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Can the structure of inequality explain fiscal redistribution?
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Malte Luebker

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Can the structure of inequality explain fiscal redistribution? Revisiting the social affinity hypothesis*

Malte Luebker, Institute of Economic and Social Research (WSI), Dusseldorf

Abstract: Lupu and Pontusson (2011) argue that the structure of income inequality, rather than its level, can explain differences in fiscal redistribution across modern welfare states. Contrary to the assertion that there is robust evidence in support of this proposition, the present paper challenges the argument that the distributional allegiances between social groups are a function of relative income differentials. It makes three central claims: (a) skew in the earnings distribution, the key explanatory variable in the empirical tests of the original paper, is a result of labor market institutions and hence endogenous to the welfare state; (b) relative earnings differentials are not a valid proxy measure for the structure of income inequality, the concept of theoretical interest; and (c) there is no indication that skew in the distribution of incomes (rather than earnings) is positively associated with fiscal redistribution. In sum, revisiting an influential contribution to the literature offers no support for the proposition that the structure of inequality has consequences for fiscal redistribution.

Keywords: income distribution, redistribution, labor market institutions, wages, social structure

JEL classification: D31 personal income, wealth, and their distributions, P16 political economy, J31 wage level and structure

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1. Introduction

Can the structure of inequality explain why the extent of fiscal redistribution varies over time and across countries? An extension of the social affinity hypothesis to the economic sphere proposes exactly this. It is an intriguing proposition, especially in light of failed attempts to establish the link between the level of inequality and redistribution. Lupu and Pontusson (2011, p. 316) argue that "middle-income voters will empathize with the poor and support redistributive policies when the income distance between the middle and the poor is small relative to the income distance between the middle and the affluent". Drawing on the literature on racial and ethnic fragmentation (Luttmer, 2001) and Shayo's (2009) notion of perceived social distance, they develop a framework where – in the absence of cross-cutting ethnic cleavages – income differentials are the source of social affinity between groups. Social affinity, in turn, shapes the allegiance of the middle class and hence which political coalitions emerge in the distributional conflict (see also Kristov *et al*., 1992).

In its economic variant, the social affinity hypothesis builds on the idea that the middle class has a decisive role in the distributional conflict, commonly associated with the canonical median-voter model (Romer, 1975; Meltzer and Richard, 1981). However, unlike these early models, the literature on social affinity and other second-generation theories of redistribution place greater emphasis on social structure and the formative effect of institutions. For instance, Korpi and Palme (1998) argue that the middle class supports redistribution when social benefits are universal, while Moene and Wallerstein (2001) stress the role of the welfare state in providing insurance against risk. Iversen and Soskice (2006) outline how electoral systems influence the alliances entered by the middle class. It is a natural extension to consider how the position of the middle class in the income distribution – its relative distance from the bottom and top – forms its preferences for redistribution. This question has gained relevance with the concentration of incomes at the very top and the near-stagnation of median incomes (see Thewissen *et al*., 2015). It is also of substantial consequence for the future trajectories of welfare states.

Lauded as an "important advance" (Kelly and Morgan, 2012, p. 5) and a "significant contribution to the literature on redistribution" (Dimick *et al*., 2017, p. 414), the paper by Lupu and Pontusson has received substantial scholarly attention and given rise to a small but growing branch of enquiry that further explores the "structure of inequality logic" (Tóth *et al*., 2014; see also Dallinger, 2015; Hansen and Jensen, 2018). It has also introduced a new variable into the comparative political economy literature, namely skew, a measure for the relative position of the middle class between the two poles of the distribution. The paper's impact is hardly surprising, given that it relates to a salient aspect of the present political discourse – the fate of the middle class in an age of income polarization – and lays down a finely textured and intuitive theoretical argument. Last but not least, Lupu and Pontusson provide an empirical test that confirms their predictions and remains robust across a number of specifications: skew cannot only predict redistribution and social spending, but also public opinion and government partisanship, in line with theoretical expectations about the transmission mechanism.

Cited widely and overwhelmingly with approval (see e.g. Förster and Tóth, 2015, p. 1783), the empirical validation of the proposition has so far attracted little scrutiny. A partial exception is the work by Alt and Iversen (2017, p. 22), who suspect that findings in favor of the social distance model might be driven by omitted variable bias. Given the prominence that the structure of inequality has gained in the literature on redistribution, the present paper revisits the original analysis by Lupu and Pontusson (2011) in greater detail. It makes three central claims: (1) Like fiscal redistribution itself, skew in the earnings distribution is an outcome of policies and institutions aimed at creating greater equity, notably labor market regulation. This gives rise to endogeneity. (2) Moreover, skew in the distribution of earnings among full-time workers is not a valid proxy measure for relative income distances, the concept the theoretical interest. The distinction between earnings and incomes is crucial, given that significant redistribution occurs between those in employment and those who are not (and hence have no earnings). (3) In line with theory, a valid test should therefore rely on data for the structure of income inequality (rather than earnings), However, when such a test is performed, it produces no indication that skew is positively associated with redistribution or non-elderly social spending.

In short, this paper argues that the income-based application of the social affinity hypothesis lacks empirical support and that previous findings presented in its favor are not robust. It reasons that the failure to confirm its predictions might be linked to its micro-foundations, which are as demanding as they are intuitive.

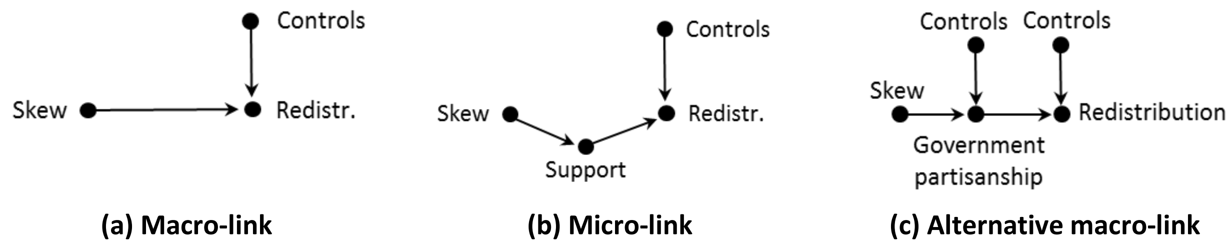
2. The structure of inequality: Solution to a long-standing paradox?

The idea that greater inequality of market incomes should lead to more fiscal redistribution (usually measured as the reduction of inequality due to taxes and transfers) is one of the most extensively tested propositions of the political economy literature. Although uncontroversial from a rational choice perspective, empirical work has produced inconsistent support and some evidence to the contrary (see Kenworthy and McCall, 2008, and the review in Jäntti *et al*., 2018, p. 3ff). Lindert (2004) pointedly referred to this as the "Robin Hood paradox". In the words of the editors of the American Political Science Review, Lupu and Pontusson (2011) offered one "very plausible resolution of the paradox" (APSR, 2011, v): Inequality matters, although it is not the level, but the structure of inequality that matters. Like much great scholarship, their theory comes in a complex and a concise version. The complex version explains how the concept of social affinity that stems from shared identities can be transferred to income stratification, and is best elaborated in the original paper. The concise version, as summarized by Pontusson in a joint paper with Weisstanner, reads as follows:

> "The Lupu-Pontusson thesis boils down to this: if the distance from the middle to the bottom of the income distribution is smaller than the distance from the middle to the top, middle-income citizens will be inclined to join a pro-redistribution coalition with the poor, but if the distance to the bottom is bigger than the distance to the top, middle-income citizens will be inclined to join an anti-redistribution coalition with the affluent." (Pontusson and Weisstanner, 2017, p. 4)

As a helpful short-hand, Lupu and Pontusson use the term "skew" to describe the relative position of the middle class. The utilize OECD data on earnings differentials and measure skew as the ratio of the upper decile ratio (D9/D5) over the lower decile ratio (D5/D1). Hence, values greater than unity indicate that the median is closer to the poor than to the rich. Under the structure of inequality model, greater skew should cause more redistribution, with policy preferences of middle-income voters acting as the causal mechanism (*ibid*., p. 328). Figure 1 summarizes the overall macro-link between skew and redistribution (Panel a) and the micro-foundations that run from skew through support for redistribution to actual redistribution (Panel b). The model allows for other causes of redistribution and expects that the influence of skew remains unaffected when control variables are introduced. Lupu and Pontusson (2011, pp. 330ff.) also suggest an alternative macro-link where the effect of skew on redistribution is mediated by government partisanship (Panel c).

Figure 1 Diagram of causal links between skew and redistribution under the structure of inequality hypothesis



Source: Own compilation based on Lupu and Pontusson (2011).

Lupu and Pontusson (2011) use the first part of their empirical section to show that there is indeed a strong positive association between skew and redistribution, which is robust under different model specifications and remains intact when rival causes are included. They corroborate their findings by switching the dependent variable, showing that skew is also strongly associated with non-elderly social spending (*ibid*., p. 327). Next, they turn to the micro-linkage and use data from public opinion surveys (namely the ISSP and the ESS) to illustrate in a scatter plot that greater skew is generally associated with a higher share of middle-income respondents who support redistributive government interventions (*ibid*., pp. 328f.). Moreover, there appears to be a reasonably strong relationship between support for redistribution and actual fiscal redistribution (anomalous findings from Switzerland and Spain aside), offering "suggestive evidence" that the micro-linkage through middle-class preferences holds (*ibid*., pp. 329f.). They substantiate this claim by showing that, as predicted by the alternative macro-link, "skew is consistently associated with left participation in government" (*ibid*., p. 331) and can point to more tenuous evidence that left governments pursue more redistributive policies (*ibid*., p. 332). In total, some 30 regressions consistently produce findings in line with the causal paths laid down in Figure 1, leading to the conclusion that there is "robust evidence in support of the core hypotheses generated by this theory" (*ibid*.).

The application of the social affinity hypothesis to the structure of income inequality – henceforth, for brevity, just social affinity hypothesis – offers us a theory of system behavior that is rooted in the actions of individuals (Coleman, 1990; see also Hedström and Ylikoski, 2010). While the social mechanism that drives macro-level outcomes is intuitively appealing, it is worth to briefly pause and reflect on its three central premises. Firstly, individuals must be able to accurately place themselves within the distribution of incomes and then correctly assess relative income distances from this vantage point. In essence, as shown below, voters are expected to detect in how far an existing distribution departs from the usual lognormal pattern. Secondly, they must make this assessment the central pillar in their stance towards redistribution and subsequent voting decisions, overriding their inequity aversion, possible insurance motives and individual utility maximization (to name but a few of the rival explanations advanced in the literature). Thirdly, the electorate's preference for redistribution must then translate into actual government policy. While the final premise, policy responsiveness, finds support elsewhere (Kang and Powell, 2010; Luebker, 2014), the first two assumption seem much more demanding in the light of recent advances in behavioral economics – a point that is taken up again in the conclusions.

3. Endogeneity: How governments cause earnings skew

Central to the model developed by Lupu and Pontusson is the idea that the causal link runs from the structure of inequality to government policy and redistribution. But what if skew is itself an outcome of government policy? To their credit, Lupu and Pontusson (2011, p. 332) raise the possibility of endogeneity, pointing out that their "theoretical and empirical discussion treats the structure of inequality as an exogenous variable that causes changes in redistribution". Their primary concern is whether redistribution might cause skew, or that endogeneity arises from reverse causation. Addressing this potential challenge to their findings, they point to their model specifications (where, among others, skew is averaged for the years preceding the dependent variable) and conclude that endogeneity does not represent a serious challenge to their interpretation (*ibid*.). However, endogeneity can also arise when a confounder influences both the dependent and independent variable. So do earnings skew and fiscal redistribution have a common origin?

This section concludes that this is the case. It first makes the general argument that redistribution and the structure of earnings inequality are indeed both (at least in part) the result of government policies and socio-economic institutions. It then turns to one specific policy tool, namely minimum wages. The choice of minimum wages as an illustrative example is based on two considerations: (a) There is an extensive body of theoretical literature that provides us with a solid understanding of how minimum wages affect the earnings distribution. (b) In addition, the impact of minimum wages on the structure of earnings inequality is well-documented in the empirical literature.[1] Therefore, unlike papers that take stock of all relevant political and institutional determinants of wage inequality (Wallerstein, 1999; Pontusson *et al*., 2002; Koeniger *et al*., 2007), this section has a more limited objective: to support the broader argument that earnings skew is endogenous by tracing the effects of one policy variable in detail. This discussion then informs a replication of key findings from Lupu and Pontusson (2011) and motivates the inclusion of minimum wage variables to control for the endogeneity of skew.

