The Case for Welfare State Universalism,
or the Lasting Relevance of the Paradox of Redistribution

Olivier Jacques
olivier.jacques@mail.mcgill.ca

Department of political science, McGill University

Alain Noël
alain.noel@umontreal.ca

Département de science politique, Université de Montréal

Montréal, November 2016

Forthcoming, *Journal of European Social Policy*

Abstract

In 1998, Walter Korpi and Joakim Palme proposed a political and institutional explanation to account for the greater redistributive success of welfare states that relied more on universal than on targeted programs. Effective redistribution, they argued, resulted less from a Robin Hood logic — taking from the rich to give to the poor — than from a broad and egalitarian provision of services and transfers. Hence, the paradox: a country obtained more redistribution when it took from all to give to all than when it sought to soak the rich to help the poor. Recent studies, however, failed to confirm the existence of this paradox. This article suggests that the original argument was theoretically sound but inadequately operationalized. Korpi and Palme measured universalism indirectly, not by the design or character of social programs, but rather by their outcomes, namely by their income effects. These outcomes, however, are influenced by exogenous factors. We use two new OECD indicators to capture universalism directly, through the institutional design of social programs: 1) the percentage of social benefits that are means or income tested; and 2) the proportion of private spending in total social expenditures. These two indicators are combined into a universalism index and tested with a time-series cross-sectional design for 20 OECD countries between 2000 and 2011. This approach, we argue, better captures institutional design, in a way that is consistent with Korpi and Palme's original argument, and it suggests that there is still a paradox of redistribution in the twenty-first century welfare state.

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In 1998, Walter Korpi and Joakim Palme proposed a political and institutional explanation to account for the greater redistributive success of welfare states that relied more on universal than on targeted programs (Korpi and Palme, 1998). Effective redistribution, they argued, resulted less from a Robin Hood logic — taking from the rich to give to the poor — than from a broad and egalitarian provision of services and transfers. Citizens were more willing to sustain redistributive policies with their taxes when they also saw themselves or their relatives as present or future beneficiaries. Hence, the paradox: a country obtained more redistribution when it took from all to give to all than when it sought to soak the rich to help the poor.

Two recent developments illustrate how the paradox of redistribution may work, one in Canada, the other in Denmark. In 1997, the Quebec government created a universal childcare system: every family, regardless of its income or employment situation, obtained access to regulated daycare places at a uniform, modest daily fee (five dollars initially). Enrolments exploded and the new service rapidly garnered broad popular support, even though places were missing and the cost of the program grew significantly. By all accounts, the new childcare system facilitated the labour market integration of low-income young women and a reduction in child poverty, but it also became enshrined as a middle-class entitlement that no government could withdraw with impunity (Noël, 2013). A program helpful for the poor became entrenched because it also constituted a much-appreciated benefit for the middle class.

In contrast, the labour market and social assistance reforms undertaken by Denmark's liberal-conservative coalition between 2001 and 2011 reinforced the cleavage separating the middle class and the poor with, for instance, lower social assistance benefits for immigrants from

outside the European Union (adopted in 2002) and more demanding work and activation requirements for all social assistance beneficiaries (in 2005; Thelen, 2014: 150-51). Gradually, the pension regime also changed, to become less universal and more private and means-tested (Obinger et al,, 2010: 120). Overall, the Danish welfare state remained generous, with a strong, persistent universalist orientation, but it became less redistributive. Poverty and inequality increased, and tensions became more apparent around questions of distribution and redistribution, especially regarding the status of immigrants (Andersen, 2011: 54-55; Obinger et al., 2010: 123). Kathleen Thelen evokes "a different kind of universalism" to characterize Denmark's new flexicurity model, which coexists with "a particularly virulent strain of anti-immigrant sentiment" (2014: 198-99).

Korpi and Palme's argument and empirical findings consolidated a view that was already conventional wisdom in political debates, at least on the left, and it anchored the case for universalism for more than ten years. Lately, however, a number of convergent studies have cast doubts on this demonstration, and questioned the very existence of a paradox (Kenworthy, 2011; Marx, Salanauskaite and Verbist, 2013; Brady and Bostic, 2015). These studies seemed convincing because they replicated faithfully Korpi and Palme's research design, with the same or with expanded and updated data, but failed to confirm the expected relationships. Either the original study was illusory or the world had changed. In either case, the argument in favor of universalism appeared shaken.

There is no doubt that the configuration of social programs has evolved since the late 1990s, with the rise of hybrid patterns of targeted universalism that make the distinction between targeting and universalism less clear-cut. Targeting benefits within a universal framework may well be an effective redistributive tool (Kenworthy 2011; Van Lancker and Van Mechelen 2014).

We argue, however, that it remains true that the more universal a welfare state is, the higher the social budget and the redistributive effort tend to be (see figures 4 and 5 below). Indeed, the ranking of countries in terms of redistribution has not changed all that much over the years. Sweden and Denmark are still more egalitarian than, say, the United Kingdom and the United States. The overall configuration of their social programs must have something to do with this outcome.

In this article, we revisit the question from a somewhat different perspective. We suggest that Korpi and Palme's argument was theoretically sound but inadequately operationalized. Later replications used the same or similar empirical procedures, and reproduced the initial problem. Indeed, Korpi and Palme measured universalism indirectly, not by the institutional design of social programs, but rather by their outcomes, namely by their income effects on different groups of the population (Marx, Salanauskaite and Verbist, 2013; Van Lancker and Van Mechelen, 2015). The outcomes of social programs, however, are influenced by exogenous factors. A universal program, for instance, can appear targeted to lower (or higher) income groups depending on the characteristics of the beneficiary population. When more children belong to lower quintiles families, for instance, a universal child benefit may appear as a measure targeted at the poor.

This article proposes to revisit the paradox of redistribution with different indicators, meant to capture universalism not through its effects, but rather through the institutional design of social programs. The idea is to probe directly the institutional characteristics of a country's welfare arrangements. Focusing on institutions rather than on outcomes captures more closely whether or not social policies have a universalist or targeted orientation, and leaves out many exogenous factors influencing post-transfers distribution. To do so, we resort to two OECD

indicators, which make it possible to conduct cross-sectional time-series analyses on the main welfare state cases: 1) a new measure on the percentage of social benefits that are means or income tested and 2) a relatively new measure of the proportion of private spending in total social expenditures, to assess the presence of universalism and targeting not only in transfers, but also in social services. These indicators, we argue, bring us closer to institutional design, in a way that is consistent with Korpi and Palme's original argument, and they help us see that, indeed, there is still a paradox of redistribution in the twenty-first century welfare state.

The first section revisits the paradox of redistribution and its critics, to summarize the literature on the question. We then present our proposal to reframe the question by focusing on institutional design along with our models, indicators, and data sources. The third section covers the main results, for bivariate and multivariate models, and the conclusion discusses some of the theoretical and empirical implications of the proposed turn in favor of institutional design.

The paradox and its critics

Korpi and Palme's article on the paradox of redistribution remains one of the most widely cited articles in comparative welfare state research. They present and support a politically important and rather counterintuitive argument: the more social benefits are targeted to the poor, the less poverty and inequality reduction a welfare state achieves. Anchored in power resources and institutional theory, the argument identifies three clusters of countries in 1985: the basic security, the encompassing, and the corporatist model, more or less akin to Gøsta Esping-Andersen's three worlds of welfare capitalism (1990). In the basic security model, the limited benefits offered to the middle class encourage it to turn to private insurance for social protection, and make it less likely to support a large redistributive budget. By contrast, in the encompassing

model, generous universal benefits and public insurance programs that offer good replacement rates for the middle class crowd out private insurance and ensure solid support for redistribution. The corporatist model stands somewhere in between, offering good protection to the middle class but leaving some outsiders behind. The key institutional and political contrast divides the basic security and encompassing models, the former being inimical to redistribution, and the latter favorable.

The key mechanism, according to Korpi and Palme, is the difficulty welfare states targeted to the poor have in sustaining middle-class support. Targeting creates a zero-sum conflict between low-income households receiving benefits and middle-income citizens financing them. This division discourages broad coalitions in support of the welfare state, and increases the salience of conflict lines over redistribution (Korpi and Palme, 1998). Indeed, Christian Albrekt Larsen finds that targeting fosters public discussions on the worthiness, neediness and deservingness of beneficiaries, which tend to lower public preferences for redistribution (2008). Even among beneficiaries, means testing creates a negative experience, which also erodes support for public programs (Kumlin, 2004). Thus, low-income targeting reduces the size of the redistributive budget, whereas universalism shores up middle-class support and sustains a stronger redistributive effort. There would thus be a trade-off between low income targeting and the size of the redistributive budget and, consequently, a trade-off as well between targeting and redistribution.

