rates. In the French monthly magazine *Gestion de fortune*, aimed at a professional readership, a tax lawyer explains that “most relocations have to do with executives’ professional mobility.” And he adds: “While the wealth tax may appear to be a crucial factor in many clients’ decision about whether to leave the country, relocations motivated purely by fiscal considerations in fact represent a negligible share of all departures” (Issue no. 202, 2010). In many cases, other factors – friends, family, job, and so on – are such important anchors that the tax burden can hardly ever be a sufficient reason to leave the country. A wealth manager working in a private bank told us a story about one of his clients. After selling his pharmaceutical laboratory

for forty million euros, this man decided to leave France for Marrakech for tax reasons, despite the doubts of his financial advisor.

Gil – Six months later, his wife calls me and says, “Gil, Hughes would like to see you.” I asked, “What’s wrong with him?” She says, “Well, he’s in a psychiatric hospital right now.” After three months, he had a huge nervous breakdown. And then [laughing], he came back to France. He didn’t know how to deal with it all. And he told me, “Gil, it’s awful! You’re always with the same people, who play the same golf, who repeat the same stories. They all miss Parisian cafés, and they all want to come back. And they’re going crazy, with nothing but the palm grove and the staff, thirty people walking around in circles in their luxury residence.”²

Moreover, some of the wealthiest individuals already have an international lifestyle, owning properties in several countries in which they stay a few months a year. An interviewee told me about some of his “very rich” friends who “have large businesses in real estate and banking”:

Charles – One day, I asked a couple of friends living in Switzerland, who are very rich, “Don’t you mind living there?” And they said, “Well, you know ...” – they’re living between New York, Paris, Switzerland, and Saint Barths. They have a very pleasant life. He answered right away, he said, “Skiing four months a year makes me save five million euros a year.”

Indeed, for some wealthy people who already live in several countries, mobility has become a way of life. What is commonly called exile, which makes the departure seem like a forced and irreversible decision, can actually result from a value-neutral decision about how much time is actually spent in the French apartment. For example, taxation may be a relevant factor in the specific case of the migration of highly mobile professional football (soccer) players (Jacobsen Kleven, Landais, and Saez 2013). But referring to this mobility as “tax exile” seems rather inappropriate.

Tax exile has all the features of what Joseph Gusfield (1980) calls a persuasive fiction, a belief that seems to be true, to which some scientific qualities are attributed, and that fosters an emotional interpretation of the facts. It is important to understand how tax exile has become a public problem, a persuasive fiction often used in the political arena.

Many political speeches and parliamentary reports in France present tax relocations as a crucial political issue. The expression “tax exile” makes politicians responsible for wealthy people’s departures and consequently erases the latter’s responsibility and their

opportunistic depiction of their departure. Insofar as tax rates have already become a permanent and salient political issue (Martin 2008), tax exile has been constructed as a political threat, one that is raised during elections in particular to justify decreases in higher marginal tax rates. With the support of several graphs and charts, Philippe Marini (a right-wing French senator), in a report for the Senate, pointed out the rise of relocations among wealth tax payers and attributed it to the tax rate without any discussion: “These numbers, coming from official statistics released by the French Ministry for the Economy and Finance, provide a new perspective on tax relocations. The evidence is so striking that it doesn’t require much comment” (p. 60). Because the motives for relocation among those who are required to pay the wealth tax can hardly be differentiated, there is no real data about tax exile. This fuels the idea that tax exile is a widespread phenomenon that the government is not willing to measure.

I am not arguing that tax exile does not exist. However, this persuasive fiction, presented as fact and constructed as a major political issue, relies on questionable assumptions. It creates a political framework through which to understand emigration among the wealthy, which in reality is largely motivated by factors other than taxation. As Thomas Piketty and Emmanuel Saez argue (2011, 163), “the likelihood of top earners emigrating is often overestimated.” Relocations, commonly interpreted as tax exile, inform us less about resistance to taxation than about the political construction of the wealth taxation, a construction that protects the wealthy from tax increases. Not all the departures of wealthy people from France are political. At the same time, we should not assume that the wealthy consent to taxation simply because they fill out their tax returns and pay their taxes.

Ordinary tax resistance

In the past, mobilizations against taxes regularly erupted into riots. Charles Tilly (1986) even considers tax rebellions to have been characteristic of the peasants’ repertoire of contention in the seventeenth century. The decrease in tax riots does not mean that mobilizations against taxes have disappeared: “Their form and purpose have changed: they now result from new social classes’ dissatisfaction with the state” (Hmed 2011, 236). The mobilization to limit property taxation in California in 1978, studied by Martin (2008), is typical of these tax revolts, mostly involving members of the upper middle class who are concerned with potential tax increases. There have been no such movements

against the wealth tax in France. However, the striking aspect of tax mobilizations over time should not cause us to overlook other forms of resistance to taxation. Indeed, James C. Scott (1987) has highlighted everyday “petty stratagems” of resistance to taxation, comparing peasants’ opposition to the Islamic *Zakat* in Malaysia in the 1980s and eighteenth-century Christian tithe in France. Scott points out that, instead of open resistance, peasants engage(d) in the “patient labor of nibbling” (447) to reduce tax amounts. Despite the geographical, social, and historical gap between his subject and ours, there are similarities in the modes of contention. While there is no public movement against the French wealth tax, resistance to taxation, as will be demonstrated, is partly defined by petty practices, especially when taxpayers fill out their tax returns and calculate the tax amount they have to pay.

