PARADOXES LOST AND FOUND: THE DIMENSIONS OF SOCIAL WELFARE TRANSFERS, RELATIVE POVERTY AND REDISTRIBUTION PREFERENCES*

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ABSTRACT

Korpi and Palme's (1998) classic "The Paradox of Redistribution" contends that universalism reduces poverty while targeting worsens poverty because of the politics of welfare states. Though there have been fundamental changes to social policy, politics, and inequalities since the mid-1980s period KP analyzed, very few have reinvestigated their study. With data on the distribution of welfare transfers received, we develop measures of extensity (transfers as a percent of household income), low-income targeting (the low-income concentration of transfers) and universalism (the homogeneity of transfers across the population). We examine how these country-level dimensions associate with individual-level poverty and redistribution preferences. We also systematically compare the results within rich democracies and a much broader sample of developed and developing countries. While some results are consistent with Korpi and Palme, there are also key differences. Poverty is significantly negatively associated with extensity and universalism. Low-income targeting is not robustly associated with poverty, but is surprisingly negatively signed. Redistribution preferences are only significantly negatively associated with low-income targeting. We also show that while universalism is strongly associated with extensity, low-income targeting is also surprisingly positively associated with extensity and universalism in the broader sample. Therefore, we revise the paradox of redistribution into two new paradoxes. First, there is a clear mismatch between what reduces poverty and what matters to redistribution preferences. Second, in developing countries, the dimensions that best reduce poverty correlate with the one dimension that undermines support for redistribution. We conclude by discussing the implications for research on inequality, politics and social policy.

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One of the most important articles in recent decades of social policy research is Korpi and Palme's (1998) classic: "The Paradox of Redistribution." Korpi and Palme, henceforth KP, investigate the enduring question of whether social policies should be targeted at the poor or universally distributed to all. Considerable literature contends targeting more efficiently concentrates scarce resources on those most in need and better aligns with poverty-reducing incentives like encouraging work (Barry 1990; Besley 1990; Kakwani and Subbarao 2007; Le Grand 1982; Saez 2006; Tullock 1997). Critiquing such scholarship, KP counter-intuitively argue the more countries target welfare transfers at the poor, the less poverty is reduced. Greater equality results when transfers are distributed universally rather than concentrated on the most vulnerable. The reason is that universal social policies are more popular, which results in larger welfare states. KP have been central to many literatures, and along with the intellectual currents they showcased, have oriented and inspired a great deal of scholarship.[1]

Despite the clear significance of KP, there have been substantial advances in methods, data, and theory that could improve our understanding of the paradox of redistribution. Moreover, there have been fundamental changes to social policy, politics, and inequalities since the mid-1980s period KP analyzed. Nevertheless, in the 15 years since KP, very few have reinvestigated their precise questions or results (Kenworthy 2011; Marx et al. 2013). We update and expand beyond KP's 11 rich democracies in the mid-1980s. By including all countries in the

[1] According to Google Scholar, KP has more than 1,150 citations. This appears to be the most cited article in the welfare state literature published since 1998. Articles with similar citations only exist before 1998 (e.g. Lewis 1992; Orloff 1993; Pierson 1996). These highly cited articles and many classics from the past few decades (Esping-Andersen 1990; Huber and Stephens 2001; Korpi 1989; Skocpol 1992) share many arguments with KP (e.g. universalism's superiority over targeting, critiques of welfare effort, and feedback effects).

Luxembourg Income Study (LIS) and International Social Survey Programme (ISSP), we examine larger samples of rich democracies and broader samples of developed and developing countries in the mid-2000s. Building on KP and advances in the literature, we conceptualize and measure three dimensions of received welfare transfers. With multi-level models, we then examine the relationships between these country-level dimensions and individual-level poverty and redistribution preferences. Foreshadowing our analyses, several results are consistent with KP. However, there are also key differences. Therefore, we ultimately propose a revision of the paradox of redistribution.