Korpi and Palme's data, however, are from the 1980s and their demonstration is based on relatively weak correlations and on scatterplots for eleven countries. Welfare states have changed significantly since the 1980s, if only because time has passed. Neo-liberal retrenchment and reforms inspired by the social investment perspective must also have left an imprint (Hemerijck,

2013). Data and methods have progressed as well, making it possible to test the argument more thoroughly, on a larger number of years and cases (Brady and Bostic, 2015: 269).

Three recent reassessments of the paradox of redistribution find that although there was a clear correlation between universalism and redistribution in the 1980s and early 1990s, this relationship fades out and disappears in recent years. In the 2000s, the size of the redistributive budget is actually correlated with low-income targeting, which goes against Korpi and Palme's core argument (Kenworthy, 2011; Marx, Salanauskaite and Verbist, 2013; Brady and Bostic, 2015). In a more circumscribed study of child benefits, Wim Van Lancker and Natascha Van Mechelen also find that targeting to low-income households may be associated with higher benefits and lower child poverty (2015). These new findings seem robust, whether the sample includes only the eleven cases studied by Korpi and Palme or a broader sample of OECD nations (Marx, Salanauskaite and Verbist, 2013; Brady and Bostic, 2015).

In a recent *American Sociological Review* article, David Brady and Amie Bostic propose one of most thorough analyses of the paradox of redistribution, which they cast as a multifaceted and complex phenomenon (2015). They innovate, in particular, by including public opinion data in their model. In line with Korpi and Palme's argument, they find that income targeting reduces public support for redistribution, but universalism, which they measure differently, does not seem to boost support significantly. From this perspective, the general power resources argument in favour of universalism does not hold very well.

Brady and Bostic, however, measure public support for redistribution through one question only, which asks respondents whether it should be "the government's responsibility to reduce income differences between rich and poor" (2015: 279). Jason Jordan's (2013) more sophisticated assessment of public support for specific programs in 17 OECD countries finds

indeed that, as Korpi and Palme predicted, citizens express stronger support for universal programs than for targeted measures. In fact, more specific studies on universalism and targeting often seem consistent with the paradox of redistribution argument. David Brady and Rebekah Burroway (2012), for instance, documents the advantages of universalism for anti-poverty policies for single mothers in 18 OECD countries. Mathieu Lefèbvre (2007) identifies a paradox of redistribution in pension systems, with systems offering a higher replacement rate to low-income households ending up less generous for the poor than contributory systems.

Korpi and Palme's argument, however, concerned the welfare state as a whole, the effect of an overall institutional arrangement on the politics of redistribution. Some studies still find a negative relationship between targeting, public support for redistribution, and redistributive effort (Whiteford, 2008; McKnight, 2015; Beramendi and Rehm, 2016), but the challenge posed by the contrary results of Lane Kenworthy (2011), Ive Marx, Lina Salanauskaite and Gerlinde Verbist. (2013), and Brady and Bostic (2015) appears important. Perhaps, as Marx et al. suggests, the world has changed, with the emergence of new targeted programs, like negative income taxes for the working poor, which are not stigmatizing, include large segments of the population, and are designed to encourage work and undo poverty traps. Or maybe the paradox was always more illusory than real?

Before bidding farewell to the paradox of redistribution, we should assess it carefully, with data that capture the core theoretical argument, about the class politics of redistribution in different types of welfare states. In the following section, we argue this has not been done so far, to a large extent because critics based their empirical tests on outcomes rather than on institutional design, just like Korpi and Palme did when they operationalized their theory.

Back to Institutional Design: Theory, Operationalization and Data, and Methods

   Theory

   Korpi and Palme's insistence on the institutional characteristics of welfare states left no doubt on the importance they gave to institutional design. Their ideal-typical model of social insurance institutions were precisely defined by eligibility rules, benefit-level principles, and forms for governing, all features that expressed more or less universal or more or less targeted programs. Policy design was intimately connected to the coalitions that facilitated or impeded welfare state development.

   In the first part of their empirical analysis, Korpi and Palme compared redistributive budget and inequality among different types of welfare states, and found indeed that encompassing welfare states redistributed more than corporatist welfare states, which did more than basic security models. To ascertain this finding with correlations, however, they developed an "index of targeting of transfer income," which, more or less like a Gini or Kakwani index, measured the concentration of transfers on low or high-income households (1998: 684). As they did, they moved from institutions to outcomes, and brought in a number of exogenous effects unrelated to the institutional design of the welfare state. All other things being equal, for instance, a more unequal labor market, or a higher proportion of single-parent families, could generate a distribution of transfers more concentrated on low-income households. Transfers do reflect institutional design, but they are also shaped by a number of other factors.

   When they questioned the existence of a paradox of redistribution, Kenworthy (2011), Marx et al. (2013), and Brady and Bostic (2015) replicated Korpi and Palme's analysis with recent data, covering more countries, and with more sophisticated methods, but they did not question the original operationalization and conflated institutional design with targeting

outcomes. Kenworthy (2011) basically reproduced the same research design for recent years. Marx, Salanauskaite and Verbist acknowledged the possible distance between institutional design and outcomes, but nevertheless resorted to the same type of concentration index (2013: 10). Their concentration coefficient, it should be noted, yielded counter-intuitive scores for universalism, with the United States being more "universal" than Denmark, Norway and Sweden (Marx et al. 2013: 47-48). Kenworthy's estimates also present the United States as more universal than Sweden in 2000 and 2005 (2011: 55). Brady and Bostic used two concentration coefficients, one to measure low-income targeting, and the other to capture universality, understood as homogeneity in benefits, coverage, and eligibility.

Recognizing this discrepancy between the institutional design postulated in the theory and the outcomes considered in the operationalization, Van Lancker and Van Mechelen construct a targeting indicator for child benefits based on statutory information on benefits for various households (2015: 63). Theoretically satisfying, this indicator is specific to child benefits, and does not allow a test on the whole welfare state arrangement, on par with Korpi and Palme and their critics. For this, we need to turn to different indicators, which can serve as proxies for a universalist institutional design.

Operationalization and Data

The distinction between universality and targeting, suggest Wim van Oorschot and Femke Roosma, is never clear-cut, it "is a matter of degree, not of essence" (2015: 8). An income supplement provided to all single mothers, for instance, may well be universal by design but targeted in its effects, if single mothers have lower incomes. Likewise, free university tuition for all may have targeted consequences, in this case in favor of the rich, if those going to university

come primarily from well-to-do families (Bergh, 2004; Van Lancker and Van Mechelen, 2015; Van Oorschot and Roosma, 2015). From a political standpoint, however, it is less the effects on incomes that matter than the fact that programs treat all citizens alike. When middle class voters see the welfare state as a source of protection and benefits, they are more likely to support redistribution. This is the core insight of the power resources approach.

In this perspective, a welfare state can be seen as universal when transfers are not means or income tested, and when citizens do not have to resort to private alternatives to publicly funded government services. While some income targeting remains possible within a universalist framework, to offer extra support to those most in need, means testing is a form of targeting that stands contrary to the very idea of universality. Indeed, Bo Rothstein defines universalism precisely by the absence of means tests (1998). Korpi and Palme also understand means tests as a way to limit the universality of a program (1998). The same logic can apply to social services. When welfare state institutions compel citizens to pay for their services, or to buy private insurance to cover private costs, they introduce a sort of reverse means test, whereby those who can afford it are better protected. By contrast, an encompassing, universalist welfare state crowds out private insurance by providing good social services to all (Korpi and Palme, 1998).

Two recent OECD indicators capture these dimensions of universality: a measure of the percentage of social benefits that are means or income tested and a measure of the proportion of private spending in total social expenditures. The first indicator comes from a new OECD series on the percentage of cash transfers that are means or income tested. For this indicator, which constitutes a direct measure of institutional design, we rely on the OECD *Social Expenditures Update* (OECD, 2014), which covers the years 2000 to 2011.[1]

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[1] The means-tested programs considered by the OECD include: "spending on 'other contingencies - other social policy areas' as in the OECD Social Expenditure database (SOCX), income-tested spending on the unemployed (e.g.

Our second indicator measures the proportion of private spending in total social expenditures. This indicator has the advantage of reaching beyond transfers, to cover the services provided or not by the welfare state, like public health care services. Encompassing public services that benefit all citizens tend to crowd out private services and increase support for the welfare state. To build this indicator, we divide private social spending as a proportion of GDP by the sum of public and private social spending as a proportion of GDP (and then multiply by 100), using the data compiled in the OECD *Social Expenditures Database* (OECD, 2016a).[2] Because we have a relatively small N and the two indicators represent complementary dimensions of universalism, it seems logical to combine them into an integrated index of universalism, which is done with a factor analysis.[3]

This measure of universalism is not perfect. It captures only one of the three dimensions of universalism identified by Korpi and Palme, namely egibility rules. As can be seen in Figure 1, conservative welfare states such as Spain, Austria, Belgium, and Italy appear as relatively universalist countries even though their form of governance is segmented. This is the case, in part, because we operationalize universalism negatively, by the absence of means testing and of an important private component in social spending. Many segmented, Bismarckian, welfare states have shared these characteristics with social-democratic welfare states, even though they have relied largely on a fragmented patchwork of social insurance for different categories of workers. However imperfect, these Bismarckian social insurance programs have offered, in many countries, a nearly universal coverage of the population (Palier, 2010: 375). Our index is not sufficient to capture the difference between social-democratic and conservative welfare states,

unemployment assistance payments for Germany), income-tested support payments to elderly and disabled (e.g. Belgium and the UK), other income-tested payments (survivor payments, family cash transfers) but do not include spending on active labour market policies, housing or income-tested medical support" (OECD, 2014: 6).