During the interviews with wealth tax payers, I always encouraged them to talk about their ordinary tax practices (which spouse fills out the form, how long it takes to do so, whether they seek advice or assistance from a tax specialist, how they evaluate their assets, and so on). What was noticeable was that most interviewees have an inclination to undervalue their assets when they fill out the form, sometimes significantly. Valuing an asset accurately is not that simple, especially when the asset is priceless or has belonged to the family for a long time. The measure of potential value “involves more than mercantile calculations” (Zelizer 2005, 53). The interviewees related their difficulties in estimating their wealth. But they also mentioned that they deliberately undervalued their assets in their tax returns. For example, one of the interviewees, a 76-year-old man who partly inherited his wealth and also got money from the sale of a prosperous firm he created, explained: “Today, the amount I fill on my wealth tax return is about 5 million euros. But inevitably the amount I report is always. ... Well, I never inflate it. So, if I redid the calculations in some other way or a bit differently, I would maybe have 6 or 6.5 million euros.” Another interviewee, an inheritor who manages the family company, tried to give me an overview of his wealth by telling me that the amounts he writes on his tax return are “almost true.” Another, a retired civil servant who has worked in public education and owns more than one million euros, mostly in real estate, explained to me: “We know very exactly what the price per square meter is, because we pay attention to real estate transactions in the building and nearby. But I declare less.”

In the literature, authors offer two main explanations for people undervaluing assets or making “mistakes” when filing out their tax returns. One of the

most common arguments is the “deterrence theory.”⁴ According to this utilitarian theory, when taxpayers undervalue assets, they weigh the advantages and disadvantages of noncompliance in terms of the probability of detection and the severity of punishment. Kent Smith and Karyl Kinsey (1987) offer another, less common, explanation: Asset undervaluation, and taxpaying more generally, do not involve a deliberate decision; noncompliance is unconscious and mostly shaped by habit and inertia. I would propose a third explanation: Asset undervaluation relies on a relativistic relationship to rules and compliance among wealthy people – undervaluing assets is indeed collectively endorsed and legitimated by wealth tax payers.

First of all, undervaluation is fully accepted, even sometimes openly admitted. As a result, it cannot be considered a simple mistake. Undervaluation seems to involve a deliberate decision. The people interviewed usually alluded to two different asset values to describe their assets, comparing for instance a market value, based on a possible resale, and a historic one, based on the purchase price. In other words, they are willing to recognize that the value they declare is not the one they could get if they decided to sell their assets. One of the interviewees, a shareholder in a flourishing family company whose wealth is about 20 million euros, said that he owns a second home in the South of France “that is worth three times more than the value I declare in my wealth tax return” (three million euros versus one million euros in the tax return). He explained that he undervalues all of his properties. He said that his apartment, 240 square meters in one of the wealthiest neighborhoods in Paris, is “maybe worth 2 million euros, maybe more, I’m not sure,” but he declares it at 1.4 million euros. While undervaluation is partly based on the vagueness surrounding wealth and its valuation, it can also be seen as a petty stratagem to avoid taxes. Wealth tax payers play with the different values they have in mind – historic, market, affective – while justifying why they have chosen the lowest. They deliberately undervalue their wealth, even though they probably do not know by exactly how much. And yet, on the whole, the amount of assets that elude the wealth tax can be significant.

Interviewees were usually unwilling to mention tax-evasion practices. For example, even though I knew that some of them had undeclared money stashed offshore, none of them ever told me about it. But interviewees mentioned undervaluation without embarrassment. We can therefore conclude that wealth tax payers do not consider undervaluation to be a fraudulent or deviant practice. No stigma seems to be associated with it because undervaluation is collectively en-

dorsed and admitted. Far from being based on individual taxpayers' weighing of the advantages and disadvantages of noncompliance, this practice is embedded in collective attitudes.

First, assets are often shared within the family and collectively owned. Many interviewees mentioned trying to coordinate with other members of their family in order to declare the same value for assets in their returns. Asset valuation is not a solitary decision and usually involves family talks. One of the interviewees said he organizes family reunions to "agree on something consistent for the tax authorities" and mentioned some conflicts with his cousins about the valuation of a property.

Second, wealth tax payers may also coordinate with their neighbors living in the same building or neighborhood to report similar values on their tax returns. Indeed, the more wealth tax payers there are in a given, concentrated area, the more they can undervalue their properties without the risk of being stigmatized. Wealth concentration is especially significant in Paris and its surrounding areas.

Third, undervaluation relies on a relativistic relationship to rules and compliance among the wealthy. A 70-year-old interviewee, the son of a "great banker" who has worked in publishing and whose net worth is about 15 million euros, explains: "I cheat, like everybody else. I undervalue. Well, people are always surprised by tax evasion, but ... Do I feel like I'm cheating? No. Because I know I will be caught by the cops." This wealth tax payer is not avoiding taxes because he thinks it will spare him an audit, but rather because he anticipates one. Wealth tax payers do not consider undervaluation a deviant practice, but instead a reasonable one. An interviewee, condemning "people who try by any means to avoid the wealth tax," said: "Well, I'm not saying I value my assets at the highest. We fill out our tax return... cleverly, as people say, meaning we declare rock-bottom values, like everybody else." Undervaluation is not seen as a fraudulent way to pay lower taxes, but as an insignificant, ordinary practice, without any stigma attached to it. According to Luc Boltanski (2009), this kind of attitude toward rules is specific to the dominant class, and it is characterized by cynicism: dominants will talk about the importance of compliance and rules, on one hand, but they often manipulate and get around rules to their own advantage, on the other. This hypothesis helps to explain why undervaluation is not thought of as resistance to wealth taxation and may even go hand in hand with calls for tax civism.

In their work on white-collar crime and deviance, respectively, Edwin Sutherland (1983) and Howard Becker (1963) found that the government and

many workers contribute to the labeling of certain behaviors as "deviant" or as "nondeviant". Indeed, tax lawyers, wealth managers, and financial advisors play a key role in turning resistance to taxation into an ordinary practice not associated with stigma.