THE PARADOX OF REDISTRIBUTION

Building their argument step by step, KP demonstrate low-income targeting increases poverty and weakens redistribution preferences while universalism reduces poverty and increases redistribution preferences. They contend that different "institutional structures" (e.g. social insurance regimes) enable countries to exercise different "strategies of equality" (i.e. targeted or universal). These strategies create different risks and resources, which produce different interests and identities. These identities and interests are pivotal for KP because they lead to political coalitions. Specifically, KP (1998: 663) argue targeting "splits the working class and tends to generate coalitions between better-off workers and the middle class against the lower sections of the working class." They also write, "The targeted model creates a zero-sum conflict of interests between the poor and the better-off workers and the middle classes who must pay for the benefits of the poor without receiving any benefits. . .[targeting] drive[s] a wedge between the short-term material interests of the poor and those of the rest of the population" (KP 1998: 672). Conversely, universalism "brings low-income groups and the better-off citizens into the same

institutional structures. . .can be expected to have the most favorable outcomes in terms of the formation of cross-class coalitions. . .[and] pool[s] the risks and resources of all citizens and thus create[s] converging definitions of interest" (KP 1998: 672, 682).

The political coalitions that emerge from these interests and identities then drive "redistributive budget size." Redistributive budget size, measured as the population average of transfers as a percent of household income, ultimately predicts poverty and inequality. KP critique past research: "focused almost exclusively on how to distribute the money available for transfer and has largely ignored variations in the size of the redistributive budget" (KP 1998: 672). KP further stress: "[W]e can expect a tradeoff between the degree of low-income targeting and the size of the redistributive budget size, such that *the greater the degree of low-income targeting, the smaller the redistributive budget*. . . it is impossible to maximize both the degree of low-income targeting and budget size" (emphasis in original, KP 1998: 672). By contrast, universal welfare states "are expected to generate the broadest base of support for welfare state expansion and the largest budget size" (KP 1998: 672).

KP use LIS data on 11 rich democracies in the mid-1980s.[2] Their evidence includes macro-level patterns and correlations, maximum pension replacement rates, and indices of transfer targeting and redistribution derived from LIS micro-analyses. They show redistributive budget size increases redistribution and reduces poverty and inequality. Further, they demonstrate low-income targeting of transfers reduces redistributive budget size and redistribution, and increases poverty and inequality. KP (1998: 681-682) conclude: "The more we target benefits at the poor. . .the less likely we are to reduce poverty and inequality."

[2] Australia, Canada, Finland, France, West Germany, the Netherlands, Norway, Sweden, Switzerland, U.K., and U.S.

Beyond their core arguments, KP deeply shaped the literature in at least two other ways. First, like the emerging conventional wisdom at the time, KP crystallize the view that the quality of welfare programs matter more than the quantity of welfare effort (also Esping-Andersen 1990; Korpi 1989). Though scholars had long studied welfare effort (e.g. welfare spending as a percent of GDP) [Wilensky 2002]), KP and others fault it for conflating welfare generosity with the needs and composition of the population (e.g. welfare effort mechanically grows with population aging) (Scruggs 2008). As Esping-Andersen (1990: 19) writes, welfare effort is "epiphenomenal to the theoretical substance of welfare states." Utilizing their Social Citizenship Indicators Project and linking it to the then recently available LIS, KP encouraged a lot of subsequent research by demonstrating the value of measuring the precise criteria and rules of welfare programs rather than the more crude welfare effort.[3]

Second, KP demonstrate one of the more salient institutionalist arguments in the welfare state literature. Since at least the early 1990s, scholars have stressed how social policies feedback into preferences and politics (Fernandez and Jaime-Castillo 2013; Gingrich and Ansell 2012; Huber and Stephens 2001; Pierson 1996; Rothstein 1998). As Skocpol (1992: 531) remarks, "Policies not only flow from prior institutions and politics; they also reshape institutions and politics, making some future developments more likely, and hindering the possibilities for others." By explaining how targeting and universalism construct the interests, identities and coalitions supporting social policies (pp.664-665), KP illustrate and provide evidence of feedback effects and path dependency (Pierson 2004; Nelson 2007).

[3] KP also included a typology of social insurance regimes: targeted, voluntary state-subsidized, corporatist, basic security, and encompassing. Probably partly because their typology was similar to others (Esping-Andersen 1990; Huber and Stephens 2001), it has received less attention.