[2] We include both mandatory and voluntary private social expenditures.

[3] See the online appendix for a description of the procedure used to create the index of universalism.

but it does set apart the countries that target the poor and rely more on private services, precisely the orientation that Korpi and Palme considered inimical to redistribution. This index also seems more plausible than measures that rank the United States as more encompassing than Sweden.

The example of the United States is useful to understand the difference between our index of universalism and a concentration coefficient. In the United States, pensions are proportional to earnings and represent a very large share of government social spending, largely because the overall level of public social spending is low. As a result, the American welfare state appears universal, when universalism is measured with a concentration coefficient (Kenworthy 2011). This welfare state, however, uses means tests for on a 2000-2011 average of 28.6% of its cash benefits, which is at the low end of the liberal welfare state category, but maintains the highest share of private social expenditures in our sample, with a 2000-2011 average of 38.7% (see Figures A1 to A4 in the Appendix). Combining these two measures in an index puts the United States where it belongs: near the bottom of the universalism scale.

Figure 1: Mean of universalism index, 20 OECD countries, 2000-2011
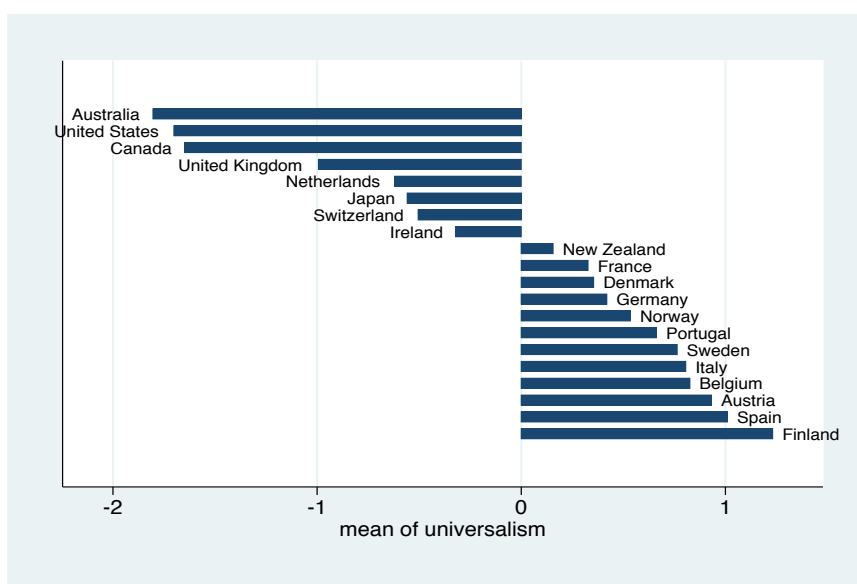
In our multivariate models, this measure of universalism is complemented by a few conventional control variables: logged GDP per capita (in US dollars at constant prices and purchasing power parity; OECD, 2016) to control for relative national wealth, which may have an impact on inequality (Roine et al., 2009); the unemployment rate as a percentage of civilian labour force, to control for the current economic and social situation (OECD, 2015c); and the old age (+65) dependency ratio (20-64), to control for the weight of pensions in welfare state expenditures (OECD, 2015d). While these variables have no obvious relationship with our indicator of universalism, they are likely to have an impact on our dependent variables.

The dependent variables measure the size of the redistributive budget, inequality, redistribution, and poverty reduction. First, in line with Korpi and Palme's argument, we include a measure of the size of the redistributive budget, as a key intervening variable between welfare institutions and redistribution. For this purpose, we use the OECD's measure for gross public social spending as a percentage of GDP (OECD, 2016a).

Then, we use both the level of disposable income inequality and a relative redistribution index constructed with Gini measures of pre-redistribution and post-redistribution inequality (the difference between market income Gini and disposable income Gini, divided by market income Gini and multiplied by 100; OECD, 2015e).[4] For poverty, we use a similar index comparing the rate of persons with an income below 50% of the median income before taxes and transfers to the same rate after redistribution (the difference between the market poverty rate and the welfare state poverty rate, divided by the market poverty rate and multiplied by 100; OECD, 2015e). These measures of redistribution and poverty reduction are not without difficulties.[5] They force

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[4] Unfortunately, the OECD does not provide Gini data for every country/year. These gaps in the data reduce our number of cases from 240 to 143.

[5] One may note that our measure of private social spending concerns services, while the measures of inequality, redistribution and poverty are all related to on to incomes, which are not directly connected to services. Our

us to assume, notably, that the pre-distribution outcomes are pure market outcomes, uninfluenced by welfare state institutions, which, of course, is unlikely (Esping-Andersen and Myles, 2009: 651-52). They nevertheless remain a reliable measure of redistribution and poverty reduction.[6] Because we do not use a transfer concentration index as an independent variable, but rather institutional indicators, these redistribution variables are not at risk of being contaminated by an independent variable that captures similar dimensions.

The time frame for our model is constrained by the availability of data for our first independent variable: the percentage of social benefits that are means tested, only compiled by the OECD for the years 2000-2011. This is a rather short period, but it is a crucial one to test our argument, since critics of the paradox of redistribution suggest that it stopped working in the twenty-first century. If we can demonstrate that universal institutions continue to reduce inequality and poverty in the first decade of this century, we can claim there is still a paradox of redistribution.

Even though some critics have extended the argument to newer welfare states in Eastern Europe, and even to developing countries outside the OECD, the paradox was originally conceived as a characteristic of class politics in well-established welfare states. For this reason, and because data are not available for all countries, we focus on the 20 classical welfare states of advanced democracies.[7]

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argument is that universal institutions increase redistribution and poverty reduction directly and indirectly, notably via the size of the social budget. Universal public services are part of this logic, even though their impact on incomes is indirect.

[6] The Luxembourg Income Study could also have been a pertinent source for redistribution data, but it provides less data points than the OECD. The data we choose are also readily and publicly available in the OECD database. In any case, the correlation between the LIS and the OECD measures of redistribution is very high, around 0.94 (Swank, 2015).

[7] Huber and Stephens estimate that it takes about twenty years "for democracy's effects on social processes to work its way through to income inequality" (2012: 148). In this perspective, it seems prudent not to include Eastern European countries in our set of cases.

Methods

We use a time-series cross-sectional (TSCS) approach to evaluate the impact of universalism on our four dependent variables. Surprisingly, given the currency of this statistical approach in comparative politics, this method has never been used to test the existence of a paradox of redistribution. Most studies testing the paradox of redistribution have used correlations or bivariate regressions, except for Brady and Bostic's article (2015), which is based on individual level observations.

The Breusch-Pagan test shows that a panel model with random effects (RE) is preferable to a simple OLS regression, but the Hausman test indicates that between-country and within-country effects are different in our case, suggesting that a fixed effects model (FE) is more appropriate than a random effects model (Bell and Jones, 2015). We have to recognize, however, that our universalism index, like most institutional variables used in comparative politics, varies little over time, as can be seen in Figure 2. In light of this reality, it appears fruitless to adopt a time-series cross-sectional design with FE, a procedure that precisely tracks within-case changes and controls out between country variance. In fact, such a procedure risks making substantive information disappear (Plumper and Troeger, 2007; Greene, 2012: 380; Bartels 2015; Bell and Jones 2015).