3.1. An alternative causal model to link skew and redistribution

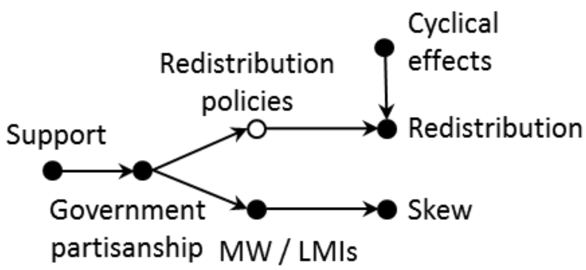
While the notion that fiscal redistribution is a function of tax and transfer systems – and the tweaks that governments make to them – is now a staple of the political economy literature, the literature has paid somewhat less attention to the distributional effects of government interventions into labor markets (for notable exceptions see Wallerstein, 1999; Bradley *et al*., 2003; Rueda, 2008). However, the idea that political and institutional factors influence the distribution of earnings is hardly new. As Pontusson *et al*. (2002) show, union density, bargaining centralization and public sector employment all reduce earnings differentials. Importantly, they often have a much stronger effect on the D5/D1 ratio than on the D9/D5 ratio. Hence, they influence the structure of earnings inequality, giving rise to skew. Lupu and Pontusson (2011, p. 332f.) cite this work when discussing the causes of skew in their closing paragraphs, but they do not draw the crucial conclusion: that earnings skew is endogenous to modern welfare states.

Conceptualizing earnings skew as an outcome of public policy and socio-economic institutions has important implications for the direction of the causal links between the variables used by Lupu and Pontusson (2011). Figure 2 re-arranges the sequence of their key variables and

[1] A related, more technical consideration is that the level and existence of statutory minimum wages are a policy variable, as opposed to an outcome variable. The same applies to employment protection legislation (EPL). Other potential explanations of skew – such as such as educational spending, vocational enrollment or collective bargaining coverage – are arguably not policies, but policy outcomes (akin to earnings skew or fiscal redistribution).

outlines and an alternative model of the causal links between them. Following Lupu and Pontusson, it is likely that government policy is responsive to voters' demand for greater equity (see also Brooks and Manza, 2006).[2] However, the response of governments need not be limited to fiscal redistribution policies, but they can also resort to "direct normative redistribution", i.e. policies that influence the distribution of market incomes (Hicks and Swank, 1984). These include interventions into the labor market, for instance by setting up institutions that support (or hinder) union strength and collective bargaining.[3] Another prominent example for a deliberate policy intervention is the minimum wage. Although the details of its operation are often left to semi-autonomous bodies, governments usually exert significant influence on the level of minimum wages (ILO, 2013, pp. 58ff.). Crucially, as argued in Pontusson *et al*. (2002, p. 292), "left governments are likely to set the minimum wage closer to the median wage than right governments". Likewise, it is plausible that left governments are also more likely to introduce minimum wages (unless strong collective bargaining institutions serve as a functional equivalent; see Rueda, 2008; Eldring and Alsos, 2012). If these arguments are correct, the partisan orientation of governments – one of the transmission mechanisms in the model developed by Lupu and Pontusson – should not only influence redistribution policies and subsequent redistribution, but also shape labor market institutions (LMIs) and ultimately the structure of earnings inequality.[4]

Figure 2 Diagram of alternative causal links between skew, partisanship and redistribution



Source: Own compilation.

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[2] The ISSP item used by Lupu and Pontusson reads "it is the responsibility of the government to reduce the differences in income between people with high incomes and those with low incomes" (see e.g. ISSP 2009, *Social Inequality IV*, Q6b). Although commonly interpreted as support for redistribution (Luebker, 2007; Dallinger, 2010), governments can reduce inequality also through labor market regulation.

[3] Relevant examples would be the Ghent system in the Nordic countries and recent attempts by conservative governments to undermine it (Bandau, 2018), the extension of collective bargaining agreements (which has recently come under attack in countries such as Portugal; see Schulten *et al.*, 2015) or Austria's still-stable system of multi-employer bargaining though chambers with compulsory membership (Pernicka and Hefler, 2015).

[4] As Rueda and Pontusson (2000, p. 376) point out, the effect of partisanship on the earnings distribution is clearly detectable in liberal market economies, but may not hold for social market economies (where the wage floor is generally determined without direct government involvement). These complexities are touched on again below.

In other words, the central argument made here is that the structure of earnings inequality and fiscal redistribution are jointly determined.[5] Although radically different in the way it links the relevant variables, the alternative model is perfectly reconcilable with the findings reported in Lupu and Pontusson (2011). This holds not only for the association between skew and redistribution, but by extension also for the association between skew and social spending. However, the relationship is not one of cause and effect, but the variables are connected through a confounder in the form government policy (approximated by partisanship) and welfare state institutions more broadly. Likewise, under the alternative model, support for redistribution should be associated with both redistribution itself and with skew in the earnings distribution (in line with the suggestive findings in Lupu and Pontusson, 2011, pp. 329f.). Moreover, one should expect an association between skew and government partisanship (though the direction of causality is reversed). Taken by themselves, the results obtained by Lupu and Pontusson (2011) do not allow us to determine which of the two explanations holds. Expressed in formal terms, the two alternative hypotheses are in part observationally equivalent, giving rise to identification problems.

The remainder of this section will therefore develop an identification strategy, leaving aside aspects where the two models share common ground (such as the role of voters' preferences in shaping public policy). Hence, it will focus on the nature of the link between earnings skew and redistribution. Recall that the economic variant of the social affinity hypothesis assumes a direct causal relationship that runs from skew to redistribution. By contrast, the alternative model claims that earnings skew is the result of policy efforts meant to achieve greater equity. To support the alternative interpretation, this section first seeks to provide evidence for the endogeneity of skew, using the example of minimum wages. Secondly, it replicates the original work by Lupu and Pontusson (2011) to test whether their findings hold when controlling for endogeneity.

3.2. Minimum wages as a proximate cause of earnings skew

When investigating the effect of minimum wages on the dispersion of earnings, it is useful to start by asking how the earnings distribution would look like in the absence of labor market institutions (i.e. to develop a counterfactual). Fortunately, traditional labor economics can guide theoretical expectations. A commonly held assumption is that the wage distribution is generated by Gibrat's (1931) law of proportionate effect, resulting in a lognormal distribution of earnings (see Mayer, 1960; Balintfy and Goodman, 1973; Sutton, 1997). By the 1960s, this insight had been widely accepted and researchers turned to the finer points of detail, such as whether or not the extreme upper tail is better approximated by a Pareto distribution (see Harrison, 1979). Thatcher (1976, p.

[5] Note the parallel to the notion developed by, *inter alia*, by Iversen and Soskice (2009) that level of inequality and redistribution are co-determined.

233) provided a time series for the United Kingdom, showing that the D9/D5 ratio had been approximately equal to the D5/D1 ratio ever since 1886. The implied skew ratio of unity is a natural consequence of a lognormal distribution.[6] In other words, according to traditional labor economics, skew should not exist.

However, skew clearly does exist. As argued above, labor market institutions hold a plausible explanation. Minimum wages are a self-evident example, and for lack of space the discussion focuses on them (leaving aside the more complex impact of collective bargaining). Despite sharp disagreements over the damaging or beneficial effects of minimum wages (see Card and Krueger, 1995; Neumark and Wascher, 2006), the arguments made on both sides of the divide imply that higher minimum wages should lead to greater skew. The first strand of the literature has focused on the wage effects of statutory minimum wages, by-and-large confirming that they achieve their stated objective and raise the wages of low-paid workers (see e.g. Metcalf, 2008; Dube *et al*., 2010; Autor *et al.*, 2016). The second major strand of the literature has concentrated on employment effects and (controversially) claimed that minimum wages price workers with low productivity out of the market (Brown *et al*., 1982; cf. Metcalf, 2008; cf. Schmitt, 2013). Therefore, regardless of which position one takes, minimum wages should disproportionately increase the earnings at D1: either by lifting the wages at the bottom, or by truncating the distribution through the displacement of low-productivity workers. Kernel density plots typically show a clustering of wages at or just above the minimum wage, resulting in a characteristic departure from the lognormal pattern (see DiNardo *et al*., 1996; Rycx and Kampelmann, 2012). Although minimum wages can spill over to workers with higher wages, the effect is unlikely to reach the median (Lopresti and Mumford, 2016). Note that the impact of minimum wages on skew is instantaneous: employers have to pay wages in compliance with the current minimum wage legislation, not a prior year’s.

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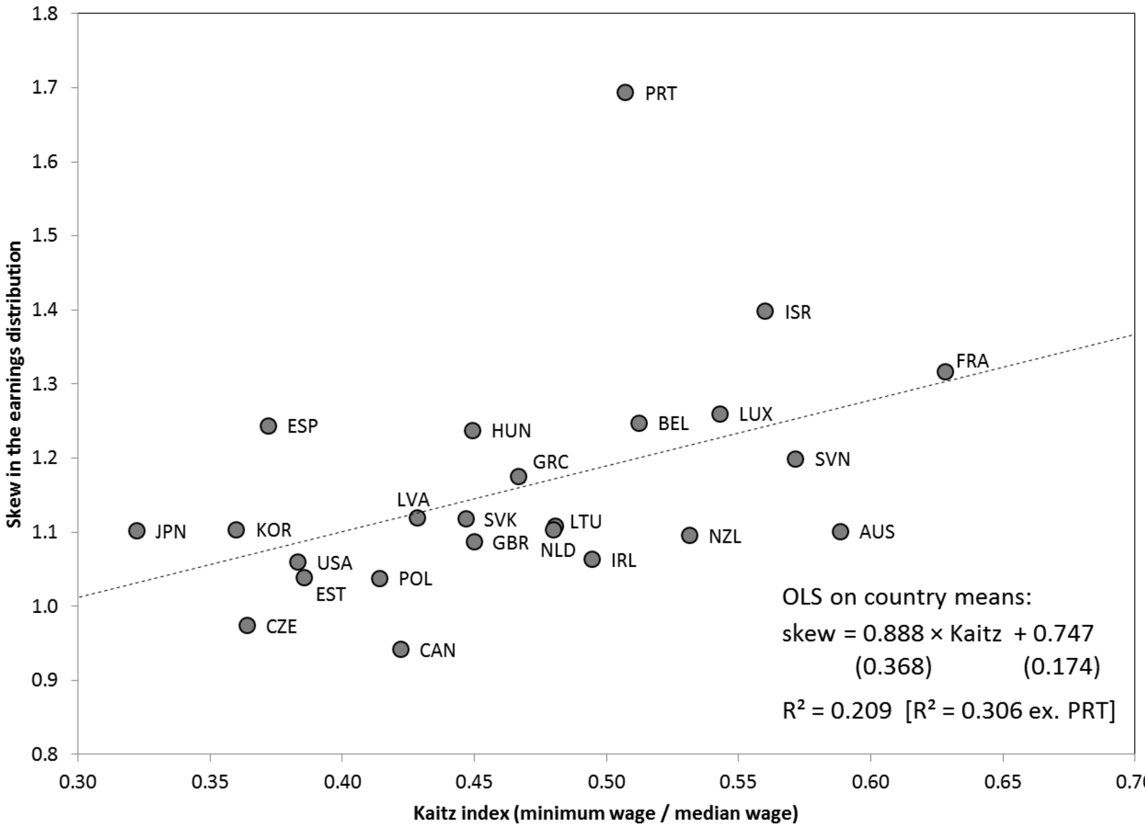
[6] For a non-technical proof, consider that when the *x*-axis is in logarithmic form, the earnings distribution resembles the normal curve. D1 and D9 are then at equal distance from the median (D5), regardless of shape and location parameters. Raised to the base of *e*, the distance on the logarithmic scale corresponds to ratios in the non-logarithmic world. Hence, the upper and the lower decile ratios should be identical.

Figure 3 The Kaitz index and skew in the earnings distribution (country means)



Note: Refers to country means, based on all years where a non-zero Kaitz index and data on earnings skew are available. Country abbreviations correspond to ISO 3166.
Source: OECD and ILO (see Online Appendix, Table A11).

Based on an updated data-set that primarily draws on the OECD's database,[7] Figure 3 confirms that countries with a higher Kaitz index – minimum wages expressed as a fraction of median wages – typically also display higher skew in the earnings distribution. On the lower left-hand side, the United States, the Czech Republic, Korea and Japan have low minimum wages and exhibit low skew; at the other extreme, France, Israel and Luxembourg set higher minimum wages and reach higher levels of skew. With just over 20 % of the cross-national variance explained, the effect of minimum wages is relatively modest (the R^2 rises to 0.306 when the outlier Portugal is excluded). However, keep in mind that these countries differ in a myriad of other ways, and that finding a perfect relationship would be highly unusual. The explanatory power increases when control variables are added in a more complete between-effects model (see Online Appendix Table A1).

[7] The dataset covers 24 of the 28 EU member states (the exceptions are Bulgaria, Croatia, Cyprus and Malta), as well as Australia, Canada, Iceland, Israel, Japan, Korea, New Zealand, Norway, Switzerland, Taiwan and the United States. See the Online Appendix for descriptive statistics (Table A9) and variable definitions and sources (Table A11).