ADVANCES IN SOCIAL POLICY RESEARCH

As mentioned above, advances in the literature have the potential to enhance our understanding of the paradox of redistribution. First, and most obviously, the data and methods available to scholars have improved considerably. Cross-national surveys now include a much larger and more diverse set of countries. Such surveys enable one to analyze the individual-level preferences theorized but unobserved by KP (i.e. identities and interests). Relatedly, multi-level models can now be utilized for modeling the effects of both individual- and country-level factors for poverty and redistribution preferences.

Second, because data is available on many more countries, we can assess whether the paradox of redistribution generalizes to a larger share of the world's population and countries. Though there is substantial variation within rich democracies, there is even greater variation when incorporating developing countries. Even though many of these are not democracies, developing countries also have social policies and their citizens have preferences (Golden and Min 2013; Haggard and Kaufman 2008; Mares and Carnes 2009). Indeed, the very first social insurance programs were established in non-democratic Germany and Austria. In developing countries "the overwhelming number of social insurance programs were initially adopted by nondemocratic governments" (Mares and Carnes 2009: 97). While there is less research on social policy on developing/non-democratic countries than on rich democracies, there has been growing interest in political science and development economics, and in international institutions like the World Bank (Golden and Min 2013; Haggard and Kaufman 2008; Huber and Stephens 2012; Mares and Carnes 2009; Rudra 2007).

Third, much has been learned from precise program criteria and official rules (Esping-Andersen 1990; Nelson 2004; Scruggs 2008; Scruggs and Allan 2006). Nevertheless, to better

understand social policy, more research is needed on how much households actually "take up" and receive welfare transfers (Currie 2004; Van Oorschot 1991). This has the advantage of identifying households that while eligible for benefits do not actually enroll and/or receive benefits. Indeed, there is abundant evidence households routinely do not receive the transfers they are legally entitled to receive (Bansak and Raphael 2006; Currie 2004; Shaefer 2010). Administrative on-the-ground implementation of social policies has often curbed the intended generosity of welfare programs (Piven and Cloward 1993; Schram et al. 2009). Thus, actually received transfers provide an essential complement to program criteria. Partly, this is because measures based on program criteria are forced to selectively concentrate on a few tractable programs such as unemployment or old age insurance. Unfortunately, spending on different programs are not well correlated across countries, and the focus on particular programs obscures the distinctive mixes and emphases of different countries (Castles 2008). The reality is that households have a variety of strategies to pool a variety of transfers to make ends meet (Edin and Lein 1997; Rainwater and Smeeding 2004). Because the LIS measures all cash and near cash transfers, it is possible to comprehensively capture the entire distribution of interdependent transfers actually received (Wilensky 2002).

DIMENSIONS OF WELFARE TRANSFERS

Building on KP and advances in the literature, we consider three dimensions of welfare transfers: extensity, low-income targeting, and universalism. Our measures of extensity and low-income targeting follow but revise KP's measures. Universalism is a novel measure that we propose as a third dimension that is actually distinct from (not simply the opposite of) low-income targeting. We conceptually and operationally define each dimension and hypothesize

how each relates to poverty and redistribution preferences. To make the dimensions more concrete, Table 1 displays countries exemplifying the dimensions of transfers.

[TABLE 1 ABOUT HERE]

Extensity

We rename and slightly revise KP's "redistributive budget size" into the simpler "extensity." Extensity is conceptualized as the average degree to which household income is publicly provided or socialized. Extensity is measured as the mean percent of household income resulting from welfare transfers. As Table 1 shows, transfers amount to a mean 49 percent of household income in Sweden. Low-income households receive a very high share of income from transfers, middle-income households receive a fairly high share, and even high-income households receive a moderate share.[4] By contrast, in Colombia, transfers only amount to a mean 7 percent of household income. Low- and middle-income households receive only a small share of their income from transfers, while high-income households receive a moderate share.