Figure 2: The evolution of universalism in 20 OECD countries, 2000-2011
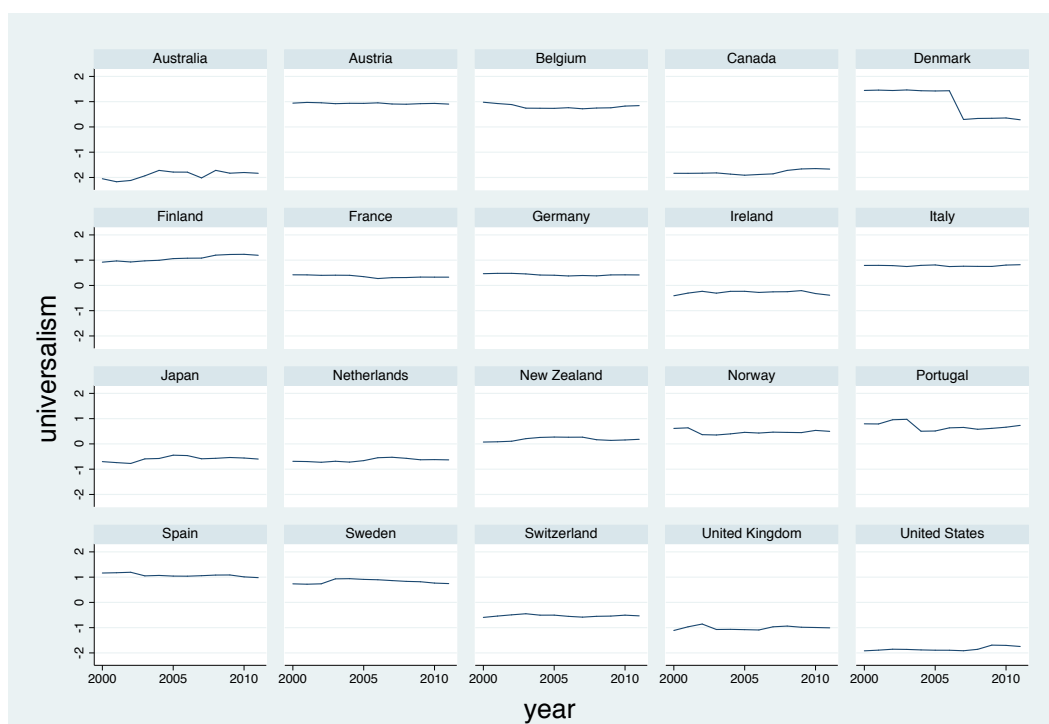
The best option is a random effect time-series cross-sectional model that distinguishes the between-country from the within-country effects (Bartels, 2015; Bell and Jones, 2015; Haddow, 2016). Classical random effects models assume that between and within-country effects are the same and that unobserved heterogeneity across countries is not correlated with the independent variables and remains random (Bartels, 2015; Bell and Jones, 2015; Greene, 2012: 379). Because most comparative political economy data violate these assumptions of random effects models, fixed effects models have become a gold standard in the discipline (Bell and Jones, 2015).

Beck and Katz (2007) have shown, however, that RE models perform well with TSCS data, especially in datasets where the number of countries (N) is larger than the number of years (T) (Bartels, 2015; Bell and Jones, 2015). In fact, if we separate explicitly the between-country

from the within-country effects[8], within-cluster transformations become uncorrelated with the between-cluster random effect, making unobserved heterogeneity across country independent from the covariates (Bartels, 2015; Bell and Jones, 2015). Contrary to classical RE models, we are not assuming that between and within country effects are the same, which allows a more meaningful discussion of substantive results (Bell and Jones, 2015). Indeed, it is possible that the effect of universalism plays differently across countries (the more universal a country is, the more redistribution it should achieve), and within countries (a change in universalism over time leads to more or less redistribution).

Considering the clustering of standard errors and a short, unbalanced panel of only 12 years, cluster robust time-series cross-sectional regression appears warranted (Bradley et al., 2003; Cameron and Trivedi, 2010: 239 and 273). The Wooldridge test for serial correlations shows first order auto-correlation, suggesting the use of a lagged dependent variable (Beck and Katz, 2011). Finally, because the method we use is fairly new in political science, we present more common country fixed effects models in the online appendix, along with a first differencing model and a fixed effect vector decomposition model to take care of the slowly varying nature of our main explanatory variable (Plumper and Troeger, 2007).

Lastly, we test the effect of universalism on public support for redistribution, an implicit relationship in Korpi and Palme's argument. To do so, we use the International Social Survey Programme 2006 survey on the "Role of Government". More specifically, we take the same question as that selected by Brady and Bostic (2015): "On the whole, do you think it should or should not be the government's responsibility to reduce income differences between rich and poor." Our measure represents the share of respondents in a given country answering, "definitely

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[8] To do so, we use the clustergen function in STATA, developed by Brandon Bartels to generate a country mean for every covariate (between effects). The within-country effects represent deviations in units of measurement from the cluster means.

should be" and "probably should be." As Brady and Bostic argue, this question directly measures preferences for redistribution whereas, "alternative questions about spending preferences are relative to each country's current spending, which makes them less cross-nationally comparable and conflates attitudes about government responsibilities with perceptions of the efficacy and efficiency of government programs and taxation" (2015: 79). Responses for this survey question are only available for 2006, allowing only a cross-sectional correlation.

Results: Still the Paradox

Consider, first, bivariate correlations between the main variables of interest, as shown in Table 1. All relationships are strong, significant, and in the expected direction. The universalism index is positively correlated with redistribution, poverty reduction and social expenditures and negatively correlated with inequality levels. The size of the redistributive budget, measured by public social expenditures, is also strongly correlated with redistribution and poverty reduction. Redistribution and poverty reduction are correlated together, as the literature would predict.

Table 1: Correlations between universalism and various measures of redistribution, 20 OECD countries, 2000-2011

	Universalism	Gini	Public social expenditures	Redistribution index	Poverty reduction
Universalism	1.000				
Gini	-.5981*	1.000			
Public social expenditures	.7266*	-.6067*	1.000		
Redistribution index	.6653*	-.7682*	.7071*	1.000	
Poverty reduction	.5604*	-.6858*	.6559*	.8867*	1.000

Note: The table shows correlation coefficients (r), * = significant at 0.01.