Vagueness in the service of clients: Wealth managers and the legitimization of tax evasion

These professionals contribute to legitimizing tax evasion in various ways. For example, we have found many documents in which wealth managers support asset undervaluation. In an article titled "Wealth tax: How to reduce the bill?" published in *Gestion de fortune* (No. 28, 1994), after a list of various tax setups, the following appears: "Of course, it can be reasonable to undervalue your wealth." Another article warns wealthy tax payers about assets' possible drop in value and advises them not to declare values that are too high, arguing that "tax-investigation procedures are so burdensome [for the authorities] that they aren't worth the trouble. Wealth tax investigation ... isn't profitable for small sums of money" (*Gestion de fortune*, No. 55, 1996). As can be seen, financial advisors play an "active role in manipulating the boundary between legitimate and illegitimate practices" (Spire 2011, 60). I will demonstrate how financial advisors, in helping wealthy people to manage their money, tend to blur the boundary between compliance and noncompliance and sometimes turn their tax-evasion practices into reasonable legal practices.

This transformation is apparent in their interviews. Wealth managers did not tire of reiterating that their work does not focus on tax dodging. Although they described spending a great deal of time working on tax issues, they emphasized that taxes are not their primary concern. Their work on tax law and tax setups appears to be simply a matter of method, just one avenue toward maintaining wealth – not the main strategy, but a way to reach this goal with less effort. Indeed, wealth managers often lament and make fun of their clients' obsession with tax issues. Rupert, a financial advisor who works in both New York and France, told it this way: "Many people only pay attention to how to organize their wealth in light of tax rules ... But money is much more frequently lost through a lack of understanding than through a lack of tax skills." Many of the wealth managers interviewed explained that they refuse to promote government-backed tax incentives because they view them as tools for short-term tax reduc-

tions that are often not profitable in the long run. Mike, a self-employed wealth manager, related the following:

Mike – Yes, taxes are a bit over-present, but I am against tax incentives because I consider taxes to be just one of the game constraints. They have to be taken into consideration. They need to be mastered perfectly, because they're everywhere. ... But we will never think or make a decision according to the tax advantages our position offers. On the other hand, it is clear that if we have two solutions, both of which are acceptable and involve the same amount of risk, we'll definitely take the one that generates the lowest taxes.

Wealth managers' depiction of their activities minimizes the role of tax advice, which nonetheless takes up a significant amount of their time, especially during debates regarding, votes on, and the enactment of public-finance legislation. The wealth managers I met never broached the subject of illegal practices that may lead to a tax audit. Austin, one of the first so-called family officers⁵ in France who manages a well-known family office in Paris, alludes to "what isn't official": "There is the issue of noncompliant practices, and we don't want to know about that. This is their own business, what they don't declare etc. If they have some undeclared assets abroad, that's not our problem." During a more informal meeting, a corporate lawyer who had previously worked in Switzerland as a tax advisor told me that his previous work activities mainly involve creating offshore companies (in places such as the Seychelles, Panama, Singapore, and the British Virgin Islands), arguing that his activities are completely legal. When I argued that this kind of setup makes it possible to hide great amounts of money from the authorities, he replied in the same way as Austin: "Yes, but we don't need to know about that." Wealth managers deny their responsibility in helping their clients hide money offshore and get away with it.

Even with their clients, wealth managers are ambiguous about who is responsible for fraudulent decisions. Peter, whose net worth is about 15 million euros, hired a tax advisor to fill out his tax return. He explained that he "took a chance" in not declaring some capital gains:

Peter – We [he and his tax advisor] took a chance, and we were wrong, by the way. I had a large tax reassessment. But I had anticipated it. I thought that it was quite risky and, well, yes, I agreed to do it. After that, he sort of forgot that he was the one who'd suggested taking the chance.

This fraud story shows how the advisor was indeed involved in a tax-dodging decision and how he denied having been involved in it.

It is difficult to assess the extent of tax evasion, especially because there is a serious lack of official data on the subject (Zucman 2013). Based on a unique Swiss data set, Gabriel Zucman found that around 8% of households' global financial wealth is held in tax havens. Combining micro-data leaked from financial institutions in tax havens with randomized audits and population-wide registry data, Alstadsæter, Johannesen, and Zucman (2017) estimated more recently that "the top 0.01% of the wealth distribution – a group that includes households with more than \$45 million in net wealth – evades about 30% of its taxes" (36). One thing that was striking in the remarks of wealth managers and the articles in *Gestion de fortune* was the double talk on tax evasion: Wealth managers emphasized that they do not encourage illegal practices, but they often simultaneously claimed that offshore practices are either legal or widespread in the profession. In the *Gestion de fortune*, tax evasion is often condemned. In an article describing the creation of a new section of the magazine dedicated to the "professionalization of assets," introduced as a way to minimize taxes legally, the journalist mentions a "commitment that has grown out of the wish to rule out tax setups that escape the scrutiny of tax authorities and are close to the limits of the law. Goodbye 'offshore' companies in the Bahamas or the Cayman Islands" (*Gestion de fortune*, No. 35, 1995). But the condemnation of tax-evasion practices implicitly reveals that such practices are commonly known and endorsed. A *Gestion de fortune* journalist ironically recounts that on airplanes bound for European countries "well known for their financial expertise and their taste for secrets," we are likely to meet "a lot of politicians ... but also many wealth-management specialists, who have always sworn they have never worked with foreign banks in their comfortable Parisian office" (*Gestion de fortune*, No. 137, 2004). These few sentences from another article also drip with irony: "After a few months' respite, 'specialists' in international tax optimization are back. Of course, they are working through firms located in global megalopolises (Luxembourg, Gibraltar, Jersey ...). Naturally, the offshore services they offer are absolutely legal. And surely, the grass is greener there than in France" (*Gestion de fortune*, No. 126, 2003). Another article relates the secrets of an executive manager involved in a "well-known private bank located in one of the nicest neighborhoods in Paris." This executive revealed that "his clients have sent more than 250 million euros abroad this week" (*Gestion de fortune*, No. 228, 2012).