Extensity can be thought of as the household-level version of welfare effort (Wilensky 2002). Indeed, in the 29 countries with data (see below), extensity correlates .70 with the OECD's standard measure of welfare effort (social welfare expenditures as a percent of GDP). Just like welfare effort, extensity rises because a country has generous welfare programs, but also because a household has recognized needs (e.g. being elderly or unemployed), mechanically increasing transfers relative to market income. Though Korpi (1989), Esping-Andersen (1990), and many others criticize welfare effort, KP actually demonstrate extensity is pivotal to poverty

[4] The 49 percent includes the entire population while Table 1 only displays select points in the income distribution. Sweden's low-income households' transfers exceed equivalized income because we measure transfers before taxes (e.g. taxes on social insurance pensions). Below, we discuss the need for research indexing transfers by taxation (Ferrarini and Nelson 2003).

and inequality.[5] In fact, recent LIS research shows that welfare effort predicts inequality and poverty quite well, and possibly even better than more sophisticated program-based measures like decommodification (Brady 2009; Moller et al. 2003; but see Scruggs 2008).

We expect extensity to be negatively associated with poverty for at least two reasons. First, household income is composed of: A) less equally distributed market income and B) more equally distributed public transfers. As the share of household income shifts from A to B, inequality and poverty should mechanically decline (Huber and Stephens 2012). Also, as B grows relative to A, public transfers crowd out private pensions and capital income, further reducing inequality and poverty (KP 1998; Huber and Stephens 2012). Second, even though extensity and effort are criticized for conflating needs and generosity, this criticism obscures the social construction of need. Welfare states make political choices about which "needs" receive public support. It is a political choice for welfare states to automatically spend money on the unemployed or elderly, and to not automatically spend money on other risk groups. Thus, by automatically raising effort/extensity in response to particular needs, welfare states make political choices to recognize those needs and socialize those risks. If countries politically choose to expand the risks that are socialized, greater extensity and lower poverty should result. Further, greater extensity should lead to less poverty for all regardless of which risks are socialized. For instance, Brady and Burroway (2012) demonstrate extensity, not generous benefits specifically for single mothers, best explains cross-national variation in single mother poverty.[6]

[5] Despite Esping-Andersen's criticisms of effort, there are also similarities between extensity and decommodification. Decommodification entails relief from having to commodify or sell one's labor on the market (Esping-Andersen 1990; Scruggs and Allan 2006). By definition, greater extensity means a greater share of the typical household's income does not come from selling labor on the market (i.e. from public transfers).

[6] Brady and Burroway (2012) refer to extensity as the "universal replacement rate." We call it extensity to distinguish from universalism and replacement rates.

In the broader sample, the relationship between extensity and poverty is less certain. On one hand, extensity's effects may be weaker as social policies in developing countries are typically exclusive (Haggard and Kaufman 2008; Huber and Stephens 2012; Mares and Carnes 2009). Developing countries often contain a relatively privileged, formally-employed elite with access to public employment and welfare programs, and informally employed masses excluded from such programs (Portes and Hoffman 2003). Therefore, extensity, measured as the population average, might conceal a highly skewed dualization of transfers (Emmenegger et al. 2012). If so, extensity should be less effective. On the other hand, extensity's effects might be even stronger in the broader sample. Higher extensity may be empirically necessary to incorporate low-income households into social policies. Thus, extensity may reflect how much social policies have expanded beyond a privileged elite to include the poor.

We expect extensity to increase redistribution preferences. The literature on path dependency shows large welfare states set expectations that reinforce the popularity of social policy (Campbell 2012; Huber and Stephens 2001; KP 1998; Larsen 2008; Nelson 2007; Pierson 1996; Sachweh and Olafsdottir 2010; Svallfors 2007). This is partly because high extensity means a larger pool of beneficiaries and stakeholders. These constituencies have an interest in programs' maintenance and expansion (Pierson 2004). Also, high extensity should reflect and amplify egalitarian norms (Brooks and Manza 2007). Because there has been less research on how extensity shapes preferences in developing countries, we expect the relationship to be similar in the broader sample.

Low-Income Targeting

Low-income targeting is the disproportionate concentration of welfare transfers on low-income households. This definition follows the targeting efficiency literature and gauges how

much eligibility for transfers is means-tested (Barry 1990; Besley 1990; Creedy 1996; Le Grand 1982; Mkandawire 2005). A large literature measures targeting with an index of concentration assessing the distribution of transfers on the pre-transfer income distribution (Kakwani and Subbarao 2007; KP 1998). Targeting is typically justified because it is efficient, and with tight budgets, it focuses scarce resources on the neediest (Blank 1997; Greenstein 1991). Low-income targeting avoids "leakage" – when the affluent or middle class are the primary beneficiaries of transfers (Tullock 1997). Purportedly, targeted programs also avoid disincentives to poverty-reducing behaviors like work and marriage (Saez 2006). As illustrated in Table 1, Australia targets to low-income households more than others. Middle-income households receive more than twice the transfers of high-income households, and low-income households receive about 1.8 times the transfers of high-income households.