Table 1 Explaining skew in the earnings distribution with the Kaitz index and labor market institutions

	(1)	(2)	(3)	(4)
	Skew (earnings)		Skew (earnings)	
Kaitz index	0.590***	0.597**	0.642***	0.577**
	(0.152)	(0.215)	(0.136)	(0.207)
No minimum wage (dummy)		0.420*		0.395*
		(0.204)		(0.185)
Unemployment rate	-0.315*	-0.109	-0.203	0.0458
	(0.145)	(0.138)	(0.213)	(0.210)
Female labor force participation	0.604+	-0.192	0.789**	-0.399
	(0.313)	(0.412)	(0.244)	(0.484)
Employment protection legislation	0.0864***	0.101***	0.0615*	0.0925*
	(0.0147)	(0.0196)	(0.0236)	(0.0364)
Vocational training			0.0582	0.0519
			(0.103)	(0.0847)
Trade union density			-0.201	-0.0471
			(0.129)	(0.174)
Constant	0.299	0.649*	0.289+	0.824**
	(0.217)	(0.267)	(0.149)	(0.250)
n =	329	496	271	427
Countries	24	33	22	30
R^2 (within)	0.601	0.352	0.604	0.322
Model	2-way fixed-effects with cluster robust standard errors (xtreg i.year, fe vce(r))			

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Refers to observations from 1985 onwards due to the limited availability of the OECD's EPL indicator. Models (1) and (3) only include observations where a statutory minimum wage was in place. In Models (2) and (4), observations where no general statutory minimum wage was in place enter with a Kaitz index of 0 and a dummy "No minimum wage" with the value of 1.

Source: See Online Appendix (Table A11).

Table 1 exploits the within-country variation of skew and minimum wages in a two-way fixed effects model.[8] It controls for unobserved unit heterogeneity (i.e. differences between countries that are not adequately modelled) through country-fixed effects. By adding year dummies, the model also controls for changes that have occurred over time and might have affected skew across countries, but are not explicitly measured (such as the ups and downs of globalization or technical progress; see Acemoglu, 1998).[9] Model 1 starts with a sub-sample of those observations where a statutory minimum wage was in fact in place. As expected, it shows that the Kaitz index has a positive and highly significant impact on skew. To test for the impact of other labor market policies, the OECD's measure for Employment Protection Legislation (EPL) is introduced as a proxy (setting aside methodological doubts; see Bertola *et al*., 2000). The significant and positive

[8] Sargan-Hansen statistics indicate that the more efficient GLS random-effects model is not appropriate.

[9] Since the social affinity hypothesis was (at least implicitly) generated for developed countries, the analysis is limited to developed countries. Given the difficulty to compute a meaningful Kaitz index under a system of sectoral minimum wages, observations for the United Kingdom up to 1992 are dropped. Following Lupu and Pontusson (2011, p. 320), all available observations for the dependent variable "skew" are included, regardless of minor definitional differences. As long as measurement error is random, it only inflates the error term and hence works against finding significant effects. For descriptive statistics see Online Appendix Table A9.

coefficient suggests that other statutory forms of regulation affect the lower portion of the earnings distribution in the same direction as minimum wages. To control for labor market conditions, the unemployment rate and female labor force participation are introduced (in line with the approach in Lupu and Pontusson), leading to (marginally) significant coefficients.[10] Between all variables, the proportion of explained variance is just over 60 %, a good performance for a parsimonious model.

Unsurprisingly, the explanatory power of the model is reduced when countries that do not set a statutory minimum wage are added, as done in Model 2 ($R^2 = 0.352$). These observations are predominantly from the Nordic countries, Austria, Italy and from Germany prior to 2015.[11] For lack of data on the effective wage floor, they enter the analysis with a Kaitz index of zero – even though these countries have close functional equivalents in the form of (more or less comprehensive) sectoral minima established through sector-level collective bargaining.[12] These arrangements are not modelled in detail, but captured with the help of dummy variable that takes the value of one where a statutory minimum wage is absent. The significant coefficient suggests that non-statutory wage floors indeed matter.[13] More importantly, the inclusion of additional cases does not affect the regression coefficients on the statutory minimum wage and EPL, which remain highly significant.

As argued above, minimum wages are but one example of labor market institutions that influence the earnings distribution. It is tempting to explore other possible causes of skew, such as vocational training systems (Thelen, 2004), public spending on education (Iversen and Stephens, 2008) or the level of collective bargaining (Wallerstein, 1999). However, given the narrow scope of this section, the analysis limits itself to two further variables to check the robustness of results: the percentage of students in secondary education enrolled in vocational programs (TVET) and the trade union density rate. Both are familiar to readers from the analysis in Lupu and Pontusson (2011, pp. 325ff.). As evident from the insignificant regression coefficients in Model 3 and 4, neither of them helps us to predict within-country variations in skew.[14] Arguably, this result does not point to their irrelevance, but has a more sanguine interpretation. Namely, the effects of vocational training or unionization are typically only observable in the long term (as graduates enter

[10] The sole rationale for using female (rather than male or total) labor force participation throughout this paper is to stay in line with the design in Lupu and Pontusson (2011, p. 325).
[11] A smaller number of observations are from Ireland prior to 2000, from the United Kingdom during the period of 1993 to 1998, and from Switzerland.
[12] See Neumark and Wascher (2004, Table 1) for estimates of the effective Kaitz indices for these countries, based on a comparison of negotiated minima and mean wages. In the early 1990s, they ranged from 0.51 (Sweden) to 0.71 (Italy), far exceeding the statutory minimum wages in the United States (0.36).
[13] Note that the FE models refer to within-country changes, i.e. the introduction of a statutory minimum wage in Ireland, Korea and the United Kingdom. The coefficient in the corresponding between-effects model is marginally significant only when controls are added (Online Appendix Table A1).
[14] The coefficient on vocational training is (marginally) significant in the BE model, but carries a negative sign. This would – contrary to expectations – indicate that higher levels of vocational enrollment do lead to greater lower-half wage dispersion (see Online Appendix Table A1).

employment, or unions leverage membership to bargain over wages).[15] Further, their effects might be indirect and conditional on the presence of other institutions, requiring a more complex model that allows for these interactions (see Rueda and Pontusson, 2000). And while training systems and unionization may well have a disproportionate effect low wages, their reach extends to the median and beyond (Kristal and Cohen, 2017, p. 207). This distinguishes them from the minimum wage, which has a direct and very selective effect on the lower end of the wage distribution (see DiNardo *et al.*, 1996). And unlike the long-run determinants, adjustments in the level of the minimum wage have a more or less instantaneous impact on the lowest wages actually paid.

This explains why adding TVET and unionization to a regression (as in Lupu and Pontusson, 2011, pp. 322ff.) is not an effective strategy to control for the endogeneity of skew. By contrast, minimum wages stand out as a particularly powerful predictor of earnings skew. Changes in legislation, as approximated by the OECD's EPL indicator, seem to have similar short-run effects on skew (although the causal channels have remained unexplored here; for details see Koeniger *et al*., 2007).[16]

3.3. Controlling for endogeneity: Replication of key findings

The finding that governments produce earnings skew by setting minimum wages provides leverage to solve the identification problem discussed above. Recall that under the structure of inequality model, skew shapes social coalitions, the policy preferences of the middle class and ultimately the composition of governments. Importantly, the causal mechanism works independently of the existence or level of minimum wages. Hence, the effect of skew on partisanship (and, in a second step, on redistribution) should remain intact when minimum wages are added as a control variable (alongside the existing controls unionization and vocational training). By contrast, the alternative hypothesis states that earnings skew and redistribution are co-determined. As outlined in Figure 2, the causal link that runs from partisanship to skew is mediated by minimum wages (and other labor market institutions). Hence, when minimum wages are entered as a control variable, they should absorb much of the non-causal association between the two variables of interest. Likewise, following Morgan and Winship (2015, Ch. 4), conditioning on minimum wages should block the backdoor path between earnings skew and redistribution and control for confounding effects.

[15] Note that the coefficient on TVET enrollment is (marginally) significant in a between-effects model, i.e. a regression on country means (not reported). However, it carries an unexpected sign, suggesting that higher TVET enrollment is associated with less skew (rather than more lower-half wage compression). As noted above, the coefficients on the Kaitz index and EPL remain significant and carry their expected sign.

[16] As a further robustness test, all models were re-run with a number of alternative model specifications (not reported). These included a between-effects model and PCSE models with an AR1 process and either a LDV or FE. In all cases, the coefficients on the Kaitz index remained significant (with particularly high significance levels in the PCSE models).

Table 2 Replication of "Determinants of government partisanship" with minimum wages as an additional control variable

	(5)	(6)	(7)	(8)	(9)	(10)
		Partisanship			Partisanship	
Skew (earnings)	-0.237+ (0.126)	-0.294* (0.143)	-0.522** (0.186)	0.211+ (0.114)	0.00271 (0.130)	-0.0938 (0.169)
Proportionality	-0.0550 (0.0512)	-0.00723 (0.0557)	-0.0203 (0.0656)	0.118 (0.0734)	0.154* (0.0676)	0.135* (0.0679)
Voter turnout	-0.00111 (0.000813)	-0.000842 (0.000764)	0.000760 (0.000862)	0.00294*** (0.000624)	0.00185*** (0.000557)	0.00209** (0.000701)
Globalization		-0.00852*** (0.000782)	-0.00841*** (0.00166)		-0.00631*** (0.00105)	-0.00673*** (0.00174)
Immigration			-0.00303 (0.00318)			0.000979 (0.00201)
Kaitz index				-1.233*** (0.121)	-0.725*** (0.162)	-0.612*** (0.186)
No minimum wage (dummy)				-0.753*** (0.0590)	-0.472*** (0.0837)	-0.412*** (0.0929)
Constant	0.771*** (0.150)	1.410*** (0.162)	1.560*** (0.197)	0.496*** (0.132)	0.988*** (0.143)	1.056*** (0.186)
n =	312	312	238	284	284	229
Countries	18	18	18	18	18	18
R^2	0.025	0.193	0.163	0.224	0.296	0.222
Model	Regression with panel-corrected standard errors (xtpcse, pairwise)					

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Models 5 to 7 replicate Models 21 to 23 in Table 5 of Lupu and Pontusson (2011, p. 331). For consistency, the additional control variables (Kaitz index, minimum wage dummy) in Models 8 to 10 were subjected to the same data treatment as the other explanatory variables. Only observations from 1980 onwards are included.

Source: Lupu and Pontusson (2011); OECD, ILO and ICTWSS (see Online Appendix, Table A11).

Table 2 replicates the analysis of the "Determinants of government partisanship" in Lupu and Pontusson (2011, p. 331). Models 5 to 7 reproduce the original analysis that supported the tentative conclusion that "there is some evidence that skewed earning inequality promotes left participation in government" (*ibid*.). Models 8 to 10 then introduce the Kaitz index as a control variable, as well as a dummy for countries that do not set a statutory minimum wage (and hence enter with a Kaitz index of zero). The result is unambiguous: in Model 8, the coefficient on skew carries an unexpected sign and is marginally significant; it becomes insignificant in Models 9 and 10. There also is a highly significant association between minimum wages and government partisanship. Of course, this cannot be interpreted within the conventional logic of the regression framework: minimum wages are *not* a cause of partisanship; the minimum wage variables only serve as controls.[17]

These findings no longer offer any support for a crucial transmission mechanism of the social affinity hypothesis, namely that skew determines government partisanship. Hence, they also

[17] The direction of causality runs from partisanship to minimum wages. Note that the negative sign of the coefficients implies that right-leaning governments set lower minimum wages than their left-leaning counterparts, in line with Pontusson *et al*. (2002, p. 292).

put the causal interpretation of the association between skew and redistribution itself into doubt. However, before testing the robustness of the redistribution and social spending models, two notes of caution are in order: (1) Although the analysis above has provided sufficient evidence for endogeneity, minimum wages are not the only way through which governments and labor market institutions affect skew (see Rueda and Pontusson, 2000; Pontusson *et al.*, 2002). Hence, controlling for minimum wages alone may be insufficient to remove omitted variable bias, leaving residual confounding in place. (2) Including both skew and its proximate causes (namely minimum wages and EPL) on the right-hand side of a regression equation is a recipe for multicollinearity (Farrar and Glauber, 1967). Under normal circumstances, the advice would be to remove skew from the regression (which, however, would defeat the purpose here).