Yet, the magazine does not manage to avoid the ambiguities it itself points out. Some advertisements clearly support offshore setups. An advertisement for a

conference in Switzerland is even titled “International Tax Fraud” and lists the following as one of the issues it will cover: “... how to enable your clients to send assets offshore legally?” (*Gestion de fortune*, No. 97, 2000). An article on Luxembourg’s tax system (*Gestion de fortune*, No. 59, 1997) alternates in tone between chastising and offering advice. The author begins by citing some Luxembourgish wealth managers’ remarks on the legality of their clients’ practices. He then changes to a chastising mode, expressing regret that some clients do not report their assets to tax authorities and instead take advantage of bank secrecy. But right after that, he discusses ways to send assets abroad without reporting them, explaining: “From entrance to exit, here are the main traps.” We then learn that the suitcase method “is definitely not recommended by Luxembourgish insurers,” but that it is very easy to make a money transfer through a bank: “No one will be any the wiser, since the bank doesn’t have to specify a name on the transfer document.” This detailed presentation of several illegal practices comes with the usual caveats. At the end of the article, we find out that “it is very difficult to recover hidden assets once they have been invested; this is by far the most disappointing feature of these kinds of deals.”

Wealth managers’ admission of their involvement in tax evasion is much more obvious when they meet each other. At a French Family Office Association meeting, for example, a tax specialist mentioned that anti-laundering policies pose an indirect risk for family officers’ clients:

Tax specialist – Officially, of course, anti-laundering policies focus on drugs, terrorism, etc. But sometimes when they look at drug traffickers or bin Laden’s followers too closely, they pick up others who have discrete accounts. Because the spotlights are not well focused, they bring individuals to light who would rather stay hidden. This is an issue that concerns us all because it shapes the way we’re going to organize family fortunes or individual wealth. So we have to keep that in mind.⁶

This excerpt shows how tax evasion is indeed a concern among wealth managers. The possible implications of tax-fraud scandals alluded to by this tax specialist are reminiscent of similar scandals in the early twentieth century. Indeed, it was because of the naming of well-known rich people evading taxes that Roosevelt was able to fight tax evasion and introduce a wealth tax in the 1930s (Thorndike 2009). In 1932, a tax-fraud scandal involving wealth managers and advisors from the Commercial Bank of Basel erupted

when French tax authorities found a list containing the names of wealthy clients who entrusted money to the bank’s headquarters in order to evade capital gains tax (Guex 2007). In the excerpt cited above, the tax specialist is also worried about a scandal that would reveal clients’ names and potentially put a stop to bank secrecy.

Wealth managers’ attitudes and practices regarding taxes – denying the importance of tax dodging in their daily practice, claiming not to be involved in their clients’ illegal practices, and participating in the legitimation and organization of tax dodging – make them key actors in the wealthy’s ability to get away with a number of illegal practices, from undervaluation to the use of offshore tax havens.

Conclusion

Tax exile, one of the most debated topics in France regarding wealth tax payers’ consent, can be considered a political construction to protect the wealthy from tax increases rather than a major tax-dodging strategy. Indeed, taxation is usually not a sufficient reason for people to leave the country, unless they already have a transnational way of life. While people are not necessarily easily transplanted, money is, as the technical offshore setups created by wealth managers show. These setups, which are targeted solely at the “one percent,” are a powerful way to escape taxation.

Appearances can be deceptive: leaving the country does not necessarily imply a protest against taxation, and paying taxes does not necessarily imply consent.

But I have pointed out another, much more common – but yet significant – tax-dodging practice: the undervaluation of assets in tax returns. Appearances can be deceptive: leaving the country does not necessarily imply a protest against taxation, and paying taxes does not necessarily imply consent. Loyalty does not signal apathy (Blondiaux 2001). I have argued that resistance to taxation does not necessarily involve open protest or the committed decision to leave a country. It is much easier to move, hide, or undervalue assets. In this regard, wealth managers do not only supply technical support by putting together complex tax-avoidance setups, they also participate in blurring the boundary between legal and illegal practices, thus legitimating some questionable tax-dodging practices.

Endnotes

- 1 For example, Gabriel Zucman (2008) measures tax evasion very carefully. Nonetheless, he uses the problematic expression “tax exile” to describe wealthy taxpayers’ departures from France, without investigating the role actually played by taxation.
- 2 All the interviews cited here were conducted in French; translations by the author.
- 3 Information report prepared by the Senate Committee of Finance (No. 351, July 2004), entitled “L’impôt de solidarité sur la fortune: éléments d’analyse économique pour une réforme de la fiscalité patrimoniale” (Wealth tax: Economic analysis in favor of wealth tax reform). The first part discusses “A large effect on fiscal relocations among those subject to the wealth tax” and focuses on “tax exile.” Online: <https://www.senat.fr/rap/r03-351/r03-351.html>.
- 4 Drawing on G.S. Becker’s approach to crime, a great deal of economic research has been published on this theory (Allingham and Sandmo 1972; Frey and Feld 2002; Slemrod 2007; Thomas 2015).
- 5 Family offices are a distinct part of financial and wealth management. Family officers claim to provide high-end, tailor-made consulting services to clients worth at least 20 million euros.
- 6 Minutes of an FFOA meeting held in November 2002 (private archives).

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Why No One Cares about Inequality (Except Us)

Martin Schröder

Much social science is motivated by a tacit understanding that excessive income inequality is “bad.” But just what is “bad” about income inequality? Many social scientists argue that it is more difficult for people to live a satisfying life when the distribution of incomes is highly unequal. Curiously, however, most empirical research shows that inequality does not lower life satisfaction, at least not in the way we thought it would.

In a widely cited study, Wilkinson and Pickett (2009; 2010) famously made the argument that many social scientists were eager to hear: they claimed that everyone is worse off in highly unequal societies. The reasons are said to be manifold: Some suggest that inequality breeds unhappiness because it lowers trust and increases status anxiety (Delhey and Dragolov 2014, 160). Others propose that people simply have a “taste” for equality, which highly unequal societies violate (Thurow 1971, 327). Still others argue that those who profit from inequality profit less than those who lose out from inequality lose, so that the net effect of income inequality on life satisfaction is negative (Ferrer-i-Carbonell 2005, 1015).