Often the literature, including KP, treats universalism as the opposite of low-income targeting and places countries on a continuum from targeted to universal (Kenworthy 2011; KP 1998: 670-671). However, transfers can be targeted to low- or high-income households, and targeting to high-income households is common in developing countries (Huber and Stephens 2012; Mares and Carnes 2009). Therefore, the opposite of low-income targeting is high-income targeting not universalism. For instance, until recently, China's social policies mainly benefitted public sector and urban formal sector workers (Ringen and Ngok 2013). Indeed, Table 1 shows that in 2002, Chinese high-income households received about 15 times more transfers than middle-income households and about 66 times more transfers than low-income households.

According to KP (1998: 677), low-income targeting should be positively associated with poverty. Partly, this is because targeting should result in lower extensity. In addition, though advocates highlight targeting's efficiency, there are several unanticipated ways it ultimately

devotes less resources to actual assistance. Targeting requires monitoring and screening, is administratively expensive, and often results in arbitrary and discriminatory exclusion of beneficiaries and lower take-up (Currie 2004; Piven and Cloward 1993; Rothstein 1998; Schram et al. 2009). Contrary to the targeting efficiency literature, some argue targeting counterproductively discourages work and poverty-reducing behavior by forcing unreasonable choices between employment and welfare (Edin and Lein 1997). As a result, low-income targeting should be positively associated with poverty.

As explained by KP, targeting is unpopular and should undermine redistribution preferences (also Moene and Wallerstein 2001; Skocpol 1991). Low-income targeting stigmatizes risk groups, splits the working class, drives a wedge between the poor and others, and discourages broad coalitions supporting programs (Skocpol 1992). Scholars of American social policy often explain public reluctance to support social policy as an interaction of racial prejudice and the targeting of welfare on the "undeserving" poor (Katz 2001; Quadagno 1994; Wilson 1999). Notably, economists advocating targeting often neglect the unpopularity of targeting and the potential feedbacks into politics (Blank 1997; Saez 2006; but see Sen 1995). So, even within the targeting efficiency literature, there has been little rebuttal to claims that targeting is unpopular and weakens redistribution preferences.

In a broader sample of countries, the effects of low-income targeting might differ from the sample solely containing rich democracies. In developing countries, transfers are often biased in favor of middle- or upper-class insiders and expanding coverage could require reaching downwards in the income distribution (Huber and Stephens 2012). Therefore, targeting transfers to the bottom-half or even bottom-two-thirds of the income distribution may better remedy poverty and be more popular in developing countries.

Universalism

Universalism is one of the most widely used concepts in the literature. However, surprisingly few actually define this concept (Bergh 2004). KP do not appear to define universalism, but refer to "[u]niversal programs covering all citizens . . . encompassing all citizens in the same program. . .All citizens in the same programs. . .low-income groups and the better-off citizens in the same institutional structures" (KP 1998: 669, 672). Esping-Andersen (1990: 25) alludes to universalism as: "All citizens are endowed with similar rights, irrespective of class or market position." He (1990: 69) also characterizes the socialist regime as universal because it "exhibit[s] the lowest level of benefit differentials." Rothstein (1998: 19) describes universalism as uniform rules, non-means-tested benefits, and coverage of the entire population throughout different stages of life.

Accordingly, we define universalism as homogeneity across the population in benefits, coverage and eligibility (Bergh 2004).[7] We propose a novel measure of universalism as the inverse of the coefficient of variation in transfers. To understand this measure, consider the classic universal program of a guaranteed basic income. If the guarantee provides the same exact amount to every resident, by definition, there will be homogeneity in benefits. Even if other programs provide supplementary transfers, the guarantee would lift the floor, and less heterogeneity would result than in the absence of the guarantee. Therefore, measuring the homogeneity in transfers effectively gauges the uniformity of benefits, coverage and eligibility.