Table 3 Replication of "Determinants of redistribution and social spending with government partisanship" with minimum wages as an additional control variable

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Redistribution				Non-elderly public social expenditure			
Partisanship	0.974 (2.045)	-0.707 (1.633)	4.980* (2.119)	4.489** (1.713)	-0.405 (0.285)	-0.309* (0.151	0.034 (0.337)	0.0908 (0.151)
Skew (earnings)	9.742* (4.728)	14.08*** (3.238)	6.785+ (3.545)	1.373 (2.332)	2.647*** (0.800)	1.846*** (0.444)	0.705 (0.832)	0.465 (0.518)
D9/D1 ratio (earnings)	-0.00314 (1.344)	-0.266 (0.963)	1.177 (1.032)	1.041 (1.181)	0.414* (0.209)	0.263* (0.115)	0.615** (0.206)	0.526*** (0.131)
Kaitz index			19.02** (5.87)	25.65*** (5.743)			4.299** (1.393)	3.525*** (0.843)
No minimum wage (dummy)			3.265 (2.671)	5.541* (2.469)			2.268*** (0.587)	1.760*** (0.382)
n =	60	50	58	51	241	217	232	212
Countries	14	14	14	14	18	18	18	17
R^2	0.889	0.961	0.945	0.975	0.993	0.996	0.991	0.996
Model	Regression with LDV, PCSE and common AR1 process (xtpcse, pairwise cor(ar1))				Regression with LDV, PCSE and panel-specific AR1 process (xtpcse, pairwise cor(psar1))			

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Models 11 to 12 and 15 to 16 replicate Models 27 to 30 in Table 6 of Lupu and Pontusson (2011, p. 331). For consistency, the additional control variables (Kaitz index, minimum wage dummy) in Models 13 to 14 and 17 to 18 were subjected to the same data treatment as all other explanatory variables. Following the original publication, coefficients on the other control variables are not reported. In the redistribution models, the order of observations serves as the pseudo-time variable. Due to missing data for the United Kingdom prior to 1993, the replications in Model 13 and Model 17 are missing two and nine cases, respectively. In the original publication, the negative sign on two coefficients was omitted in Model 12. Models with odd numbers include all cases, those with even numbers drop outliers.

Source: Lupu and Pontusson (2011); OECD, ILO and ICTWSS (see Online Appendix Table A11).

With these caveats in mind, Table 3 reproduces the results for the concise models for the "Determinants of redistribution and social spending with government partisanship" (Lupu and Pontusson, 2011, p. 331). Models 11 and 12 present the original analysis for redistribution, first for the full data-set and then excluding outliers. When the Kaitz index is entered as a control variable in Model 13, the association between skew and redistribution remains marginally significant.

However, the coefficient on skew becomes insignificant once outliers are dropped in Model 14. Likewise, when using social spending as the dependent variable in Models 15 to 18, the coefficients on skew lose their significance once the minimum wage is introduced an additional control. Across specifications, there is a highly significant association between minimum wages and the dependent variable. Again, it would be misleading to say that minimum wages can "explain" redistribution or social spending. The coefficients simply indicate that governments which set higher minimum wages also tend to engage in more redistribution and have higher levels of social spending, in line with the idea that the earnings distribution and redistribution are jointly determined.

Similar, but less straightforward effects emerge when replicating Tables 2 and 3 of the original analysis (Lupu and Pontusson, 2011, pp. 325ff.). The inclusion of minimum wages generally weakens the predictive power of skew, but does not always render coefficients insignificant (see Online Appendix, Tables A2 to A5). Given that minimum wages are not the only mediating variable between government policy and skew, an additional set of robustness checks also enters EPL and replaces the D9/D1 ratio (which measures overall wage inequality) with the D9/D5 ratio (to measure the part of the wage distribution largely unaffected by minimum wages). Now, the coefficients on skew become insignificant in each and every single model (with standard errors often approaching or outstripping the size of the coefficients, and signs frequently reversing). As expected, regression diagnostics indicate the presence of multicollinearity, with particularly high variance inflation factors for skew. The results should therefore not be interpreted in any substantive way, beyond demonstrating that the effect of skew on redistribution and social spending is not robust.

In sum, the findings of this section leave little doubt that endogeneity is a serious constraint of the earnings data, and that the results obtained by Lupu and Pontusson (2011) are affected by omitted variable bias.

4. Measurement validity: Earnings, incomes and distributional conflict

A curious and generally overlooked aspect of the paper by Lupu and Pontusson is that it develops a theory that explicitly refers to the structure of income inequality, but then tests its predictions on data for skew in the distribution of earnings. The use of earnings data is not uncommon in comparative political economy (see Iversen and Soskice, 2009), and often a pragmatic choice driven by data availability. While "earnings" and "incomes" are often used synonymously in popular discourse, the two concepts differ in very significant ways: the OECD's earnings data refer to the distribution of labor incomes among individuals in full-time employment; income inequality refers to the distribution of income from all sources among households (usually adjusting and weighting for household size). Although the dispersion of earnings should influence the distribution of household incomes, so do the distribution of capital income, the distribution of working hours and unemployment between individuals, and the sorting of high- and low-wage earners across households (Blau and Kahn, 2011, p. 179; Huber and Stephens, 2014).[18] In fact, the link is so complex that the "two strands of study, of wage dispersion on the one hand and household income distribution on the other, are miles apart" (Salverda and Checchi, 2015, p. 1537).

What should be clear from this brief discussion is that the structure of earnings inequality does not map one-to-one into the structure of income inequality, raising questions about measurement validity (Adock and Collier, 2001). Hence, it is doubtful whether earnings data can capture the theoretical concept of interest, relative income distances. Claiming that a key concept has been inadequately measured usually serves to undermine the conclusions of a paper. However, in this case, it has the opposite effect: if the social affinity hypothesis should not have been tested against earnings skew in the first place, it does not fall in light of the results obtained above. This section argues that the rather technical distinction between earnings and incomes is in fact relevant for the political dynamics of redistribution. It aims to show that distributional conflict is not exclusively a within-group conflict among wage workers, but that significant redistributive transfers occur between those who are in employment and those who are not.[19] It then tests whether skew in the earnings distribution and skew in the income distribution approximate each other.

4.1. Labor market segmentation and between-group redistribution

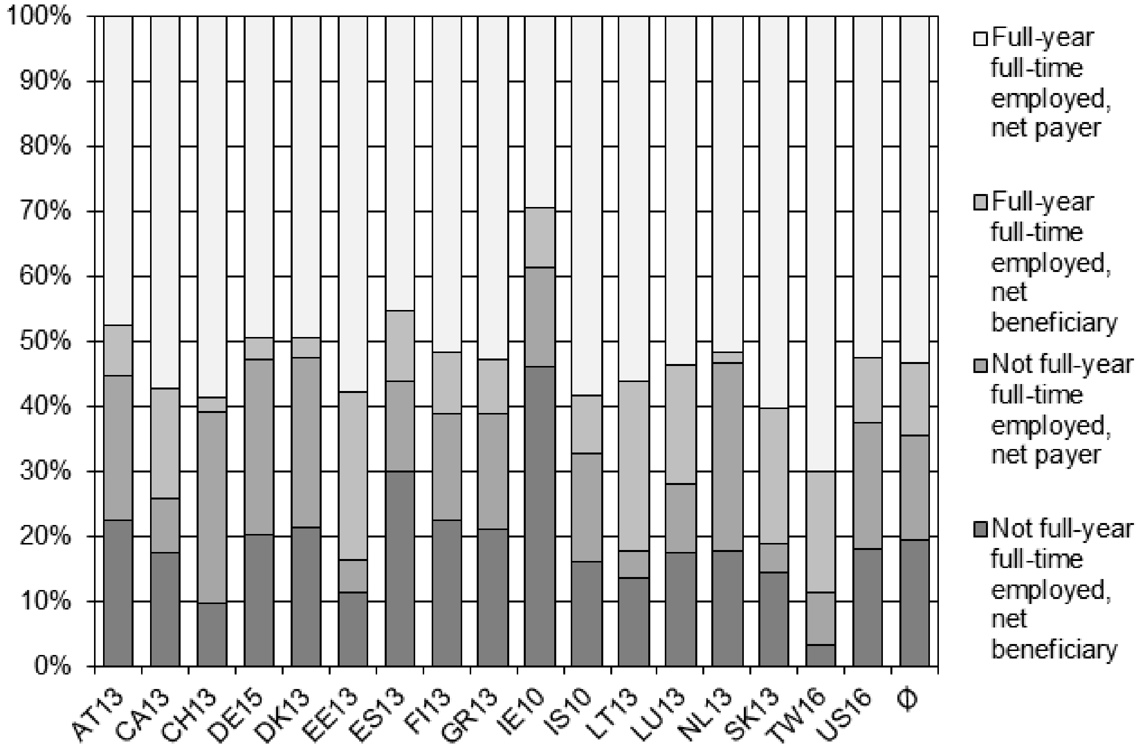
One need not fully subscribe to Standing's (2011) dystopian views of an emerging precariat to appreciate that the exclusion of a large section of the population from stable, long-term employment should give rise to exactly the kind of fractionalization among economic lines that Lupu and Pontusson (2011) describe. Where the poor are long-term unemployed or in precarious employment, this should increase their social distance vis-à-vis the middle class which, for the larger part, still has access to stable employment (see also Pontusson and Weisstanner, 2017, pp. 17f.). Entrenched welfare-dependence among the poor could also undermine the idea of reciprocity that gives the middle class a stake in social insurance systems (Korpi and Palme, 1998; Mau, 2004). Indeed, Alt and Iversen (2017) develop an alternative model where greater labor market segmentation leads to an asymmetric distribution of risk, and by extension to less support for redistribution among middle-income voters. But even if one accepts social distance as the causal mechanism that shapes group allegiances, it is plausible that the patterns of social identities (to use Shayo's term) cannot be deducted solely from the distribution of wages. With the days of full

[18] See also the comparison in Kenworthy and Pontusson (2005, p. 452), who argue that the earnings data "fail to capture the distributive effects of unemployment, underemployment, and labor force exit".

[19] Due to data constraints and limitations of space, this section does not model live-cycle effects and the insurance function of unemployment benefits.

employment and the single breadwinner long gone, we need to look at incomes in the context of households (see also Häusermann *et al*., 2016).

Figure 4 Net payers and net winners of fiscal redistribution by status of household head in full-time full-year employment, 17 countries (2010-16)



Note: Employment status refers to the household head; only households headed by an individual aged 25 to 59 years. Net payers are households where disposable income is less than or equal to market income; net beneficiaries are households where disposable income exceeds market income. Market incomes include private transfers received. Weighted by household size. Country abbreviations are two-letter codes (ISO 3166); the numbers refer to the income reference year.

Source: Own tabulation, based on Luxembourg Income Study (LIS) Database (2018 Summer Data Release).

The proposition that between-group redistributional transfers matter can be put to an empirical test. Based on data from the Luxembourg Income Study (LIS, 2018), Figure 4 distinguishes between households headed by somebody in full-year full-time employment (FYFT) and the remaining households, restricting observations to households with heads in the typical working-age bracket (keeping in line with the measure for redistribution in Lupu and Pontusson, 2011). It then identifies net payers and net beneficiaries from fiscal redistribution, according to whether disposable household incomes are lower (net payers) or higher (net beneficiaries) than market incomes. Despite this somewhat crude operationalization (which is necessitated by the structure of the data),[20] the main finding is clear-cut: More than three-quarters of the net payers are found in

[20] The Luxembourg Income Study measures employment status at the level of individuals, but incomes at the level of households. This means that additional household members might have an employment status that differs from that of the household head, limiting the extent of redistributive flows between the two groups.

households with a fully employed head, but almost two-thirds of the net beneficiaries live in households with a head who lacks full-year full-time employment. Moreover, the redistributive flows between groups are substantial: Averaging across all 17 countries, the tax and transfer system reduces the incomes of households with a head in FYFT by almost a quarter, whereas the remaining households are better off after taxes and transfers (not tabulated).

In sum, there are strong indications for between-group distributional conflict along the lines of employment status (with the potential exception of Taiwan, where full employment is still the norm). The OECD's earnings data map within-group inequality among those in full-time employment, and hence cannot capture this conflict. All of this underlines that Lupu and Pontusson (2011, p. 318) have good reasons to refer to the structure of income inequality, rather than relative earnings differentials, when extending the concept of social affinity to the economic domain.

4.2. Is earnings skew a valid proxy for income skew?

Can skew in the earnings distribution at least serve as a proxy for skew in the distribution of household incomes? Again, the question is best answered empirically. The LIS database can be used to measure relative income distances between the poor, the middle class and the affluent. However, because those at the 10th percentile often have zero market incomes, the computation of skew ratios cannot be transferred one-to-one. Instead, the poor are defined as those at the 25th percentile and, equivalently, the affluent as those at the 75th percentile.[21] Keeping in line with the measure for skew suggested in Lupu and Pontusson (2011, p. 334), and following them in restricting observations to the working-age population, this allows calculating income skew as the ratio of the upper and lower quartile ratios, or as (P75/P50)/(P50/P25).