If this is so, then one empirical connection should have shown up time and again: societies with more income inequality should have less satisfied populations. Indeed, some empirical studies find this to be the case (Cooper, McCausland, and Theodossiou 2013, 952; also cf. Valdmanis 2015, 995). But an overwhelming number of studies make the opposite and

somewhat disturbing discovery: unequal societies seem to have more satisfied populations (Zagorski et al. 2014, 1105; Kelley and Evans 2016, 21; Bjørnskov et al. 2013, 90; Schneider 2012, 435; Cojocar 2014, 606; Berg and Veenhoven 2010, 187; Katic and Ingram 2017). Reviewing the literature, Clark and D’Ambrosio (2015, 1173ff.) find nine studies that document a negative impact of inequality on wellbeing, five studies documenting a positive relationship, six with no relationship and eight in which the link depends on other variables. Go figure! What these unclear results illustrate, however, is that, well, results are unclear: populations of unequal societies are neither more nor less satisfied than populations of more equal countries. In this sense, existing research really does not show that inequality lowers wellbeing.

Variation of inequality over time, not between places, influences life satisfaction

However, all existing studies share a common flaw: they compare the populations of different countries, instead of making comparisons over time. This is problematic, as people may get used to the long-run level of inequality they experience in their own countries. What they may not adapt to, however, at least not in the short run, is the experience of an increase in inequality in their own country, in comparison with the inequality that they had become accustomed to in the past. Technically speaking, people’s life satisfaction might not be influenced by a between-country effect of inequality, which draws its variation from relatively static differences between countries. Rather, people’s life satisfaction could be influenced by a within-country effect, which draws its variation from how inequality changes within countries over time. Indeed, empirical studies find astonishing re-

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sults when they distinguish these two effects.

One study could show that Germans who live in federal states with high levels of income inequality are as happy as Germans who live in federal states with a more

equal income distribution. But while the variation of inequality between different places has no effect on life satisfaction, Germans interviewed in years when inequality is higher are less satisfied than Germans interviewed in years when inequality is lower (Schröder 2016a). Cross-country studies confirm this finding: populations of countries with a higher long-run level of inequality (compared to other countries) are no less satisfied with their lives. But within each country, people are less satisfied during years in which they experience more inequality than during a typical year in their country (Schröder 2017a). In other words, people are just as happy in countries where inequality is permanently higher as they are in countries where the level of inequality is permanently lower. However, they are unhappier when inequality in their own country exceeds the level they have come to experience in a typical year. It is thus variation of inequality over time, rather than the relatively stable differences in inequality between countries, which influences life satisfaction. But why should people be bothered by short-run increases in inequality over time, rather than by the long-run level of inequality in their country compared to another? Much evidence suggests that people simply grow accustomed to whatever the long-run level of inequality is, so that it has no influence on their life satisfaction.

People adapt to whatever they find to be the case

Many say that adaptation to the status quo is a universal feature of human existence. Kahneman et al. (1986: 730f.) suggest that “any stable state of affairs tends to become accepted eventually, at least in the sense that alternatives to it no longer come to mind.” Melvin Lerner (1982, 10) argues that “people imbue social regularities with an ‘ought’ quality” and George Homan (1974, 249–50) claims that “what people say ought to be is determined in the long run and with some lag by what they find in fact to be the case.” An extreme illustration is slavery in antiquity, which was widely seen as normal, simply because it existed over extended periods. People got so used to the idea of slavery that even slave revolts never aimed to abolish slavery as an institution. Slaves fought for their individual freedom, but never against the concept of slavery (Bradley 1989, xiii, 129–30; Vogt 1974, 89, 40; Finley 1959, 155). Paradoxically, they “fought for a freedom that included the right to possess other individuals as slaves” (Elster 1986, 152). Think about how crazy that is. In a world marked by slavery, even those who fought it could not imagine life without it. It is a stark illustration that, however unfair something

may appear to a detached observer, it may not bother those who actually experience it, as they simply adapt to whatever they see as normal.

The same seems to be true for inequality. Empirical studies have shown that when inequality increases, people tolerate more inequality within three to four years, so that about 60 percent of each increase in inequality gets “absorbed” as people adapt their normative expectations to the new reality they experience (Schröder 2017b). This adaptation of expectations to the status quo means that people are no less satisfied in societies with a permanently higher level of inequality. But precisely because people’s fairness views adjust to increased inequality with a time lag, short-run increases in inequality bother them, as long as their fairness views have not yet adjusted.

Adaptation to inequality: A new answer to old research questions

That people adapt to inequality with a time lag can explain why the long-run level of inequality does not seem to influence life satisfaction, while a short-run increase in inequality does lower life satisfaction. It also explains why people routinely acknowledge that there is – in principle – too much inequality, but then just shrug their shoulders and go about their lives, unfazed by what seems outrageous to those who are unaccustomed to high levels of inequality. It also implies that when inequality increases, a window of opportunity of about three to four years exists in which people’s normative views have not yet adjusted to increased inequality. After this period of adjustment, the new normal has infected people’s fairness views, so that they cease to experience cognitive dissonance between what they perceive as factual and what they perceive as fair.

This could explain why Meltzer and Richard’s (1981) model, which posits that more inequality leads to more calls for redistribution, often fails empirical tests. Some confirm Meltzer and Richard’s idea that when more inequality exists, more support for redistribution ensues (Finseraas 2009). But others did not find that more inequality leads to more redistributive demands (Kenworthy and McCall 2008; Schmidt-Catran 2016, 21–22). Such unclear results may come about because fairness views adapt to the status quo as described above: when inequality increases, people’s expectations about inequality adapt, but with a time lag. While normative views have not yet adjusted to the increased level of inequality, more inequality does indeed lead to stronger demands for redistribution. However, after fairness views

have adapted to the new normal, the gap between what is and what ought to be has closed, so that more inequality ceases to translate into stronger demands for redistribution.