As noted above, universalism is not simply the opposite of low-income targeting. Nor is universalism simply an absence of low- or high-income targeting. While targeting involves

[7] Esping-Andersen (1990: 71-73) also seems to embrace the homogeneity of benefits in his measure of universalism – assessing the ratio of basic to maximal benefits and the equality in benefits. Recall KP simply present universalism as the opposite of low-income targeting.

heterogeneous benefits across the pre-transfer income distribution, there could be heterogeneity by sex, race, age, citizenship or other categories (Lewis 1992; Orloff 1993). These categories are never perfectly associated with the income distribution. Imagine societies C1 and C2 have identical low-income targeting and equal distributions of age across the income distribution. If there are large age differences in transfers in C1 but no age differences in C2, C2 would be more universal even though C1 has identical low-income targeting. Indeed, as we demonstrate below, universalism and low-income targeting do not correlate in a way consistent with capturing the same dimension. Thus, universalism distinctively involves homogeneity of benefits in general, and across any and all categories (not just the income distribution).

Table 1 compares transfers across urban and rural areas to illustrate universalism. On average, both rural and urban households receive transfers of approximately 59,000 kronor in the Czech Republic. In Mexico, the average rural household receives less than 3,000 pesos of transfers and the average urban household receives almost 4,400 pesos. There is much more universalism in the Czech Republic where urban households only receive about 1 percent more transfers than rural households. By contrast, Mexican urban households receive about 46 percent more transfers than rural households.

Universalism should reduce poverty more effectively than targeting (KP 1998; Nelson 2004). Like targeting, universalism should have a reduced form relationship with poverty because of the intervening effect on extensity. Thus, omitting extensity, we expect a negative relationship between universalism and poverty. Universalism should crowd out private insurance and capital income, increasing equality (Huber and Stephens 2012). Further, universalism should reduce poverty because it delivers more resources to actual assistance, and avoids the administrative and supervisory costs required by targeting. Universalism also better addresses the

heterogeneous risks that vulnerable households face. People become eligible for targeted programs only after falling into poverty, while universalism reduces the chances and costs of risks (e.g. illness), and thus prevents descents into poverty (Krishna 2007). Universalism thus reduces poverty because it protects all citizens from a wide variety of insecurities and risks. Just as some are skeptical targeting actually reduces disincentives to work and poverty-reducing behavior, scholars have argued universalism removes such disincentives. For example, Lindert (2004) shows the historical rise of universalism from the 19th through 20th century reduced work disincentives because a greater percent of the population shared basic rights to public services like health care. Finally, the literature on developing countries has made similar arguments about universalism's effectiveness in reducing poverty (Mares and Carnes 2009: 106; Huber and Stephens 2012). Thus, we have similar expectations for the broader sample.

The principal reason universalism should reduce poverty is because it is politically popular and leads to supportive coalitions (Esping-Andersen 1990, 1999; Gingrich and Ansell 2012; KP 1998; Larsen 2008; Nelson 2007; Rehm et al. 2012; Rothstein and Uslaner 2005; Skocpol 1992). Following the same logic as in extensity, universalism should feed back into the politics of social policy. Universalism implies all are equal stakeholders and constituencies of beneficiaries, who have a rational interest in and likely normative commitment to maintaining social policies (Pierson 2004). Also, universalism lessens the stigma of being a recipient of transfers (Katz 2001; Skocpol 1991; Wilson 1999). Therefore, universalism should be positively related to redistribution preferences. Finally, less research on social policy in developing countries evaluates the popularity of universalism. In a rare study that does, Huber and Stephens (2012) demonstrate universalism facilitates Leftist parties' electoral success and the expansion of social policy. Therefore, we have similar predictions for the broader sample.

METHODS

The analyses are conducted in two stages. The first predicts individual poverty as a function of country-level dimensions of welfare transfers and individual-level characteristics. The individual-level data is the Luxembourg Income Study (LIS), and the unit of analysis is an individual of any age. The second stage predicts individual redistribution preferences as a function of country-level dimensions of transfers and individual-level characteristics. The individual-level data is the International Social Survey Program (ISSP), and the unit of analysis is an individual adult. Descriptive statistics are displayed in Appendix I.