The data show substantial variation for income-based skew both within and between countries (see Online Appendix, Table A10), but there is no evidence for a systematic relationship between the two measures for skew: The correlation between the income-based measure of skew and the measure for earning skew used in Lupu and Pontusson (2011) is in fact negative, with a Pearson's r of -0.187 (p-value: 0.102, n = 78). When compared to earnings skew calculated from the current version of the OECD database, the relationship becomes insignificant (Pearson's r = -0.058, p-value: 0.529, n = 121). While it holds that countries with greater overall earnings dispersion also display higher levels of income inequality, this regularity does not extend to the structure of inequality.[22] We therefore cannot substitute one measure of skew for the other.

[21] This is done using standard LIS routines with respect to top-coding, bottom-coding, and equivalence scale (namely dividing household incomes by the square root of the number of household members). The income concept is market incomes, calculated as factor incomes plus private transfers received.

[22] The D9/D1-ratio and the Gini coefficient for market incomes (among households with a head in the age bracket from 25 to 59 years) correlate with a Pearson's r of 0.503 (p < 0.001, n = 135). Expressed in technical terms, departures from

5. Redistribution and the structure of income inequality

The findings of the two preceding sections can be condensed into two arguments against using earnings skew to test the economic variant of the social affinity hypothesis: Firstly, there are practical obstacles to obtaining valid coefficient estimates in the presence of endogeneity. Secondly, earnings data are not a valid measure for the concept of theoretical interest, and do not capture between-group distributional conflict. A natural response to this double quandary is to perform a fresh hypothesis test, based on skew in the income distribution. This should not only improve measurement validity, but potentially also help to reduce endogeneity.

5.1. Main variables of interest and model specification

This section therefore revisits the relationship between skew and redistribution, using skew in the distribution of incomes as the main explanatory variable. As in the original paper, redistribution is defined as the relative difference between the Gini coefficient for market and disposable incomes for households with heads aged between 25 and 59 years. Owing to the substantial expansion of the LIS database in recent years, the number of observation more than doubles from 87 to 192 country-years.[23] The measure for skew in the distribution of market incomes is derived from the same source, as outlined in the preceding section. Details on sources and definitions for other explanatory variables in the updated data-set can be found in the Online Appendix (Table A11). In all cases, the intention was to stay as close as possible to the definitions and sources used in Lupu and Pontusson (2011).[24] Using a harmonized source for the main explanatory variable (skew) also addresses the concerns regarding definitional differences raised in Lupu and Pontusson (2011, p. 320). By construction, the main dependent and independent variables are available for the same countries and years, doing away with the need to interpolate and extrapolate them.

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lognormality in the earnings distribution (as captured by skew) do not imply similar departures from lognormality in the income distribution, while dispersion parameters (that measure the level of inequality) share communalities.

[23] The new and the old data on redistribution match almost exactly, with Pearson's $r = 0.988$ ($p < 0.001$). In line with standard practice (Mahler and Jesuit, 2006, p. 487), only so-called *gross* LIS data-sets are used. Compared to Lupu and Pontusson (2011), this leads to the exclusion of three observations each from Spain and Ireland (all coded as *net* by LIS), and four observations from France (classified as *mixed*). The sample is limited to the EU and other developed countries and includes observations from Australia, Austria, Belgium, Canada, the Czech Republic, Denmark, Estonia, Finland, Germany, Greece, Iceland, Ireland, Israel, Japan, Korea, Lithuania, Luxembourg, the Netherlands, Norway, Romania, the Slovak Republic, Spain, Sweden, Switzerland, Taiwan, the United Kingdom and the United States. For details and descriptive statistics see Online Appendix Table A10 and A11.

[24] The correlation for the new and old data on non-elderly public social expenditure is Pearson's $r = 0.967$. For the control variables, the correlation coefficients exceed 0.95 in seven out of nine cases, with weaker correlations for vocational training ($r = 0.829$) and the share of the population aged 65 years and above ($r = 0.858$) (all significant at the 0.001-level).

Table 4 Influence of cyclical factors and labor market institutions on skew in the income distribution

	(19)	(20) Skew (incomes)	(21)
Unemployment rate	-0.391** (0.128)	-0.431** (0.122)	-0.483*** (0.125)
Female labor force participation	-0.302** (0.102)	-0.513*** (0.0931)	-0.514*** (0.103)
Kaitz index		0.120 (0.172)	0.164 (0.203)
No minimum wage (dummy)		-0.0130 (0.108)	-0.0130 (0.111)
Employment protection legislation		-0.0310 (0.0188)	-0.0374 (0.0219)
Vocational training			-0.0896 (0.0806)
Trade union density			0.223 (0.239)
Constant	1.139*** (0.0417)	1.307*** (0.111)	1.284*** (0.193)
n =	192	138	124
Countries	27	24	24
R^2 (within)	0.677	0.687	0.659
Model	2-way fixed-effects with cluster robust standard errors (xtreg i.year, fe vce(r))		

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.
Note: Coefficients on the year dummies are not reported.
Source: See Online Appendix Table A11.

None of this, however, precludes the existence of endogeneity. Table 4 therefore repeats the earlier analysis of the determinants of skew for the income-based measure.[25] Models 19 to 21 use two-way fixed effects and show that within-country variation in skew is largely a function of cyclical fluctuations in unemployment and female labor force participation. Because both variables are also likely to influence redistribution, this provides a strong rationale to carry them over as controls (see also Pontusson and Weisstanner, 2017, pp. 6 and 15f.). By contrast, the two policy variables – minimum wages and EPL – exert no discernible influence within countries (Model 20). Likewise, unionization and enrollment in vocational training do not have significant effects (Model 21). In the absence of strong evidence that income skew is a direct outcome of government policy, this section follows Lupu and Pontusson (2011, p. 332) and treats skew as exogenous.

With the variables defined, the perennial question regarding model specification arises. There are natural advantages to staying in line with the original specification in Lupu and Pontusson (2011). The analysis therefore starts with their preferred PCSE model with an AR1 error process. It is either combined with a lagged dependent variable (LDV) and a full set of controls (their Model 5)

[25] Again, Sargan-Hansen statistics indicate that a more efficient GLS random-effects model is not appropriate.

or country fixed effects (FE) and a reduced set of control variables (their Model 7). Following the original design, the FE are generated by manually adding country dummies; all independent variables are subjected to the same data treatment as in Lupu and Pontusson (2011, p. 324). Arguably, these specifications have drawbacks. The most obvious is that the data-set is an unbalanced panel with unevenly spaced observations. Hence, the order of observations has to be used as a pseudo-time variable.[26] This implies that the first-order autoregressive processes may in fact be anything between an AR1 and an AR10 process (as in the case of the first and second observation from Switzerland, which are ten years apart). Likewise, the LDV may refer to the preceding year or an observation a decade old. This makes the assumption of a constant coefficient on the LDV less than obvious. Further, the PCSE design cannot leverage its strength and correct for contemporaneous correlation of error terms across units when observations with the same pseudo-time code are, in fact, not contemporaneous.[27] An LDV design can also open up backdoor paths that produce non-causal correlations between independent and dependent variables (Morgan and Winship, 2015, p. 111). As a more practical concern, data treatment and the LDV reduce the number of cases and hence have costs in terms of efficiency.

In addition to the PCSE models, this section therefore also runs two-way fixed-effects and between-effects regressions.[28] While the country fixed effects control for unobserved unit heterogeneity, the year-dummies control for unobserved shocks common to all countries that might have influenced dependent and independent variables. As in the preceding sections, the fixed-effects model is combined with cluster-robust standard errors to address heteroscedasticity and serial correlation of error terms within panels (Rogers, 1993). In the case of the between-effects model (BE), serial correlation does not arise and a conventional White test is applied to detect heteroscedasticity. Given that between-country differences account for more than 80 % of the variation in redistribution within the data-set (not tabulated), the between-effects models are arguably at least as instructive as their fixed-effects counterparts. Both models use the same set of control variables as in the original analysis (substituting the measure for overall inequality).[29] In addition, given missing data, they are repeated with a reduced set of controls to utilize all cases.

[26] See the replication files for Lupu and Pontusson (2011).

[27] There is little merit in assuming that an exogenous shock that hit Canada in 1971 affected Australia 1981 and reached Belgium in 1992 (the first observations for these countries).

[28] Sargan-Hansen statistics indicate that a random effects model is not appropriate.

[29] Variables are consistently scaled as fractions.

5.2. Results: Income skew and fiscal redistribution

Table 5 presents the results. Unlike in the analysis in Lupu and Pontusson (2011, p. 325), there is no longer any evidence that skew has a positive effect on redistribution. In Model 22, that replicates the PCSE design with a LDV, the coefficient is now negative and significant at the 0.01-level. It keeps its unexpected sign, but loses significance when PCSE are combined with FE (Model 23). Likewise, the two-way fixed-effects models produce coefficients that are far from significance (see Models 24 and 25).[30] When comparing between countries, as done in the BE-Models 26 and 27, the coefficient on skew again becomes negative and significant (at the 0.05- and 0.01-level, respectively). In other words, they suggest – contrary to the predictions of the social affinity hypothesis – that greater relative proximity between the middle class and the poor coincides with less, rather than more redistribution (see also Dallinger, 2015, p. 744). In other words, it appears that redistributive government efforts are curtailed in countries where the "income distance between the middle and the poor is small relative to the income distance between the middle and the affluent" (to use the explanation for skew in Lupu and Pontusson, 2011, p. 316).[31]

It is a puzzling finding, and will prompt readers to ask why this is the case. Might it be that, in these countries, the affluent have not only captured a disproportionate share of incomes, but also acquired the political power to resist a pro-redistribution coalition between the middle and the poor? Or does the causal link run the other way, from more redistribution to less skew in the primary distribution of incomes? Might some other mechanism be at work? All of this is possible, and it is always tempting to speculate and assign a causal interpretation to a significant coefficient. To be abundantly clear, no such causal claims are made here. Recall that the data were approached with a different, one-sided question in mind: Is there evidence for a positive relationship between skew in the income distribution and fiscal redistribution, as postulated by the social affinity hypothesis? Table 5 provides a satisfactory and unambiguous answer to this question: "no".

Three main objections can be raised against this analysis. The first is that the relationship between skew and redistribution might be distorted by extraordinary swings in both variables during the Great Recession (see Pontusson and Weisstanner, 2017). To exclude this possibility, all models from Table 5 are re-run on observations prior to 2008. In all cases, the coefficient on skew is either insignificant or carries the "wrong" sign (see Online Appendix Table A6). A second plausible objection is that people perceive relative income distances based on disposable incomes (i.e. incomes after to redistribution).[32] Although market incomes are the appropriate income concept

[30] A one-way model (without year fixed effects) produces negative and insignificant coefficients on skew (not tabulated).

[31] Coefficients on skew remain negative or insignificant when observations are limited to those country-years already included by Lupu and Pontusson (2011).

[32] The author is grateful to Noam Lupu for pointing this out.

when examining the impact of inequality on redistribution (Milanovic, 2000), another robustness test uses skew in the distribution of disposable household incomes as the main explanatory variable. Again, the coefficients on skew are either insignificant or negative (see Online Appendix Table A7). And finally, the dependent variable of choice – fiscal redistribution – might obscure welfare state dynamics where social policy favors the middle class. Following Lupu and Pontusson (2011, p. 327), all models are re-run with non-elderly public social spending as an alternative dependent variable. Again, no support for the social affinity hypothesis emerges and the coefficient on skew remains insignificant throughout (Online Appendix Table A8).

Table 5 Explaining redistribution with skew in the income distribution

	(22)	(23)	(24)	(25)	(26)	(27)
	Redistribution		Redistribution		Redistribution	
Skew (incomes)	-0.109**	-0.116	0.0560	-0.0177	-0.979*	-1.645**
	(0.0366)	(0.132)	(0.168)	(0.165)	(0.423)	(0.455)
P75/P25 ratio (incomes)	-0.000714	-0.00881	0.0302*	0.0276	-0.0491	-0.121*
	(0.00841)	(0.0228)	(0.0133)	(0.0175)	(0.0546)	(0.0579)
Voter turnout	0.0740*		0.110		0.0605	
	(0.0351)		(0.0687)		(0.105)	
Proportionality	0.0500*		-0.0355		0.183*	
	(0.0214)		(0.0413)		(0.0846)	
Vocational training	-0.0208		-0.0309		0.137	
	(0.0242)		(0.112)		(0.124)	
Trade union density	0.0635+		0.0615		0.103	
	(0.0343)		(0.284)		(0.0869)	
Unemployment rate	-0.0523		0.321*	0.322*	0.177	0.0556
	(0.100)		(0.129)	(0.142)	(0.307)	(0.430)
Female labor force participation	-0.0205		-0.0750	-0.113	-0.194	0.0735
	(0.0546)		(0.273)	(0.216)	(0.207)	(0.217)
Lagged dependent variable	0.641***					
	(0.124)					
Constant	0.105*	0.380*	0.0196	0.116	1.107+	1.971**
	(0.0498)	(0.172)	(0.172)	(0.222)	(0.535)	(0.609)
n =	125	165	141	192	141	192
Countries	23	26	24	27	24	27
R^2	0.807	0.850	0.485	0.541	0.745	0.471
Model	PCSE with AR(1) and LDV or FE (xtpcse [l1.red | i.country], pairwise cor(ar1))		2-way fixed-effects with cluster robust standard errors (xtreg i.year, fe vce(r))		Between-effects (xtreg, be)	

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Models 22 and 23 correspond to the econometric specification in Lupu and Pontusson (2011, p. 325, Models 5 and 7).Coefficients on the country dummies in Model 23 and on the year dummies in Model 24 and 25 are not reported. For the BE Models 26 and 27, a White test indicates that the null hypothesis of homoscedasticity cannot be rejected (*p*-values: 0.404 and 0.642).