The same correlations also hold when we use a single year and a small N (20), as can be seen in Figures 3 to 5 for 2010.[9] Figure 3 shows a clear correlation between universalism and redistribution, revealing the direct effect universalism has on redistribution. In line with Korpi and Palme's argument, Figure 4 presents a strong correlation between universalism and the size of the public budget, measured by public social expenditures. Finally, Figure 5 shows that the size of the social budget is strongly correlated with redistribution. In the online appendix, we present a cross-sectional model at one point in time with our three control variables. The effect of universalism on redistribution, poverty reduction, inequality levels and social expenditures is significant and strong in this cross-sectional model as well.
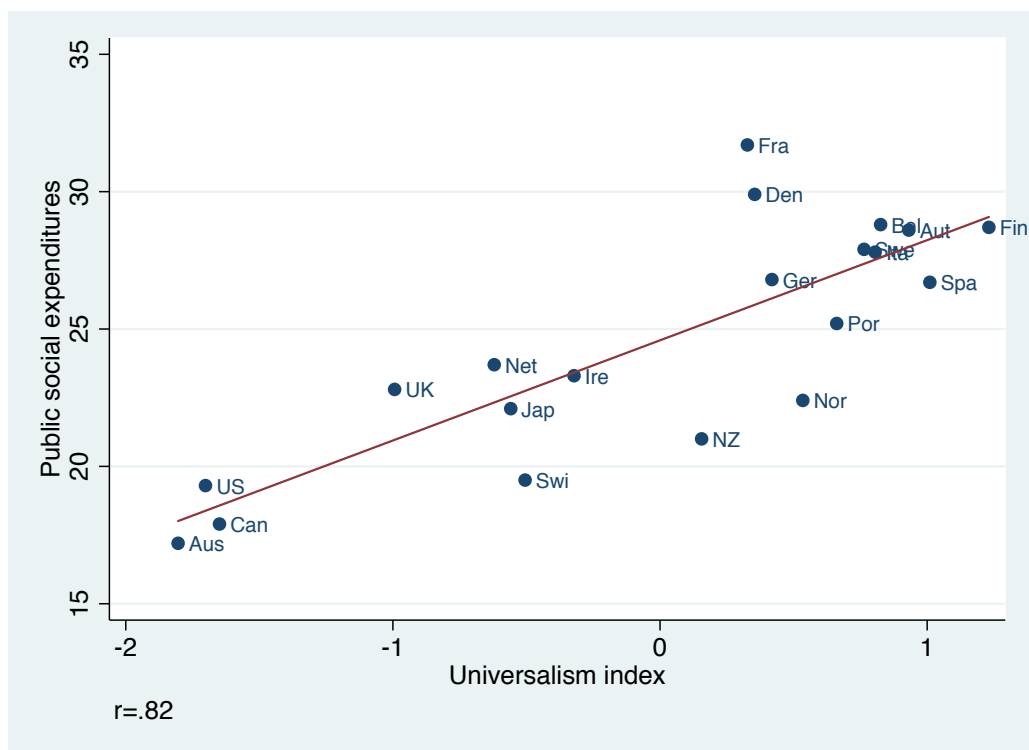
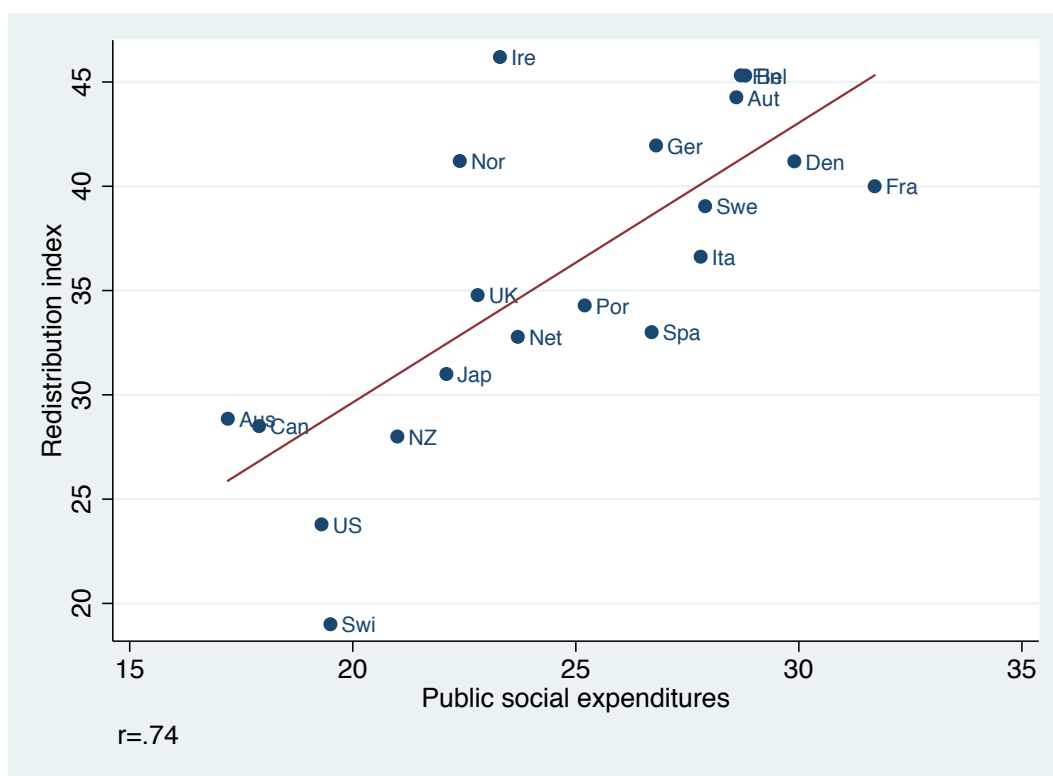**Figure 3: Universalism and redistribution, 20 OECD countries, 2010**
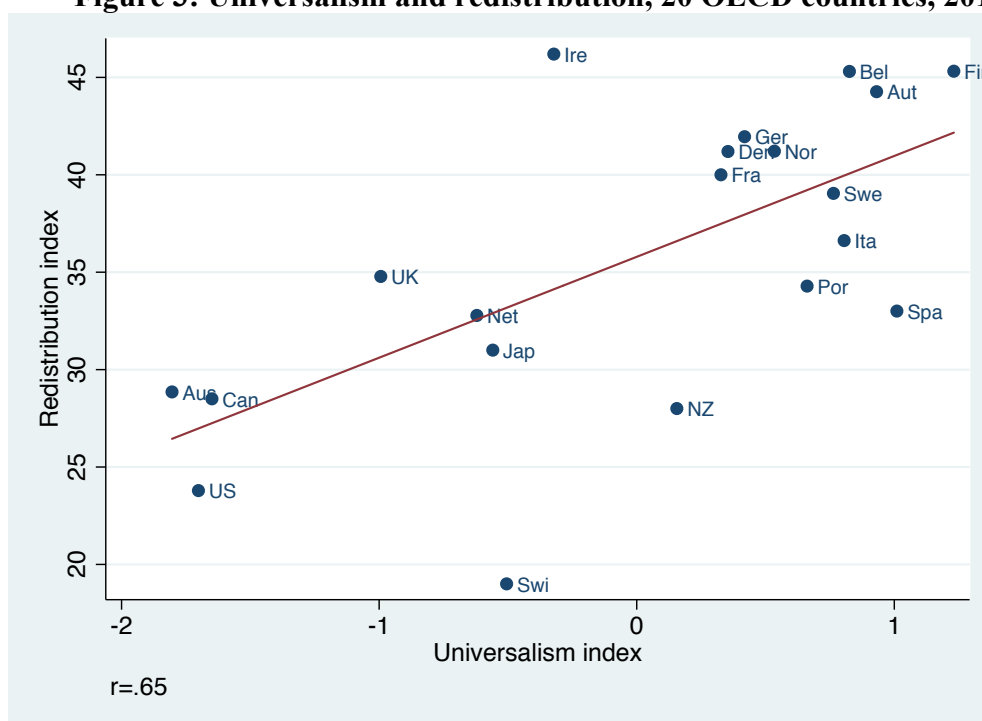
r=.65

Figure 4: Universalism and public social expenditures, 20 OECD countries, 2010

r=.82

Figure 5: Public social expenditures and redistribution, 20 OECD countries, 2010

r=.74

These bivariate results are consistent with Korpi and Palme's argument: universal programs entail a larger redistributive budget, which leads to better redistribution and more poverty reduction. By the standards that prevailed when Korpi and Palme published their study, and by those observed by Kenworthy (2011) and Marx, Salanauskaite and Verbist (2013), these results are already telling. They are indeed more compelling than the correlations Korpi and Palme and their critics obtained because, we would argue, the targeting/universalism dimension is more satisfactorily operationalized. It suggests that universalism increases redistribution directly, as well as indirectly via the size of the social budget.

Does the index of universalism predict changes in redistribution, poverty reduction and social expenditures between and within countries? The results presented in Table 2 are drawn from random effects models separating between- and within-countries estimations. Across countries, universalism comes out as a significant predictor of social expenditures, levels of inequality, redistribution, and poverty reduction, when controlling for GDP, unemployment, and the dependency ratio. Coefficients in Model 1 show that a one unit increase in universalism is associated with a 4.8 point increase in redistribution, which is roughly the difference in redistribution between the US and Canada in the mid-2000s. In fact, going from the lowest level of universalism to the highest is associated with a 17.45 points increase in redistribution.[10] Seventeen points in redistribution is equivalent to the difference between the redistributive effect of the Norwegian and the US welfare states. A one-unit increase in universalism is also associated with a larger redistributive budget, increasing public social expenditures as a

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[10] The 17.45 points increase in redistribution is the unstandardized coefficient we get when we transform the universalism index into a variable ranging from 0 to 1.

proportion of GDP by 2.8 percentage points. Similarly strong effects across countries exist for poverty reduction and inequality levels.

Table 2: Results for random effect models separating between and within effects

	(1) Redistribution	(2) Social exp.	(3) Poverty	(4) Gini
Lagged DV	0.559***	0.649***	0.236**	0.516***
	(0.118)	(0.0672)	(0.112)	(0.149)
Universalism (between)	4.803***	2.828***	6.776**	-0.0262***
	(1.341)	(0.544)	(2.719)	(0.00777)
Universalism (within)	1.047	0.469	1.433**	-0.00167
	(0.729)	(0.466)	(0.609)	(0.00281)
Unemployment (between)	-0.244	0.0405	-1.391	0.00411*
	(0.695)	(0.304)	(1.148)	(0.00213)
Unemployment (within)	0.120*	0.209***	0.662***	0.00116***
	(0.0717)	(0.0539)	(0.0861)	(0.000348)
Dependency (between)	0.129	0.320*	0.496	-0.000264
	(0.459)	(0.164)	(0.894)	(0.00119)
Dependency (within)	0.163	0.211**	-0.519***	-0.000553
	(0.144)	(0.0828)	(0.199)	(0.000669)
Logged GDP (between)	14.62**	3.249	14.72	-0.105***
	(6.848)	(3.328)	(13.19)	(0.0394)
Logged GDP (within)	-8.052**	-3.872	-4.359	0.0465***
	(3.413)	(2.931)	(6.686)	(0.0177)
Constant	-119.8	-20.34	-94.62	1.387***
	(73.35)	(35.13)	(143.9)	(0.424)
Observations	99	220	100	124
R^2	.65	.71	.47	.64

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Except for poverty reduction, the within-country effects are not significant in these models. This is not surprising since the dependent variable is strongly correlated with its lag, and our main independent variable varies slowly over time. The more conventional fixed effects model and other models presented in the online appendix do show significant within-country

effects for universalism. These results, however, are not robust to the exclusion of Denmark from the dataset, the only country that displays a large variation in universalism over time. In contrast, cross-validation of our random effect models show that the effects of universalism across clusters remain significant, whatever country we exclude.