That people seem to adapt their fairness views to the existing level of inequality also raises a philosophical question: If people adapt to a bad thing, does it cease to be a bad thing? The idea of slavery, which no one seems to have worried about in antiquity, surely lets us assume that something can be bad even though no one realizes this when they experience it. But that people's life satisfaction really seems unaffected by the long-run level of inequality may also explain why so few people really seem to care about inequality, except a few social scientists here and there.

Tasks for future research

To be sure, people have revolted against the status quo time and again. This implies an omnipresent race between outrage against and adaptation to any state of social affairs. Which of the two takes precedence over the other is an empirical question, but it has largely failed to animate empirical studies in our field. We know little

about the conditions under which people's fairness views adapt to the status quo, and under which conditions they change it. Nor are we aware of the conditions that get people to redefine as normal what they considered repulsive yesterday. Thus, we do not know under which conditions material reality infects social justice views. And we don't know much about the opposite conditions either: those under which normative ideals change material reality. We know only that for this to occur, people must stubbornly refuse to adjust their normative views to what they perceive to be the case, so that anger eventually leads to revolt, which leads to a change of the status quo towards what people see as fair.

We therefore need empirical studies that show under which conditions each of the two possibilities occurs: Under which conditions do people adapt their view of what ought to be towards what they find to be the case? And under which conditions does the opposite happen, so that people's fairness views get them to revolt? Such studies might find that people adapt to all sorts of circumstances that seem intolerable to an outside observer. Notably, they might find that no one cares about inequality because people accept almost anything as long as they have time to get used to it.

Endnotes

- 1 I treat life satisfaction and happiness as synonyms here.
- 2 In technical terms, there is no negative "between" effect, but there is a negative "within" effect of inequality on life satisfaction (cf. the distinction between "between" and "within" effects in hybrid regressions in Allison 2009; Schunck 2013).
- 3 Note, however, that this is an important discussion in the classics. Karl Marx ([1859] 1904, 11–12) is most prominently associated with his central dictum that "material life determines the general character of the social, political and spiritual processes of life. It is not the consciousness of men that determines their existence, but, on the contrary, their social existence determines their consciousness." Max Weber's study of

the Protestant Ethic is widely hailed as an important testimony of how ideas influence material structures as "switchmen" (Weber [1920] 1978). Economic sociology has shown how ideas influence what can be traded on markets (Zelizer 1979; 1985; Dobbin 1994). Political economy has shown how ideas about legitimacy influence varieties of welfare states and capitalisms (Brooks and Manza 2007; Svallfors 2010; Schröder 2009; 2013; 2016b). But neither discipline has told us under which conditions fairness views change material reality and under which conditions fairness views simply adjust to material reality. A literature that one could draw on might be the study of revolts (Moore 1978; Tilly 1978).

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Book Reviews

Rainer Diaz-Bone · 2015

Die „Economie des conventions“: Grundlagen und Entwicklungen der neuen französischen Wirtschaftssoziologie.

Wiesbaden: Springer VS

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Speaking about “new economic sociology,” what first comes to mind is, usually, “American” and not “European” economic socio-

logy (Smelser and Swedberg 2005, 17). This is no coincidence. The label was created in the United States, as a response to what was then understood as “economic imperialism” (Convert and Heilbron 2007, 35): new efforts to explain “the social” by generalizing the rational-choice approach, advancing

new institutional economics, or turning to behavioral economics altogether. In countering these tendencies, “new economic sociology” and “socio-economics” represented alternative strategies to bring sociology back in (ibid., 45–46): the “intra-disciplinary strategy” sought to revitalize the anaemic subdiscipline of economic sociology (with the support of the Russell Sage Foundation, albeit ironically under the latter’s “Behavioural Economics” program), whereas the “interdisciplinary strategy” led to the founding of the “Society for the Advancement of Socio-Economics” (in mimicry of the already existing “Society for the Advancement of Behavioral Economics”).

In this context, the “new” American economic sociology, which gathered around a concrete interest in the relations, networks, organisations, and institutions that economic action is “embedded” in, came to be distinguished from the “old” American economic sociology, whose abstract structural-functional orientation was now depicted as an “oversocialized” complement to the “undersocialized” vision of neoclassical economics (Granovetter 1985; 1990). At the same time, although this was less explicit, the new American economic sociology (e.g., Smelser and Swedberg 1994; Guillén 2002) kept their distance from what could be referred to as old or classical European economic sociology, or at least from its more historical, holistic, and critical strands (which had some parallels in old American institutionalism).

Now, what is the “new French economic sociology” about? How does it differ from older approaches in French economic sociology, and how does it relate to the classics of economic sociology? Diaz-Bone’s monograph

(henceforth EC), which reconstructs the institutional origins and conceptual foundations of the “economy of conventions,” provides answers to these questions. It points to the long-standing Durkheimian tradition and the strong influence of structuralist and post-structuralist research paradigms in twentieth-century French scholarship (EC 28–29, 61–66) as well as to the fading dominance of late-Marxist regulation theory in French socio-economics towards the end of the century on the one hand (EC 218–21) and of Bourdieu’s structuralist constructivism in contemporary French sociology on the other (EC 56–59, 64–67). The most important attribute distinguishing the economy of conventions, which is at the core of the new French economic sociology (EC 22), from earlier scholarship in this field is that it is “pragmatist” in orientation (EC 30–32, 324–27). It focuses on what actors do in situations of uncertainty, how they may interpret situations in the light of different conventions, each of which allows them to coordinate action in a different way (EC 135–36, 327–28). As to other “praxeological” approaches in contemporary French scholarship, namely, actor-network theory (EC 32–33), the economy of conventions shares the latter’s interest in “distributed cognition,” to which actors contribute as much as things, or so-called “collective cognitive dispositives” (EC 38, 259–60, 324).