Source: Luxembourg Income Study, OECD and ILO, Comparative Political Data Set, UNESCO, ICTWSS (see Online Appendix Table A11).

Although they are not the main focus of the present paper, the results regarding the other variables deserve a brief discussion. Models 24 and 25 confirm that, within countries, upturns in unemployment are associated with an expansion of redistribution (as well as in non-elderly public social expenditure; see Online Appendix Table A8). This finding corresponds to the results obtained in Pontusson and Weisstanner (2017, pp. 15f.) and supports the idea that social insurance systems act as automatic stabilizers in times of crisis (see Dolls *et al*., 2012). By contrast, within-country changes in voter turnout, proportionality, vocational training or trade union density have no significant impact on redistribution. However, these results are sensitive to model specification: Model 22 (PCSE with LDV) suggests that both voter turnout and (marginally) trade union density contribute to redistribution, in line with power resource theory (Korpi, 1983). It is plausible that, in Models 24, the proportionality of electoral representation has no measurable impact on within-country changes in redistribution, given time-invariant features of electoral systems (see Gallagher, 1991). However, in Model 22 (LDV) and Model 26 (BE), the positive and significant coefficient lends support to the argument that proportional representation is more conducive towards redistributive policies than majoritarian electoral systems (see Iversen and Soskice, 2006).

6. Conclusion: Time to bid farewell to a beautiful theory?

One of the most innovative recent contributions to the literature on the politics of redistribution is the hypothesis that the structure of inequality – and not its level – can explain variations in redistribution across time and countries. Applying the notion of social distance to the structure of the income distribution, Lupu and Pontusson (2011) argue that the relative proximity of the middle class to the poor and the affluent shapes their social affinities and their preference for redistribution. According to their analysis, this affects the partisan composition of governments, and ultimately social spending and redistribution. The authors test the predictions of their theory, using skew in the earnings distribution as their primary explanatory variable. The results lead them to conclude that there is robust empirical evidence in support of the structure of inequality model (*ibid*., p. 332). Widely acclaimed and frequently cited, their elegant and intuitively persuasive extension of social affinity theory to the economic domain had a significant impact on subsequent scholarship.

This paper has proposes a radically different interpretation of the causal links between earnings skew, redistribution and partisanship. It argues that governments can simultaneously respond to public pressure for greater equity by redistributing incomes through the tax and transfer system and through labor market regulation. Admittedly, this insight is not entirely new. As Rueda and Pontusson (2000) have argued, "[t]he effects of government partisanship will manifest themselves primarily in terms of redistribution via government taxation and spending, but government policies also affect the distribution of market incomes in general and of wages in particular" (*ibid.*, p. 362). The former is captured by the traditional measure for fiscal redistribution, while interventions into the labor market often disproportionately compress the lower half of the earnings distribution – in other words, they lead to skew. In particular, minimum wages can explain a substantial portion of the variation in earnings skew. This implies that earnings skew is an outcome of policy interventions and hence endogenous to the political economy of welfare states.

The findings reported in Lupu and Pontusson (2011) are perfectly compatible with both interpretations. However, the association between skew and redistribution is either causal in nature (social affinity hypothesis) or driven by a confounder in the form of government policy. Crucially, the causal mechanism described in the social affinity hypothesis operates irrespective of labor market institutions. This difference can be leveraged to overcome the identification problems that arise since the two explanations are, in part, observationally equivalent. If the structure of inequality model holds, the predictive power of skew should remain intact when minimum wages are added alongside unionization and vocational training as a further control variable. If the alternative explanation holds, controlling for minimum wages as a proximate cause of earnings skew should absorb much of the non-causal correlation that is due to confounding. The impact of this approach is striking: the relationship between skew and government partisanship collapses, and the link from skew to redistribution and social spending is similarly vulnerable.

All of this suggests that key results in Lupu and Pontusson (2011) are driven by endogeneity (the first central claim made in this paper).[33] In what might alternatively be interpreted as another challenge or an attempt to rescue the social affinity hypothesis in its economic variant, the present paper makes a second central claim: that earnings skew is neither a theoretically valid measure nor an empirical proxy for the structure of income inequality. Instead, the predictions of the structure of inequality model should be tested against data for skew in the income distribution (and not the earnings distribution). However, when such a test is performed on a substantially expanded data-set, no evidence for a positive association between income skew and redistribution emerges. In other words, it appears that, for all its beauty, the social affinity hypothesis does not offer a solution to Lindert's (2004) "Robin Hood paradox" – the third central claim of the present paper.

Two broader implications arise. First, regarding theory, it seems that advances in the sophistication of redistribution models have at times come at the expense of realistic micro-foundations. Under the structure of inequality logic, for instance, voters are not only expected to judge their own position in the income distribution accurately, but also that of others. Further, they have to assess relative income distances and make these the basis for their stance on redistribution.

[33] To be abundantly clear: Nothing in the present paper suggests any negligence in the original analysis. The disagreement concerns the question whether or not the findings in Lupu and Pontusson (2011) lend support to the causal mechanisms proposed by the social affinity hypothesis.

This is a demanding standard. As the OECD points out, "[m]ost of us have no idea – or the wrong idea – of how we compare with the rest of the population".[34] A series of recent survey experiments has demonstrated that people have indeed great difficulty to assess their own income position (Cruces *et al.*, 2013; Karadja *et al.*, 2014; Engelhardt and Wagener, 2016). Bublitz (2016) shows that these misperceptions differ substantially between the eight countries covered by her data. Strikingly, informing respondents about their true income position has no measurable impact on their support for redistribution (the case of Germany aside; *ibid*., p. 30f.). This implies that the two premises that underpin the model's macro-to-micro transition (skew is perceived correctly) and the action-formation mechanism (a person's relative position is decisive) look vulnerable.

Second, the endogeneity of earnings skew highlights that government policy has distributive outcomes that go beyond fiscal redistribution. Hicks and Swank (1984, p. 266) refer to these as "direct normative redistribution", or the "relatively direct (and intentional) impacts of regulatory policies in labor and other factor markets" (*ibid*.). In this framework, the minimum wage is a redistributive policy tool (Freeman, 1996). The present paper has only touched on these links, but it is arguably time for political science to fully reclaim the terrain – especially because the economic literature has conceded that the primary effects of labor market institutions are distributive (see Betcherman, 2012, p. 41). In fact, the partisan control of government or the strength of trade unions may alter the income distribution primarily via their effects on wage dispersion and factor shares. That this is a promising avenue for research is evident from detailed case studies on labor market reform (e.g. Hassel and Schiller, 2010) and the existing comparative work (e.g. Pontusson *et al*., 2002; Bradley *et al*., 2003; Rueda, 2008; Huber and Stephens, 2014).

[34] See "Compare your income" at http://www.oecd.org/statistics/compare-your-income.htm (accessed on 21 April 2017).

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Can the structure of inequality explain fiscal redistribution? Revisiting the social affinity hypothesis

Online Appendix

Table A1 Explaining skew in the earnings distribution with the Kaitz index and other labor market institutions (BE models)

	(A1)	(A2)	(A3)	(A4)
	Skew (earnings)		Skew (earnings)	
Kaitz index	0.780* (0.345)	0.758* (0.319)	0.941* (0.339)	0.981** (0.329)
No minimum wage (dummy)		0.262 (0.165)		0.340+ (0.177)
Unemployment rate	0.240 (0.797)	0.215 (0.714)	0.449 (0.710)	0.0923 (0.673)
Female labor force participation	-0.188 (0.503)	0.145 (0.319)	-0.0226 (0.500)	0.0459 (0.332)
Employment protection legislation	0.0895* (0.0373)	0.0780* (0.0333)	0.154*** (0.0369)	0.117** (0.0331)
Vocational training			-0.633* (0.236)	-0.409+ (0.208)
Trade union density			0.514 (0.327)	0.141 (0.150)
Constant	0.717+ (0.345)	0.541* (0.258)	0.422 (0.351)	0.502+ (0.257)
n =	329	496	271	427
Countries	24	33	22	30
R^2 (between)	0.430	0.389	0.662	0.532
Model	Between-effects (xtreg, be)			

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Refers to observations from 1985 onwards due to the limited availability of the OECD's EPL indicator. Models (A1) and (A3) only include observations where a statutory minimum wage was in place. In Models (A2) and (A4), observations where no general statutory minimum wage was in place enter with a Kaitz index of 0 and a dummy "No minimum wage" with the value of 1.

Source: See Online Appendix (Table A11).

Table A2 Replication of "Determinants of redistribution" with minimum wages and EPL as additional control variables (LDV models)

	(A5)	(A6)	(A7)	(A8)	(A9)	(A10)
	Redistribution		Redistribution		Redistribution	
Skew (earnings)	10.17*	12.99***	10.36*	14.31***	3.018	1.674
	(4.592)	(3.596)	(4.093)	(3.302)	(4.579)	(3.849)
D9/D1 ratio (earnings)	-0.0155	-0.162	0.904	0.988		
	(1.182)	(1.014)	(1.119)	(0.933)		
D9/D5 ratio (earnings)					4.297	9.465*
					(4.888)	(4.318)
Voter turnout	0.102**	0.0636*	0.104**	0.121***	0.121**	0.163***
	(0.0366)	(0.0297)	(0.0341)	(0.0275)	(0.0412)	(0.0327)
Proportionality	-0.0682	-2.376	0.0438	1.002	1.506	2.880*
	(2.173)	(1.633)	(1.863)	(1.586)	(2.060)	(1.452)
Vocational training	0.0199	0.0118	0.0763+	0.0737+	0.0572	0.0443
	(0.0393)	(0.0318)	(0.0452)	(0.0434)	(0.0456)	(0.0340)
Trade union density	9.013**	12.31***	15.08***	16.85***	12.66*	13.07***
	(3.337)	(2.277)	(4.180)	(3.516)	(5.059)	(3.815)
Unemployment rate	0.112	0.0512	0.00974	0.224	0.109	0.419*
	(0.181)	(0.148)	(0.190)	(0.158)	(0.244)	(0.175)
Female labour force participation	8.536	7.440+	8.134	8.545+	15.81*	27.55***
	(5.331)	(4.465)	(5.426)	(4.583)	(7.080)	(5.844)
Kaitz index			1.687	1.498	1.673	-2.228
			(4.984)	(4.114)	(4.619)	(3.773)
No minimum wage (dummy)			-2.143	-2.863	-1.785	-4.189*
			(2.649)	(2.247)	(2.477)	(1.832)
Employment Protection Legislation					0.728	2.537***
					(1.051)	(0.669)
Lagged dependent variable	0.492***	0.481***	0.439***	0.361***	0.453***	0.317**
	(0.0977)	(0.0776)	(0.113)	(0.0961)	(0.127)	(0.0971)
Constant	-14.73	-12.43	-18.42+	-24.73**	-24.25*	-43.61***
	(9.834)	(7.764)	(10.15)	(8.394)	(12.06)	(11.10)
n =	68	58	63	54	55	48
Countries	15	15	15	14	15	14
R^2	0.892	0.935	0.934	0.948	0.935	0.962
Model	Regression with lagged dependent variable, panel corrected standard errors and common AR1 process, corrected for heteroscedasticity only (xtpcse, pairwise cor(ar1) hetonly)					

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Corresponds to models (5) and (6) in Table 2 of Lupu and Pontusson (2011, p. 325). For all models, the order of observations within a panel serves as the time variable (e.g. order = 1 corresponds to 1971 for Canada, to 1981 for Australia and to 1992 for Belgium). Hence, the time variable cannot capture contemporaneous correlation of error terms and standard errors are only corrected for heteroscedasticity, explaining why they differ from those reported in the original publication. Models with even numbers drop outliers.

Source: Lupu and Pontusson (2011); OECD, ILO and ICTWSS for EPL, Kaitz index and minimum wage dummy (see Table A11).