The previous models analyze the direct effects of universalism on redistribution, social expenditures, poverty, and inequality, but we risk an omitted variable bias, since we know from Table 1 that public social expenditures have a strong effect on our other dependent variables. While they do not specify this relationship precisely, Korpi and Palme also consider that universalism influences redistribution largely through the size of the redistributive budget. To test this possibility, we run three new models in Table 3 with public social expenditures as a control variable. Model 5 shows that controlling for levels of public social expenditures, countries that are more universal have significantly higher redistribution. For a given level of social expenditure, for instance, a one-unit increase of universalism leads to 2.75 points in redistribution. Universalism is not statistically significant, however, for predicting poverty reduction and inequality when controlling for social expenditures (models 6 and 7). These results have to be interpreted in light of the strong correlation between universalism and social expenditures (0.73 in Table 1), which makes it difficult to incorporate both variables in the same model. They nevertheless suggest that the effect of universalism is in good part indirect, as Korpi and Palme first proposed. Social expenditures are indeed significant as an independent variable; both between and within countries, a result that may reflect the fact that they change more from year to year compared universalism. Since we know from Table 2 that universalism is a strong predictor of public social expenditures levels, we can conclude that it has both a direct between-country effect, shown in Table 2, and an indirect effect on poverty and inequality via the size of

the social budget. The argument that the more or less universal institutional design matters for distributional outcomes is reinforced by the statistically significant effect universalism has on redistribution, when controlling for the level of social expenditures.

Table 3: Results for random effect models separating between and within effects, with social expenditures as a control variable

	(5) Redistribution	(6) Poverty	(7) Gini
Lagged DV	0.549***	0.231***	0.528***
	(0.116)	(0.0675)	(0.158)
Universalism (between)	2.761**	2.437	-0.0134
	(1.318)	(2.485)	(0.00799)
Universalism (within)	0.707	0.552	-0.00224
	(0.679)	(0.647)	(0.00301)
Public social expenditures (between)	0.298**	0.713***	0.000230
	(0.140)	(0.171)	(0.000459)
Public social expenditures (within)	0.994***	2.106***	-0.00636***
	(0.272)	(0.399)	(0.00200)
Unemployment (between)	0.00578	-0.873	0.00216
	(0.585)	(0.963)	(0.00187)
Unemployment (within)	-0.0280	0.298*	0.00104**
	(0.0950)	(0.163)	(0.000465)
Dependency (between)	-0.477	-0.779	0.00385**
	(0.470)	(0.889)	(0.00156)
Dependency (within)	-0.0362	-0.999***	-0.000680
	(0.208)	(0.279)	(0.000789)
Logged GDP (between)	12.42**	10.05	-0.0907***
	(5.065)	(10.64)	(0.0250)
Logged GDP (within)	-7.841**	-3.957	0.0472***
	(3.279)	(6.573)	(0.0176)
Constant	-105.4*	-64.12	1.290***
	(54.18)	(116.7)	(0.270)
Observations	99	100	124
R^2	.70	.67	.72

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 6 completes the argument by showing a weak correlation, significant at the 0.1 level, between universalism and public support for redistribution in the 16 countries surveyed by the ISSP in 2006. Denmark is a clear outlier with high levels of universalism but very low support for redistribution. This anti-redistribution attitude of the Danes is a puzzle that goes beyond the scope of this paper. If we leave Denmark aside, there is a clear association between universalism and public support for redistribution (r=.72). Citizens living in countries with higher levels of universalism tend to maintain stronger support for government redistribution, as predicted by the paradox of redistribution.

Figure 6: Universalism and public support for redistribution, 2006
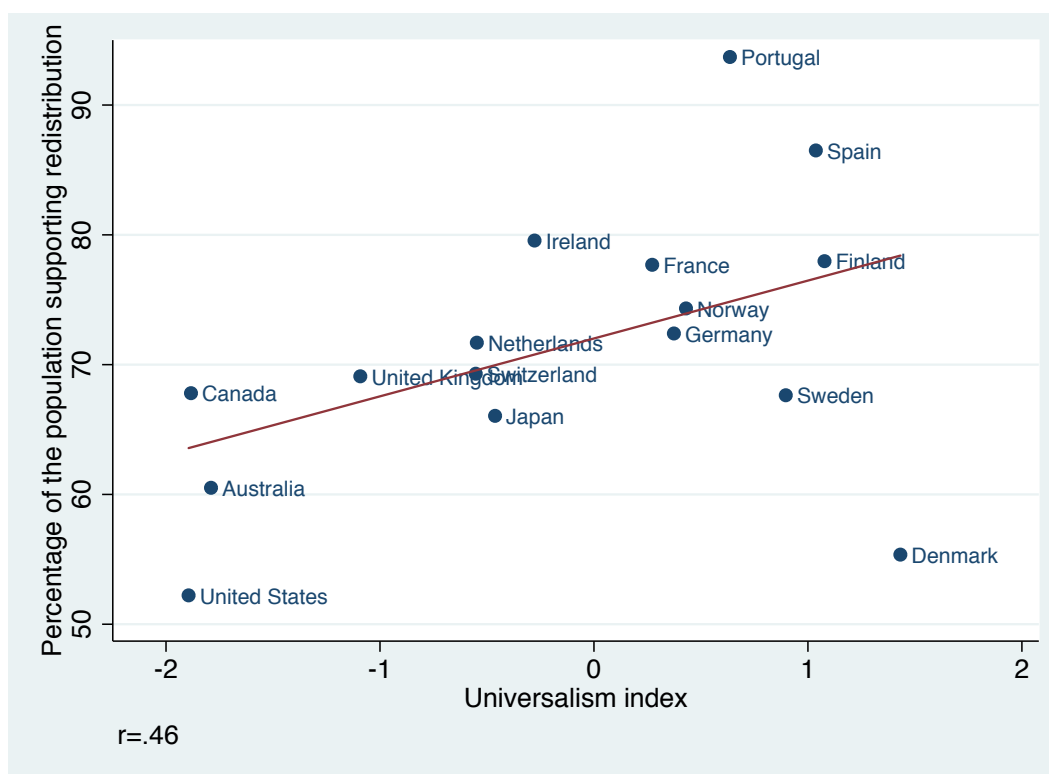
In a cross-national perspective, we have good reasons to think that the design of welfare state institutions, and more precisely their more or less universalist character, matter for

redistribution, the size of the redistributive budget, poverty reduction, and inequality levels. When placed in a macro-institutional perspective, where it belongs, the paradox of redistribution still seems relevant.

Conclusion

When they identified the paradox of redistribution, Walter Korpi and Joakim Palme started from the broad design of welfare institutions to draw out their general implications for the class politics of social policy. A predominance of encompassing programs, they argued, would sustain public support for redistribution and, hence, a large social expenditure budget. Well-financed universal programs would then generate more equality. On the contrary, a basic security model, with modest common programs and an emphasis on targeting, would lower public support for the welfare state, encourage private insurance solutions and, in the end, reduce the redistributive budget and leave society more unequal. Korpi and Palme, however, had limited evidence to back up their theoretical claims. They checked levels of expenditures in different welfare state models, examined the correlations between the size of the social budget and income redistribution, and considered the relationship between the generosity of public pensions and expenditures on private pensions. More importantly, in light of subsequent studies, they introduced a concentration index to create a measure of social transfers targeting, which proved to be a good predictor of income redistribution. As they did so, they appeared to identify the most direct connection between universalism and redistribution, providing the core test for their theory. This test would later be used to question the very existence of a paradox of redistribution, at least in this century.

Using a concentration coefficient for the targeting of transfer income, however, moved the argument away from welfare state institutions, and brought it closer to an argument about market and post-market outcomes. Market tendencies and demographic realities, in particular, shape outcomes, irrespective of institutional design. When families with children are poorer than average, for instance, a concentration coefficient makes a universal child benefit appear as targeted toward the poor. In this article, we propose to return to policy design and consider indicators that capture more directly the nature of welfare state institutions. We also improve upon the one-time correlations used by Korpi and Palme and most of their followers, with a novel random effects time-series cross-sectional design that distinguishes between and within-country effects. Our index of universalism, built by combining measures of means testing and private social expenditures, is not perfect, but it captures a key dimension of institutional variation across welfare states and is significantly related to income redistribution, the size of the redistributive budget, poverty reduction, and inequality levels. This effect is only significant in a cross-sectional perspective, not because within-country changes in the degree of universalism are not associated with changes in redistribution, but because our time period is too short and our main institutional variable varies too slowly to generate meaningful within-country estimates. In Denmark, one of the few countries that display substantial over time change in universalism, a reduction of universalism is associated with a decrease in poverty and inequality reduction, but this country alone drives the significant result of conventional fixed effect regressions.

These empirical tests suggest that, at the macro-institutional level, in the twenty-first century, the paradox of redistribution still operates. Countries where social programs are less anchored in universality have less generous redistributive budgets and are less effective in redistributing income and reducing poverty; countries with more encompassing welfare states

spend more on transfers and services and do more to redistribute and reduce poverty. To paraphrase Mark Twain, the reports of the paradox of redistribution's death may "have been greatly exaggerated."

These conclusions have several implications. First, of course, our findings suggest that the political argument for a universal welfare state still has value. Second, our analysis points to the importance of operationalization choices in comparative welfare state research. To assess the impact of political choices and institutions, we would argue, it is better to use variables and indicators that capture their institutional design directly, rather than appraise them indirectly, through outcomes. In this sense, our index of universalism is a step in the right direction to improve our measurement of welfare state institutions, even if it remains an imperfect proxy for a complex concept. Third, our results suggest that distinguishing between and within-country effects is a fruitful way to move beyond fixed effects models that cannot produce unbiased estimates for slowly changing variables such as institutions, which are critical in comparative politics.