As a label, the economy of conventions stands for a “transdisciplinary” academic movement, or network of scholars, whose personal scope and scientific output may well be comparable to that of new American economic sociology (EC 22), but whose international reception has long been hampered by it being French (EC 51–54, 357–60). More precisely, much of the litera-

ture that Diaz-Bone presents in his book to illustrate research strategies and key findings of the economy of conventions is undoubtedly more accessible and attractive to a French audience than an Anglophone or international one. This is not only a matter of language or publication strategies, but also due to the theoretical and empirical frame of reference of the studies, whose focus likewise is on France. The main task of this book is thus to act as an “intermediary” (EC 18, 110–11) between the original (French) authors and an extended (German) readership, by translating, contextualizing, and explaining what the economy of conventions is all about. This requires going beyond its most prominent and popularized works, which are already available in various languages, such as Boltanski and Thévenot’s *On Justification* (in French 1991; in English 2006; in German 2007) and Boltanski and Chiapello’s *The New Spirit of Capitalism* (in French 1999; in English 2006; in German 2003).

The latter can be understood as a sequel to the former in that *On Justification* introduces six ideal types of moral order, or logics of coordination, which *The New Spirit of Capitalism* complements with yet another one. This yields a typology of seven alternative conventions (“commercial,” “industrial,” “domestic,” “civic,” “reputational,” “inspirational,” and “network”); Boltanski and Chiapello 2006, 136), which may be available to coordinate action in different situations (EC 139–58). While a concrete situation may be governed by a prevailing convention, the pragmatist stance of the economy of conventions is that actors are often required and also competent to negotiate between a plurality of conventions (EC 324–25). Moreover, the available conventions may dif-

fer across time and space, with institutional investments being made to increase their scope and durability (EC 89–91). Similar typologies of conventions are reported for various subject areas throughout the book, which does remind the (German) reader of Weber’s ideal-typical method, not least because Diaz-Bone repeatedly uses this term as well (EC 107, 130, 159, 197).

Such ideal types can be identified for the “meso” level of certain industries, regarding the assessment of human resources (EC 107–9), creditworthiness (EC 208–11), or product qualities (EC 159, 171, 183), as well as for the “macro” level of political economies, regarding the classification of different production systems (EC 197–99), statistical regimes (EC 301–3), and legal cultures (EC 264–67). The identification of the latter is directly inspired by Weber’s ideal types of law (EC 266; Weber [1922] 1978, 641–900), which raises the question to what extent the economy of conventions is in line with Weber’s interpretative sociology and ideal-typical method (EC 336–39). Perhaps overemphasizing Weber’s “deductive” use of generalized ideal types over their “inductive” origin in different value spheres and the respective value orientations of actors (Lindbekk 1992), Diaz-Bone highlights that the position of the economy of conventions is “internalist,” which would rule out the imposition of any “external” typologies (EC 343). At the same time, the economy of conventions obviously does employ ideal types also as heuristics to find similar conventions at work in different empirical constellations (EC 140).

Somewhat internalist is also Diaz-Bone’s standpoint as a self-declared “intermediary” (EC 18) of the economy of conventions who has been engaged in this busi-

ness for quite a while already: the bibliography includes 35 entries with him as author, co-author, or editor of relevant writings. Yet, this first comprehensive monograph (EC 19) on the economy of conventions is cautious in making any authoritative claims about the academic movement as a whole or its representatives in particular, even though the reconstructions and qualifications offered cannot be neutral (EC 111). However, by and large, Diaz-Bone seems to confine his task to presenting a plurality of works from within and to merely orchestrating the multiplicity of voices, without removing any dissonances or filling any silences. This accounts for the main benefits as well as the limits of this volume.

What the reader receives is a comprehensive *vade mecum* that opens up a literature, much of which may so far have remained outside his or her purview, but which is certainly worth exploring in any endeavour to advance economic sociology (as well as socio-economics) by old or new means. What this book does not provide is certainty about how to better counter economic imperialisms of a rationalist, institutionalist, or behavioralist kind – be it with new American or old European, contemporary French or classical German economic sociology. Other than what is distilled from the writings of the scholars discussed (mostly with regard to new institutional economics), there is no systematic comparison or confrontation of different explanations of “the social” in economic spheres of action. The choice of argumentative strategy and empirical proof rests, quite pragmatically, with the reader. What may be comforting, in this situation, is that the availability of alternatives can be regarded as a value in itself (EC 123, 319).

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Marco H. D. van Leeuwen
2016

Mutual Insurance 1550–2015: From Guild Welfare and Friendly Societies to Contemporary Micro-Insurers.

**Palgrave Studies in the History
of Finance.**

London: Palgrave Macmillan.

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Marco van Leeuwen's book *Mutual Insurance 1550–2015* offers a historical analysis of the different forms of micro-insurance in the Netherlands and thereby complements the growing number of historical studies about British friendly societies (Pearson 2002; Weinbren 2006; Gorsky 1998). Van Leeuwen builds on previous economic and sociological work on insurance problems, such as adverse selection and moral hazards (Akerlof 1970; Heimer 1985), and makes use of welfare economics (Arrow 1963) in order to chart the demands for and the conditions of mutual insurance in light of their socio-historic context.