Table A3 Replication of "Determinants of redistribution" with minimum wages and EPL as additional control variables (FE models)

	(A11)	(A12)	(A13)	(A14)	(A15)	(A16)
	Redistribution		Redistribution		Redistribution	
Skew (earnings)	24.47***	24.42***	17.28*	7.345	-2.304	-9.891
	(7.192)	(4.699)	(8.462)	(6.852)	(8.786)	(7.440)
D9/D1 ratio (earnings)	1.344	-1.537+	1.648	1.147		
	(1.502)	(0.914)	(1.639)	(1.179)		
D9/D5 ratio (earnings)					12.10	11.92*
					(7.826)	(4.684)
Kaitz index			-2.784	-1.181	-3.372	-6.890**
			(5.896)	(4.697)	(4.847)	(2.658)
No minimum wage (dummy)			0.736	0.923	0.540	-1.116
			(2.162)	(1.837)	(1.851)	(0.867)
Employment Protection Legislation					1.370	-0.888
					(5.079)	(2.191)
n =	77	67	70	63	58	52
Countries	15	15	15	15	15	15
R^2	0.887	0.968	0.895	0.953	0.932	0.970
Model	Regression with fixed effects, panel corrected standard errors and common AR1 process, corrected for heteroscedasticity only (xi: xtpcse i.country, pairwise cor(ar1) hetonly)					

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Corresponds to models (7) and (8) in Table 2 of Lupu and Pontusson (2011, p. 325). For all models, the order of observations within a panel serves as the time variable (e.g. order = 1 corresponds to 1971 for Canada, to 1981 for Australia and to 1992 for Belgium). Hence, the time variable cannot capture contemporaneous correlation of error terms and standard errors are only corrected for heteroscedasticity, explaining why they differ from those reported in the original publication. Fixed effects are generated through dummy variables (coefficients not reported) and hence enter the R^2. Constant not reported since its value is a function of which country dummy is omitted. Models with even numbers drop outliers.

Source: Lupu and Pontusson (2011); OECD, ILO and ICTWSS for EPL, Kaitz index and minimum wage dummy (see Table A11).

Table A4 Replication of "Determinants of social spending" with minimum wages and EPL as additional control variables (LDV models)

	(A17)	(A18)	(A19)	(A20)	(A21)	(A22)
	Non-elderly public social expenditure		Non-elderly public social expenditure		Non-elderly public social expenditure	
Skew (earnings)	1.742**	1.652***	0.945	0.708*	-2.198	-1.311
	(0.561)	(0.336)	(0.657)	(0.328)	(1.339)	(0.995)
D9/D1 ratio (earnings)	0.301*	0.146	0.538**	0.398***		
	(0.148)	(0.0907)	(0.168)	(0.0898)		
D9/D5 ratio (earnings)					2.925**	2.157***
					(1.026)	(0.605)
Voter turnout	0.00661	0.00386	0.00560	0.00297	0.00544	0.00415
	(0.00436)	(0.00315)	(0.00390)	(0.00245)	(0.00494)	(0.00364)
Proportionality	-0.884***	-0.663***	-0.928***	-0.679***	-0.859***	-0.620***
	(0.225)	(0.133)	(0.183)	(0.139)	(0.171)	(0.124)
Vocational training	0.0206**	0.00895*	0.0272***	0.0172***	0.0337**	0.0212**
	(0.00708)	(0.00392)	(0.00783)	(0.00367)	(0.0123)	(0.00753)
Trade union density	1.032***	0.931***	1.505***	1.424***	2.152***	1.771***
	(0.305)	(0.204)	(0.319)	(0.262)	(0.436)	(0.363)
Unemployment rate	-0.0682***	-0.0459**	-0.0679***	-0.0584***	-0.0713**	-0.0401**
	(0.0202)	(0.0148)	(0.0186)	(0.0118)	(0.0266)	(0.0144)
Female labour force	0.397	-0.129	0.826	0.249	0.881	0.783
participation	(0.861)	(0.640)	(1.084)	(0.657)	(1.916)	(1.333)
Population aged 65 years	-0.0919**	-0.0764***	-0.0918**	-0.0553*	-0.0893*	-0.0579*
and above	(0.0304)	(0.0189)	(0.0341)	(0.0228)	(0.0437)	(0.0290)
GDP growth	-0.183***	-0.197***	-0.198***	-0.197***	-0.202***	-0.219***
	(0.0197)	(0.0155)	(0.0191)	(0.0145)	(0.0217)	(0.0202)
Globalization	0.00956	0.0155***	0.00580	0.00713	-0.00764	0.000435
	(0.00788)	(0.00438)	(0.00804)	(0.00437)	(0.0112)	(0.00804)
Kaitz index			3.116***	2.682***	3.649***	3.096***
			(0.833)	(0.491)	(0.960)	(0.880)
No minimum wage			1.749***	1.376***	1.865***	1.453***
(dummy)			(0.375)	(0.229)	(0.405)	(0.374)
Employment Protection					0.162	0.106
Legislation					(0.152)	(0.0866)
Lagged dependent variable	0.903***	0.914***	0.871***	0.887***	0.863***	0.878***
	(0.0211)	(0.0136)	(0.0226)	(0.0110)	(0.0422)	(0.0223)
Constant	-0.611	-0.410	-1.764+	-1.172+	-1.714	-2.025+
	(0.982)	(0.739)	(1.014)	(0.624)	(2.187)	(1.212)
n =	311	277	285	256	208	183
Countries	18	18	18	17	18	18
R^2	0.991	0.997	0.992	0.997	0.991	0.996
Model	Regression with lagged dependent variable, panel corrected standard errors and panel-specific AR1 process, (xtpcse, pairwise cor(psar1))					

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Corresponds to models (13) and (14) in Table 3 of Lupu and Pontusson (2011, p. 327). Models with even numbers drop outliers.

Source: Lupu and Pontusson (2011); OECD, ILO and ICTWSS for EPL, Kaitz index and minimum wage dummy (see Table A11).

Table A5 Replication of "Determinants of social spending" with minimum wages and EPL as additional control variables (FE models)

	(A23)	(A24)	(A25)	(A26)	(A27)	(A28)
	Non-elderly public social expenditure		Non-elderly public social expenditure		Non-elderly public social expenditure	
Skew (earnings)	9.668**	9.103***	7.285*	6.002*	1.818	-0.524
	(3.285)	(2.501)	(3.051)	(2.430)	(3.317)	(2.098)
D9/D1 ratio (earnings)	0.938+	1.053*	1.013+	1.380**		
	(0.531)	(0.505)	(0.540)	(0.450)		
D9/D5 ratio (earnings)					-1.254	-1.927
					(2.429)	(1.188)
GDP growth	-0.113***	-0.107***	-0.131***	-0.136***	-0.122*	-0.0691*
	(0.0297)	(0.0223)	(0.0323)	(0.0223)	(0.0515)	(0.0280)
Kaitz index			-0.639	-5.290+	4.024	7.206**
			(3.782)	(3.161)	(3.400)	(2.522)
No minimum wage (dummy)			-1.596	-3.780*	0.991	2.188+
			(1.865)	(1.495)	(1.548)	(1.139)
Employment Protection Legislation					6.117*	6.334***
					(2.652)	(1.061)
Constant	-1.814	-0.946	2.413	5.619	2.776	4.328
	(4.411)	(3.559)	(5.388)	(4.661)	(4.256)	(3.486)
n =	320	284	292	259	209	181
Countries	18	18	18	18	18	18
R^2	0.961	0.981	0.961	0.986	0.980	0.993
Model	Regression with fixed effects, panel corrected standard errors and panel-specific AR1 process, (xi: xtpcse i.country, pairwise cor(psar1))					

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Corresponds to models (15) and (16) in Table 3 of Lupu and Pontusson (2011, p. 327). Fixed effects are generated through dummy variables (coefficients not reported) and hence enter the R^2. Constant not reported since its value is a function of which of the country dummy is omitted. Models with even number drop outliers.

Source: Lupu and Pontusson (2011); OECD, ILO and ICTWSS for EPL, Kaitz index and minimum wage dummy (see Table A11).

Table A6 Explaining redistribution with skew in the income distribution (observations prior to 2008 only)

	(A25)	(A26)	(A27)	(A28)	(A29)	(A30)
	Redistribution		Redistribution		Redistribution	
Skew (incomes)	-0.0477 (0.147)	-0.0951 (0.129)	0.0360 (0.244)	0.0897 (0.241)	-1.006 (0.613)	-1.085* (0.499)
P75/P25 ratio (incomes)	-0.00496 (0.0293)	-0.000260 (0.0235)	-0.0173 (0.0382)	0.0173 (0.0475)	-0.113 (0.0979)	-0.119 (0.0755)
Voter turnout	0.149*** (0.0423)		0.168 (0.188)		0.154 (0.133)	
Proportionality	0.0790** (0.0266)		-0.0523 (0.0474)		0.172 (0.114)	
Vocational training	-0.0353 (0.0379)		0.0310 (0.112)		0.0380 (0.152)	
Trade union density	0.0855* (0.0395)		-0.189 (0.308)		0.0778 (0.128)	
Unemployment rate	0.282 (0.261)		0.660* (0.308)	0.382 (0.348)	0.169 (0.525)	1.213 (0.704)
Female labor force participation	0.0684 (0.0533)		-0.275 (0.356)	-0.171 (0.234)	-0.338 (0.258)	0.101 (0.238)
Lagged dependent variable	0.461*** (0.111)					
Constant	-0.0566 (0.226)	0.348* (0.171)	0.275 (0.374)	0.0615 (0.316)	1.353 (0.848)	1.377+ (0.705)
n =	70	93	85	116	85	116
Countries	16	18	20	23	20	23
R^2	0.857	0.869	0.620	0.649	0.693	0.454
Model	PCSE with AR(1) and LDV or FE (xtpcse [l1.red | i.country], pairwise cor(ar1))		2-way fixed-effects with cluster robust standard errors (xtreg i.year, fe vce(r))		Between-effects (xtreg, be)	

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Models A25 and A26 correspond to the econometric specification in Lupu and Pontusson (2011, p. 325, Models 5 and 7). Coefficients on the country dummies in Model A26 and on the year dummies in Model A27 and A28 are not reported.

Source: Luxembourg Income Study, OECD and ILO, Comparative Political Data Set, UNESCO, ICTWSS (see Online Appendix Table A11).

Table A7 Explaining redistribution with skew in the income distribution (disposable incomes)

	(A31)	(A32)	(A33)	(A34)	(A35)	(A36)
	Redistribution		Redistribution		Redistribution	
Skew (disposable incomes)	-0.101 (0.127)	-0.0570 (0.166)	-0.155 (0.208)	-0.143 (0.176)	0.601 (1.137)	-1.688+ (0.885)
P75/P25 ratio (disposable incomes)	0.0146 (0.0237)	-0.0525 (0.0320)	-0.252** (0.0726)	-0.221*** (0.0558)	-0.0412 (0.140)	-0.302** (0.0901)
Voter turnout	0.0696+ (0.0362)		0.0622 (0.0641)		0.115 (0.136)	
Proportionality	0.0386+ (0.0220)		-0.0105 (0.0473)		0.113 (0.105)	
Vocational training	-0.0182 (0.0217)		0.0250 (0.0874)		-0.0253 (0.179)	
Trade union density	0.0661* (0.0306)		-0.138 (0.206)		0.209 (0.126)	
Unemployment rate	-0.0713 (0.118)		0.728*** (0.166)	0.647*** (0.123)	0.537 (0.453)	0.539 (0.502)
Female labor force participation	0.00154 (0.0580)		-0.100 (0.277)	-0.0190 (0.203)	-0.415 (0.277)	0.105 (0.218)
Lagged dependent variable	0.695*** (0.122)					
Constant	0.0600 (0.168)	0.418+ (0.215)	0.822+ (0.422)	0.673* (0.270)	-0.260 (1.227)	2.353* (1.010)
n =	125	165	141	192	141	192
Countries	23	26	24	27	24	27
R^2	0.794	0.848	0.603	0.636	0.584	0.396
Model	PCSE with AR(1) and LDV or FE (xtpcse [l1.red | i.country], pairwise cor(ar1))		2-way fixed-effects with cluster robust standard errors (xtreg i.year, fe vce(r))		Between-effects (xtreg, be)	

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Models A31 and A32 correspond to the econometric specification in Lupu and Pontusson (2011, p. 325, Models 5 and 7).Coefficients on the country dummies in Model A32 and on the year dummies in Model A33 and A34 are not reported.

Source: Luxembourg Income Study, OECD and ILO, Comparative Political Data Set, UNESCO, ICTWSS (see Online Appendix Table A11).