Many questions remain open. First, the availability of data forced us to look at a rather limited number of countries, for a brief 2000-2011 period. Ideally, the time span should be expanded, to better assess Korpi and Palme's argument over time. Second, the positive relationship between universalism and public opinion should be verified also in a time-series setting to go beyond the cross-sectional correlation shown in this article. The basic difficulty, here, is again a paucity of comparative public opinion data that would cover more than a single year. More work remains to be done, then, to achieve a full model of the politics or redistribution in advanced welfare states. At the very least, however, we have established that the paradox of redistribution still operates in the twenty-first century. The redistributive gap between Norway

and the United States, for instance, is largely explained by our index of universalism. For those who favor redistribution and seek to reduce poverty, universalism appears to remain the best political strategy.

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Online Appendix

Building a universalism index

To build an index out of two indicators, two approaches are possible, one deductive and the other more inductive. The first approach assigns equal weights to each indicator, and standardizes, or "studentizes," each of them by setting their mean at 0 and their range at one standard deviation around the mean. The two z scores for means tested social expenditures and for share of private social expenditures are then added. Because we have two indicators for targeting, we obtain a score that is in fact a targeting index, which simply has to be multiplied by -1 to become a plausible index of universalism. The second approach, more inductive, consists in computing factor scores for the same two indicators. Factor scores also give a standardized value to each variable, with a mean of 0 and a standard deviation of 1. But they attribute a different weight to each variable, based on how central this variable is to determine the first factor, or main latent dimension. Rather than attributing a weight of one half to each indicator, factor scores determine weights according to each variable's impact on universalism. The results are then multiplied by -1, as with the first procedure. It turns out that the weights attributed to each indicator by factor analysis are relatively similar, and the two indices we compute come out very close, with a correlation of 0.9869 between them. Because they are inductive and data-driven, we retain the factor scores values to capture universalism. Tests indicate, however, that our results would be practically identical with a universalism variable based on a simple addition of standardized scores.

Figure A1: Means testing and redistribution, 20 OECD countries, 2010.
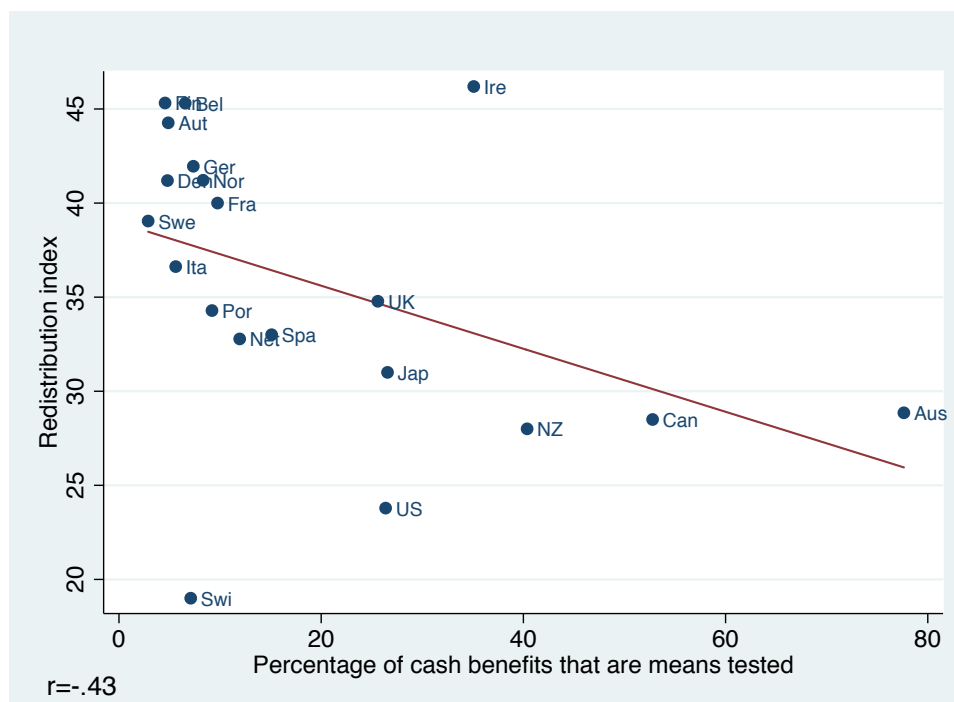

Figure A2: Means testing and public social expenditures, 20 OECD countries, 2010.
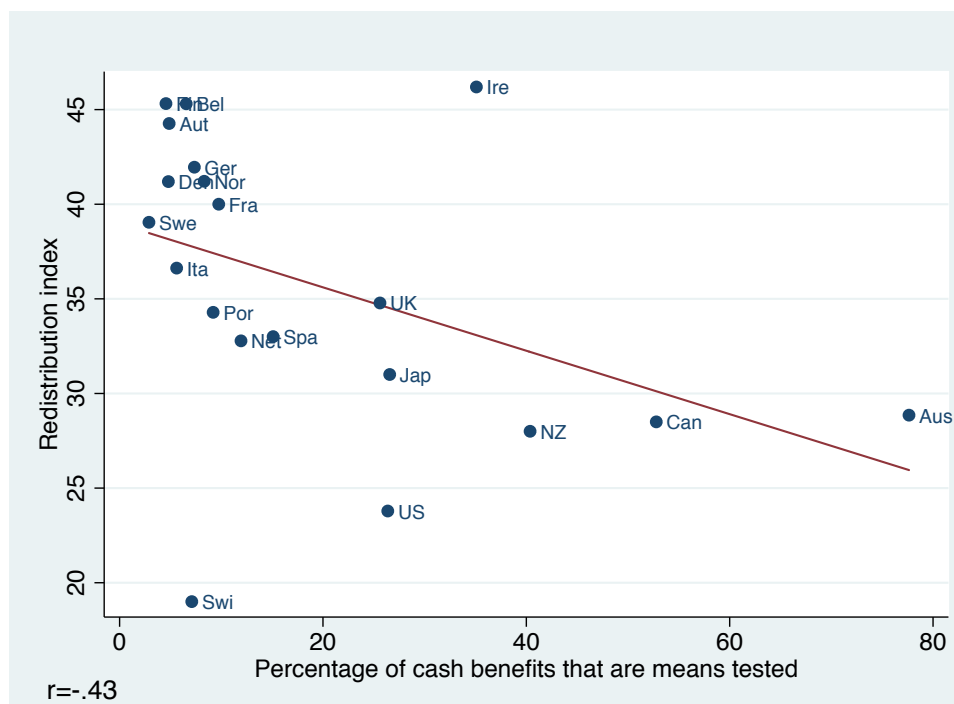
Figure A3: Share of private social expenditures and redistribution, 20 OECD countries, 2010.
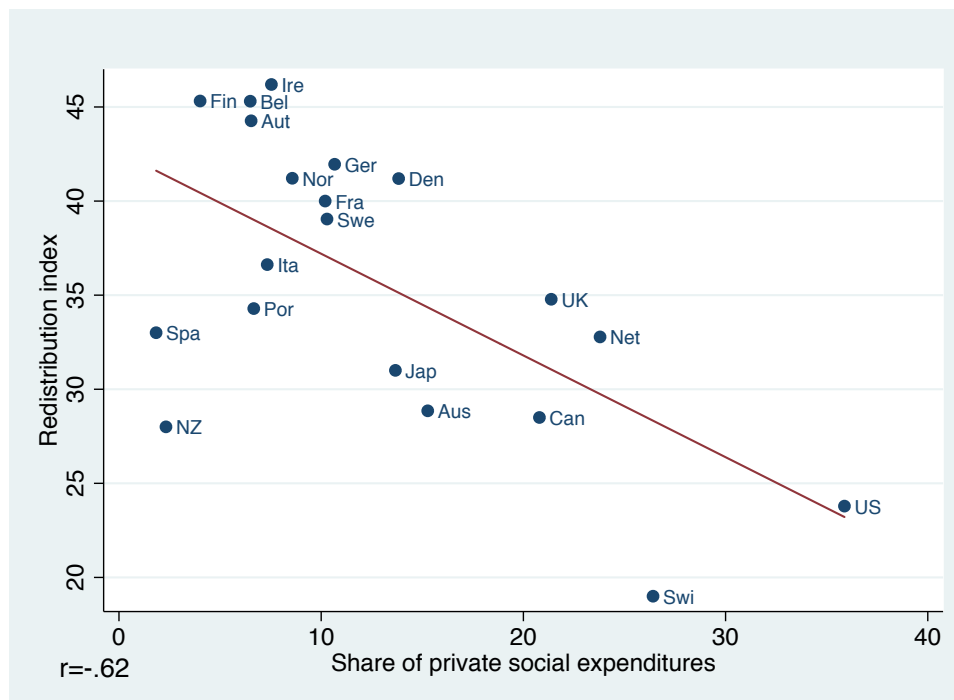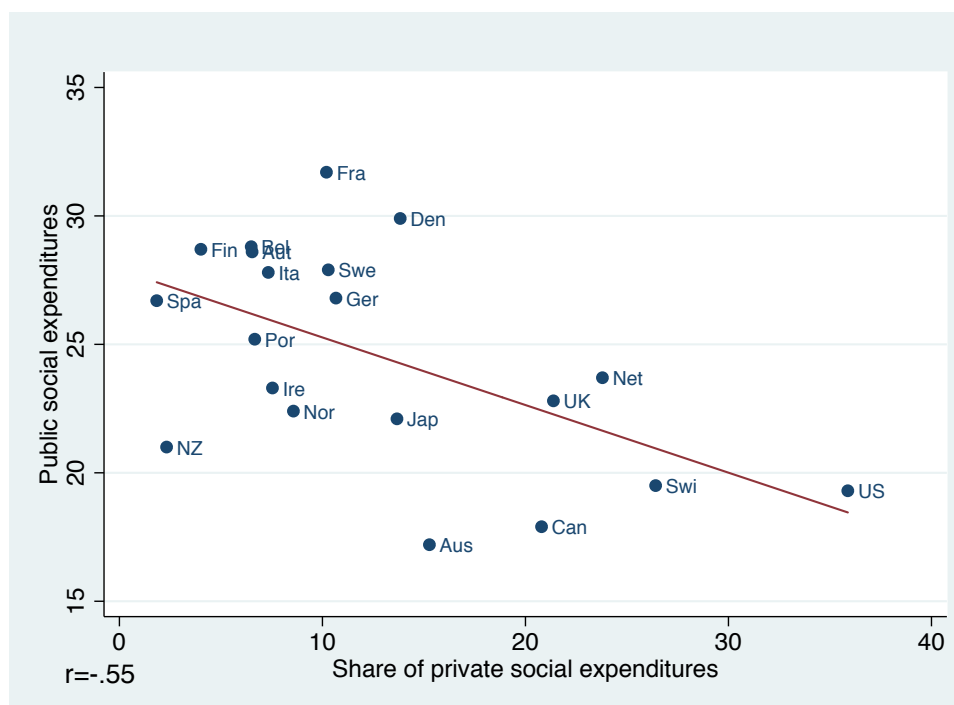