The book traces the development of mutual insurance from 1550 through 2015. In the introduction, the author explains his interest in publishing such a comprehensive historical account: Whereas mutuals had played a significant role in the earlier Dutch

welfare economy, nowadays, state and commercial insurance providers dominate the scene; the (dis)advantages of the mutual form of organizing insurance seem to have been forgotten. Hence, van Leeuwen wants to provide information as to how and why micro-insurance worked in the past, as well as to discuss whether it can still work today. Chapters 2–5 are arranged in chronological order: The analysis covers the guilds during the period of 1550–1800 (Chapter 2), the friendly societies in the nineteenth century (Chapter 3), trade union insurance from 1900–1965 (Chapter 4) and new initiatives from 1965–2015 (chapter 5). Chapters 2–4 are previously published articles that were rewritten for this book; Chapter 5 presents new material on current mutual insurance schemes. The conclusion summarizes the book's insights into the principles and practices of mutual insurance.

Overall, van Leeuwen delivers a highly informative book. The chapters of his historical analysis (2–4) are extremely rich in detail, and he aptly frames the material gathered from Dutch statistical offices by asking how and why mutuals "worked." He explicates which parts of the population joined these insurance providers, what kinds of damages they covered, and how classic problems of insurance were solved. However, the discussion of empirical details does not quite merge into one consolidated answer or hypothesis, but instead unravels into various interesting suggestions.

Two reoccurring analytical themes are particularly worth mentioning: sociability and formalization. Sociability appears to be at the core of what distinguishes mutuals from other forms of insurance, and therefore it is a promising starting point from which to

determine whether there is a place for mutuals in today's insurance mix. Van Leeuwen's analysis shows that sociability is a successful means for combating moral hazards. The Dutch guilds, friendly societies, and trade unions attached a great deal of significance (and resources) to collective festivities, the attendance of commemorative days, and representational equipment such as banners (61, 138). Furthermore, sociability also appeared in the more mundane activities such as attending members' funeral services and visiting sick members. At one point in the book, van Leeuwen departs from simply describing such activities and remarks more generally on the functions of sociability (146, similarly 265). First, he describes how sociability creates visibility or transparency of the actual physical and economic condition insurance members are in. This makes it more difficult to collect undue claims from the insurance funds and thereby integrates social control into the insurance relationship. Second, he states that social ties of mutual insurances go beyond mere insurance relations as they additionally rely on friendship or collegiality. The interweaving of different types of relationships increases the chances of getting caught with fraudulent claims and raises the overall stakes for committing fraud, since one would not only harm fellow insureds, but also one's friends and colleagues. Third, he notes that allowing for participation in the decision-making processes of mutual insurance meant that the funds were not necessarily contractually bound to fixed premiums and benefits, but would organize support to react to external events by lowering or raising them – provided members agreed. For all three functions of sociability, there are

numerous examples in the book, though the use of the concept itself remains somewhat unsystematic (for example, there is no subchapter on sociability in the last two chapters).

With these observations of sociability in the history of mutual insurance in mind, van Leeuwen moves on to the more recent phenomenon of Dutch bread funds. Surprisingly, he almost completely omits the previously emphasized aspects of social control. It is only in passing and towards the end of the book that van Leeuwen wonders at what point the sociability of modern mutuals could be 'excessive' in terms of social control (278): Would potential members be willing to expose themselves to practices of monitoring? At this point a substantial discussion of realistic alternatives to membership in mutual insurance would have been apt. In previous chapters van Leeuwen had already argued that in the nineteenth century the incipient middle class had an alternative to voluntarily joining friendly societies, namely poor relief; so did the working class when it came to the more affordable burial insurance. Poor relief, however, was hardly considered an option because it was damaging to one's budget as well as to one's reputation. Such information about the socio-economic status and realistic alternatives would have been extremely helpful in figuring out the relevance and the potential of today's Dutch mutuals. Instead, van Leeuwen simply remarks that privacy concerns may seem less relevant when control is exercised by friends and in an atmosphere of conviviality (278).

Next to sociability, van Leeuwen also stresses organizational experience and formalization as central themes relating to mutual insurance. Indeed, several tools

that van Leeuwen mentions in the book are part of the classic repertoire of insurance techniques. For example, waiting periods, i.e. arranging for a fixed amount of time before one can claim support from one's fund, are thought to reduce adverse selection. Co-insurance is another widely used mechanism which is said to combat moral hazard by making members participate in the costs of their claims (Heimer 1985). And the reliance on statistical data for purposes of extrapolating future costs is typical for many insurance branches (Lengwiler 2003). While these techniques are applied in all types of insurance, the book also emphasizes the organizational expertise that is specific to micro-insurers. Van Leeuwen points out that mutuals were not able to sustain the same level of sociability as they expanded. Instead of frequent gatherings and festivities, more formal organizational tools were increasingly experimented with to gather information, control for moral hazard, and conduct economic planning (262). Van Leeuwen shows that as mutual insurance providers grew in size, they tended to adopt structures similar to those of commercial insurance providers (or in the case of Dutch labor unions – morph into state insurance). The existence of mutuals therefore seems to hinge on maintaining a balance between sociability and organizational rationalization. He could have engaged more systematically with the argument, widely discussed in the literature (Knights and Vurdubakis 1993; O'Malley 2004), that it is this tension that marks the historic development of mutuals. It is, however, only towards the end of the book that van Leeuwen wonders whether formalization might not be detrimental to the mutuals' sociability (278). Unfortunately, he doesn't pursue

this intriguing thought further, maybe because it would have been somewhat detrimental to his narrative that one “incarnation” of mutual insurance acts as a “guiding beacon” to the subsequent one (14, 248, 253) – an assumption that justifies the goal of the book, namely to close an information gap about a widely forgotten form of insurance.

In conclusion, this book provides wide-ranging empirical insights into Dutch guilds, friendly societies, and labor unions. The chapter on new initiatives is less comprehensive, though still highly informative. Had these in-depth descriptions of mutual insurance organizations and their social environments been channelled into more general statements, for example about the tensions between different principles of association, such a study would have provided us with better analytical tools to

assess the question of how and within which limits mutual insurance schemes can still work today.

Endnotes

- 1 Van Leeuwen also comments on micro-insurance in developing countries, which I am not going to discuss, since the initial puzzle focused on the Dutch welfare economy.

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