Table A8 Explaining social spending with skew in the distribution of market incomes

	(A37) Non-elderly public social expenditure	(A38)	(A39) Non-elderly public social expenditure	(A40)	(A41) Non-elderly public social expenditure	(A42)
Skew (incomes)	5.816 (4.607)	5.621 (3.569)	1.798 (6.096)	5.990 (5.477)	-1.275 (17.18)	-29.81 (17.24)
P75/P25 ratio (incomes)	0.673 (0.662)	1.309 (0.830)	0.126 (0.555)	0.573 (0.495)	0.290 (2.290)	-3.007 (2.147)
Voter turnout	2.003 (1.766)		-7.707 (9.214)		8.037+ (4.132)	
Proportionality	-0.0805 (0.545)		-0.670 (1.272)		2.848 (3.327)	
Vocational training	-0.231 (1.110)		2.876 (3.032)		1.445 (4.909)	
Trade union density	0.721 (0.886)		-22.79* (9.646)		5.985 (3.434)	
Unemployment rate	9.491 (8.598)		43.50* (17.21)	37.37+ (19.35)	15.40 (12.47)	15.71 (17.49)
Female labor force participation	2.210 (2.110)		-8.228 (10.64)	-3.186 (8.694)	-1.849 (8.348)	6.885 (9.043)
Lagged dependent variable	0.840*** (0.0817)					
Constant	-8.351 (6.683)	2.792 (5.202)	26.09 (17.99)	2.649 (9.332)	3.024 (22.90)	41.34+ (23.39)
n =	117	140	129	156	129	156
Countries	21	23	22	24	29	24
R^2	0.750	0.855	0.631	0.609	0.714	0.237
Model	PCSE with AR(1) and LDV or FE (xtpcse [l1.red | i.country], pairwise cor(ar1))		2-way fixed-effects with cluster robust standard errors (xtreg i.year, fe vce(r))		Between-effects (xtreg, be)	

Standard errors in parentheses; + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Note: Models A37 and A38 correspond to the econometric specification in Lupu and Pontusson (2011, p. 325, Models 5 and 7). Coefficients on the country dummies in Model A38 and on the year dummies in Model A39 and A40 are not reported.

Source: Luxembourg Income Study, OECD and ILO, Comparative Political Data Set, UNESCO, ICTWSS (see Online Appendix Table A11).

Table A9 Descriptive statistics for the updated minimum wage and earnings skew data (OECD)

	Observations			Skew (earnings)			Kaitz index (minimum wage / median wage)		
Country	Number	First	Last	Mean	Min.	Max.	Mean	Min.	Max.
Australia	42	1975	2016	1.085	0.985	1.226	0.589	0.522	0.655
Austria	12	2004	2015	1.123	1.103	1.133			
Belgium	41	1975	2015	1.248	1.209	1.298	0.536	0.492	0.572
Canada	51	1965	2015	0.942	0.882	1.007	0.435	0.379	0.518
Czech Republic	26	1991	2016	0.973	0.948	0.992	0.362	0.224	0.523
Denmark	14	2002	2015	1.199	1.173	1.216			
Estonia	17	1999	2015	1.038	1.024	1.048	0.382	0.336	0.418
Finland	33	1977	2015	1.183	1.070	1.234			
France	56	1960	2015	1.317	1.280	1.335	0.576	0.424	0.670
Germany	24	1992	2015	1.001	0.888	1.087	0.478	0.478	0.478
Greece	54	1962	2015	1.175	1.103	1.294	0.599	0.439	0.851
Hungary	29	1986	2015	1.230	1.034	1.474	0.445	0.359	0.570
Iceland	11	2004	2015	1.020	0.943	1.076			
Ireland	18	1994	2015	1.132	0.962	1.670	0.497	0.428	0.675
Israel	15	2001	2015	1.398	1.325	1.463	0.563	0.545	0.581
Italy	15	1986	2014	1.025	0.918	1.095			
Japan	41	1975	2015	1.102	0.994	1.167	0.322	0.276	0.398
Korea	33	1984	2016	1.120	0.983	1.245	0.360	0.271	0.484
Latvia	19	1997	2015	1.119	1.002	1.204	0.421	0.334	0.518
Lithuania	19	1997	2015	1.108	1.071	1.181	0.480	0.410	0.558
Luxembourg	56	1960	2015	1.260	1.197	1.361	0.497	0.397	0.563
Netherlands	52	1964	2015	1.103	1.080	1.145	0.575	0.459	0.702
New Zealand	56	1960	2015	1.095	0.963	1.203	0.564	0.333	0.784
Norway	19	1997	2015	0.971	0.902	1.021			
Poland	36	1980	2015	1.017	0.947	1.097	0.423	0.097	0.513
Portugal	41	1975	2015	1.693	1.585	1.847	0.508	0.446	0.651
Romania	56	1960	2015				0.421	0.174	0.569
Slovak Republic	26	1991	2016	1.119	1.089	1.146	0.437	0.325	0.588
Slovenia	12	2002	2015	1.203	1.159	1.238	0.565	0.507	0.638
Spain	52	1964	2015	1.244	1.203	1.288	0.455	0.346	0.682
Sweden	36	1975	2013	1.177	1.127	1.231			
Switzerland	10	1996	2014	1.158	1.074	1.221			
Taiwan	8	2009	2016				0.580	0.546	0.598
United Kingdom	47	1970	2016	1.012	0.843	1.126	0.450	0.402	0.487
United States	57	1960	2016	1.062	0.980	1.168	0.409	0.307	0.551
All countries	1134	1960	2016	1.113	0.843	1.847	0.482	0.097	0.851

Note: Observations refer to observations where either skew or minimum wage data were available, or both. Descriptive statistics for the minimum wage refer to the raw data. For the multivariate analysis, a new variable was generated that treats a Kaitz index of zero as a valid value where no comprehensive minimum wage was in place (see Online Appendix Table A11).

Source: Own tabulation, based on OECD (supplemented by ILO and national sources for Taiwan).

Table A10 Descriptive statistics for the updated redistribution and income skew data (LIS database)

Country	Observations			Redistribution			Skew (incomes)		
	Number	First	Last	Mean	Min.	Max.	Mean	Min.	Max.
Australia	8	1981	2010	0.252	0.227	0.279	0.870	0.809	0.951
Austria	4	2004	2013	0.294	0.272	0.314	0.908	0.873	0.937
Belgium	2	1992	1997	0.382	0.369	0.395	0.884	0.869	0.900
Canada	13	1971	2013	0.224	0.166	0.271	0.870	0.807	0.936
Czech Republic	7	1992	2013	0.276	0.241	0.321	0.933	0.893	0.958
Denmark	8	1987	2013	0.370	0.292	0.433	0.841	0.788	0.868
Estonia	4	2004	2013	0.172	0.160	0.189	0.905	0.888	0.926
Finland	8	1987	2013	0.365	0.321	0.460	0.888	0.862	0.914
Germany	27	1973	2015	0.259	0.092	0.309	0.934	0.826	1.019
Greece	3	2007	2013	0.192	0.169	0.211	0.910	0.865	0.941
Iceland	3	2004	2010	0.225	0.159	0.302	0.934	0.904	0.952
Ireland	4	1987	2010	0.356	0.301	0.441	0.682	0.548	0.739
Israel	11	1979	2016	0.243	0.180	0.310	0.873	0.818	0.947
Japan	1	2008	2008	0.089	0.089	0.089	0.955	0.955	0.955
Korea	4	2006	2012	0.052	0.048	0.060	0.984	0.973	0.996
Lithuania	2	2010	2013	0.189	0.161	0.217	0.903	0.896	0.909
Luxembourg	4	2004	2013	0.268	0.244	0.285	0.922	0.879	0.971
Netherlands	9	1983	2013	0.312	0.247	0.421	0.958	0.871	1.035
Norway	9	1979	2013	0.307	0.210	0.361	0.918	0.872	0.978
Romania	2	1995	1997	0.172	0.170	0.173	0.918	0.917	0.919
Slovak Republic	5	1992	2013	0.260	0.186	0.413	0.908	0.872	0.931
Spain	3	2007	2013	0.190	0.158	0.224	0.856	0.813	0.911
Sweden	8	1967	2005	0.381	0.278	0.479	0.895	0.818	0.971
Switzerland	8	1982	2013	0.106	0.060	0.152	0.997	0.974	1.027
Taiwan	11	1981	2016	0.027	0.002	0.072	1.022	1.001	1.034
United Kingdom	12	1969	2013	0.238	0.169	0.281	0.801	0.565	1.021
United States	12	1974	2016	0.183	0.163	0.212	0.861	0.824	0.915
All countries	192	1967	2016	0.242	0.002	0.479	0.902	0.548	1.035

Note: Redistribution refers to relative redistribution or the change in the Gini coefficient as one move from market incomes to disposable incomes, expressed relative to the Gini coefficient for market incomes. Only households with a household head aged 25 to 59 years. Standard LIS routines with respect to equivalence scale, top- and bottom-coding; observations with zero disposable income and missing income components dropped. Market incomes are equivalent to factor incomes plus, where available, private transfers received. Skew (incomes) refers to the distribution in market incomes, measured as (P75/P50)/(P50/P25).

Source: Own tabulation, based on Luxembourg Income Study (LIS) Database (multiple countries, 2018 Summer data Release).

Table A11 Variable definitions and sources for the updated data-set

Variable	Definition	Source
D9/D5 and D5/D1 ratio (earnings)	Decile ratios for gross earnings of full-time employees. D9, D5 and D1 refer to the upper limits of the respective decile.	OECD
Skew (earnings)	Skew of the distribution in gross earnings of full-time employees, measured as (D9/D5)/(D5/D1).	OECD
Kaitz index	Ratio of the minimum wages over median earnings, expressed as a fraction. Observations where no statutory minimum wage is in force are recoded from missing to zero (see below).	OECD, supplemented by ILO and national sources for Taiwan.
No minimum wage (dummy)	Dummy that takes the value of 1 for observations where no statutory minimum wage is in place. Corresponds to code 0 in the variable "National Minimum Wage" of the ICTWSS database. (For the Republic of Korea, observations for 1987 and 1988 were re-coded to reflect the minimum wage in manufacturing.)	Visser, Jelle. ICTWSS database, Version 5.1 (September 2016), supplemented by ILO (INWORK legal database).
Employment protection legislation	OECD indicator for the strictness of regulation on dismissals and the use of temporary contracts (Version 1). Extrapolated for up to two years, based on the last available observation.	OECD
Unemployment rate	Unemployment rate, as a fraction of the total labor force aged 15 to 64 years (15 years and above for ILO estimates).	OECD, supplemented by ILO.
Female labor force participation	Civilian labor force participation for females aged 15 to 64 years, as a fraction of the corresponding population. Refers to females aged 15 years and above for Taiwan.	OECD (accessed 19 June 2018), supplemented by ILO and national data for Taiwan.
Redistribution	Relative redistribution, or the change in the Gini coefficient as we move from market incomes to disposable incomes, expressed relative to the Gini coefficient for market incomes. Only households with a household head aged 25 to 59 years. Standard LIS routines with respect to equivalence scale, top- and bottom-coding were applied. Market incomes are equivalent to factor incomes plus, where available, private transfers received.	Luxembourg Income Study (LIS) Database (multiple countries, 2018 Summer Data Release).
P75/P25 ratio (incomes)	Ratio of the 75th over the 25th income percentile, as ranked by market income (for details see under redistribution).	Luxembourg Income Study (LIS) Database (multiple countries, 2018 Summer Data Release).
Skew (incomes)	Skew of the distribution in market incomes, measured as (P75/P50)/(P50/P25) (for details see under redistribution).	Luxembourg Income Study (LIS) Database (multiple countries, 2018 Summer Data Release).
Voter turnout	Voter turnout in the most recent national election, as a fraction of eligible voters.	Armingeon, Klaus et al. 2017. Comparative Political Data Set 1960-2015. Berne: University of Berne. Supplemented by International IDEA (Voter Turnout Database).
Proportionality	Gallagher's measure for disproportionality of parliamentary representation inverted and standardized to range from 0 to 1. Higher values stand for greater proportionality.	Armingeon, Klaus et al. 2017. Comparative Political Data Set 1960-2015. Berne: University of Berne.
Vocational training	Percentage of students in secondary education enrolled in vocational programs, both sexes (Indicator: GTVP_2T3_V). Recoded as 0 if "Magnitude nil or negligible".	UNESCO Institute of Statistics (February 2018 release).
Trade union density	Trade union density rate, calculated as the number wage and salary earners that are trade union members, divided by the total number of wage and salary earners.	OECD, supplemented by J. Visser, ICTWSS database, Version 5.1 (September 2016).
Non-elderly public social expenditure	Non-elderly public social expenditure as a share of GDP, or total public social expenditure minus expenditure on the branches 'old age' and 'survivors'.	OECD Social Expenditure Database.

Variable	Definition	Source
Globalization	2018 KOF Index of Globalization; composite index of globalization that aims to capture economic globalization, political globalization, and social globalization. Standardized to range from 0 to 1 and extrapolated to 2014.	Dreher, Axel (2006): Does Globalization Affect Growth? Evidence from a new Index of Globalization, *Applied Economics* 38 (10): 1091-1110. (Version as of 5 April 2018).
GDP growth	Real GDP growth (change on prior year), expressed as a fraction (Indicator: NY.GDP.MKTP.KD.ZG).	World Bank, World Development Indicators.
Population aged 65 years and above	Share of population aged 65 years and above (annual indicator, medium variant projections from 2016 onwards).	UN Population Division, World Population Prospects 2017.