r=-.62

Figure A4: Share of private social expenditures and public social expenditures, 20 OECD countries, 2010.

r=-.55

Table A1: Fixed effects regressions with AR(1) disturbances

VARIABLES	(1) Social exp.	(2) Redistribution	(3) Poverty reduction	(4) Gini
Universalism index	0.979**	3.261***	6.037***	-0.0154***
	(0.381)	(0.843)	(1.595)	(0.00514)
Logged GDP	-2.722	2.457***	4.987***	0.0252***
	(4.188)	(0.532)	(0.927)	(0.00274)
Dependency ratio	0.460***	0.275	0.281	0.000534
	(0.0997)	(0.223)	(0.396)	(0.00118)
Unemployment rate	0.549***	0.266***	0.703***	0.00155***
	(0.0937)	(0.0940)	(0.186)	(0.000517)
Constant	34.85	1.482***	-1.600*	0.0109***
	(44.20)	(0.440)	(0.955)	(0.00347)
Observations	240	123	124	142
R-squared	0.932			
Country FE	YES	YES	YES	YES
Year FE	NO	NO	NO	NO
PCSE	YES	NO	NO	NO

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Model 1 in Table A1 uses panel corrected standard errors (PCSE) with Prais-Winsten regression. All models include country fixed effects. The use of a lagged dependent variable in a model with fixed effects could lead to a Nickel bias, because the country dummy of the fixed effect induces a correlation between the error term and the lagged dependent variable (Beck and Katz, 2011). Thus, because of the Nickel bias and the slow variation over time of our universalism index, we present fixed effect models without lagged dependent variables, but we use the command xtregar for models 2-4, to correct for serial correlation.

Table A2: Cross-sectional regressions, 2010

VARIABLES	(1) Poverty reduction	(2) Redistribution	(3) Gini	(4) Social exp.
Universalism	9.183***	5.743***	-0.0308***	3.202***
	(2.497)	(1.208)	(0.00664)	(0.580)
Logged GDP	3.668	6.864	-0.0880**	1.059
	(22.47)	(12.14)	(0.0314)	(4.635)
Unemployment	-0.124	0.0579	0.00378**	0.132
	(1.068)	(0.642)	(0.00129)	(0.190)
Dependency	-0.506	-0.118	0.000829	0.165
	(0.727)	(0.396)	(0.000773)	(0.145)
Constant	39.13	-34.08	1.182***	7.789
	(257.0)	(138.2)	(0.352)	(51.71)
Observations	20	20	20	20
R-squared	0.345	0.449	0.758	0.697

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3 : First differencing models with robust standard errors

	(1)	**(2)**	**(3)**	**(4)**
VARIABLES	Δ Redistribution	Δ Poverty reduction	Δ Social exp.	Δ Gini
Δ Universalism index	1.987***	1.504	0.853**	-0.00617***
	(0.588)	(1.239)	(0.349)	(0.00195)
Δ Logged GDP	-13.62***	-5.000	-30.54***	0.0570**
	(4.501)	(12.02)	(1.755)	(0.0274)
Δ Dependency ratio	-0.374	-1.419	-0.332*	0.00103
	(0.440)	(0.897)	(0.190)	(0.00128)
Δ Unemployment rate	0.104 (0.0983)	0.607*** (0.183)	0.137*** (0.0307)	0.00196*** (0.000429)
Constant	0.482**	0.366	0.665***	-0.000876
	(0.239)	(0.414)	(0.0698)	(0.000937)
Observations	99	100	220	124

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4 : Xtscc models to correct for serial correlation, cross sectional dependence and heterosckedasticity, using Driscoll-Kraay standard error correction (8 maximum lag)

	(1)	(2)	(3)	(4)
VARIABLES	Redistribution	Poverty reduction	Social exp.	Gini
Universalism index	3.867***	3.929***	0.878**	-0.0123***
	(0.422)	(0.345)	(0.308)	(0.00204)
Logged GDP	-14.98***	-32.33***	2.947	0.0419***
	(4.215)	(6.860)	(3.429)	(0.00801)
Dependency ratio	0.370***	0.173	0.396***	0.000739**
	(0.0609)	(0.234)	(0.102)	(0.000310)
Unemployment rate	0.352***	0.633***	0.614***	0.000865***
	(0.0655)	(0.0802)	(0.0265)	(0.000180)
Constant	182.1***	396.9***	-22.98	-0.165*
	(43.40)	(66.86)	(34.13)	(0.0789)
Observations	143	144	240	162
Country FE	YES	YES	YES	YES
Year FE	NO	NO	NO	NO

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A5 : Fixed effect vector decomposition model

VARIABLES	(1) Redistribution	(2) Poverty reduction	(3) Social exp.	(4) Gini
Logged GDP	-14.98	-32.33	2.947	0.0419
	(12.94)	(23.20)	(1.834)	(0.0650)
Dependency ratio	0.370	0.173	0.396***	0.000739
	(0.362)	(0.644)	(0.0586)	(0.00197)
Unemployment	0.352	0.633	0.614***	0.000865
	(0.261)	(0.494)	(0.0447)	(0.00169)
Universalism	3.141**	4.766*	2.175***	-0.0228***
	(1.432)	(2.548)	(0.586)	(0.00620)
eta	1.000	1.000	1.000	1.000
	(0)	(0)	(0)	(0)
Constant	182.1	396.9	-22.98	-0.165
	(137.9)	(247.2)	(18.87)	(0.715)
Observations	143	144	240	162
R-squared	0.950	0.953	0.942	0.959

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A6 : Pooled OLS with year and country dummies, using robust standard errors

VARIABLES	(1) Redistribution	(2) Poverty reduction	(3) Social exp.	(4) Gini
Universalism	4.430***	4.839***	-0.0123***	1.141***
	(0.949)	(1.239)	(0.00350)	(0.274)
Logged GDP	-35.76***	-59.83***	0.0419**	-16.43***
	(8.699)	(15.95)	(0.0197)	(3.812)
Dependency	0.236	0.190	0.000739	-0.00905
	(0.218)	(0.384)	(0.000566)	(0.0862)
Unemployment	0.271**	0.614***	0.000865*	0.331***
	(0.121)	(0.213)	(0.000476)	(0.0512)
Constant	409.6***	684.6***	-0.162	189.0***
	(92.60)	(168.2)	(0.206)	(40.73)
Observations	143	144	162	240
R-squared	0.956	0.957	0.959	0.966

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1