In each stage, we initially examine all rich countries with available data that have been stable, free democracies for more than two decades. Then, we examine all countries with available data, regardless of development or democracy. Because some LIS countries are not available in the ISSP, the samples differ across stages (see Appendix II). The first contains analyses of 21 rich democracies (N=1,064,628) and 38 countries (N=1,973,625). The second contains analyses of 16 rich democracies (N=15,887) and 25 countries (N=26,752). We analyze the rich democracies separately to be comparable with KP. In addition, Huber and Stephens (2012) show it takes 20 years for a democracy to enable public opinion and preferences to cohere into party mobilization for/against social policy. So, while it is valuable to assess generalizability in a broader sample, it is also useful to analyze rich democracies separately.

As explained below, both dependent variables are binary. Due to the clustering of individuals within countries and the inclusion of country-level variables, standard logistic regression is inappropriate. Therefore, we utilize multi-level logistic regression models. We estimate random-intercept models that can be expressed as two equations (Raudenbush and Bryk 2002). First, the log odds of a dependent variable ($\log(p_{ij}/1-p_{ij})$) for the *i*th individual in the

jth country is represented by eta (η_{ij}) and is a function of country intercepts (β_{oj}), and a set of fixed individual-level characteristics (βX_{ij}):

$$\log(p_{ij}/1 - p_{ij}) = \eta_{ij} = \beta_{0j} + \beta X_{ij}$$

Second, each country intercept (β_{0j}) is estimated as a function of a general intercept (γ_{00}) and a set of country-level variables (γC_j) and an error term (u_{0j}):

$$\beta_{0j} = \gamma_{00} + \gamma_0 C_j + u_{0j}$$

Because even the broader sample contains a limited number of countries, we focus on random intercepts models and mostly treat the individual-level variables as fixed effects. Due to the limited number of countries and the occasionally high correlation between country-level dimensions of transfers (see Figure 3), it is essential to be parsimonious at level 2 (Stegmueller 2013).[8] As a result, we mostly analyze dimensions separately.

Despite these limitations, multi-level analyses have at least two advantages over the macro-level approach used by KP (also Kenworthy 2011; Marx et al. 2012). First, multi-level models condition covariation in the dependent and level-2 variables by the individual-level variables. Failing to adjust for individual-level characteristics might conflate the level-2 effects with unobserved differences in population heterogeneity. Because macro-level analyses have limited degrees of freedom, it would be impossible to condition on all the country-level aggregates of the individual-level independent variables. Therefore, a multi-level analysis should result in more accurate estimates of level-2 effects. Second, a consensus seems to have emerged that multi-level models more efficiently estimate level-2 effects than macro-level models. Multi-

[8] While a random intercept model only estimates the intercept variance, even one random slope estimates three parameters: the intercept variance, the slope variance, and (with an unstructured covariance matrix) the covariance between the intercept and slope. Two random slopes results in five parameters. Thus, random coefficients quickly exhaust the level-2 units.

level models do so by estimating level-2 effects while sharing individual-level information between countries (Gelman and Hill 2006).

Country-Level Measures of Dimensions of Welfare Transfers

The data on the dimensions of transfers are from the LIS. The LIS is an archive of nationally-representative individual-level datasets on income and related variables in over three-dozen countries. The LIS is arguably the best available source for measuring dimensions of transfers because the datasets: a) contain fine-grained information on a variety of transfers; [9] b) are nationally-representative; and c) are cross-nationally harmonized. This is one of the first studies to include every LIS country. The one exception is Brazil, which lacks information on the essential individual-level variable marital status. Therefore, we only include Brazil in the figures, and are forced to omit it from the multivariate models. Mostly, we use datasets from the mid-2000s (see Appendix I) because this allows us to place the country-level measures slightly prior to the observation of redistribution preferences in 2006. If a dataset was not available for the mid-2000s, we included a dataset as early as 2000 and as late as 2008.

The key measures used to calculate the dimensions of transfers are household public transfers and household income (see Appendix VI). For transfers, we use the standardized LIS measures of the value of total government assistance received as cash and near cash transfers (Rainwater and Smeeding 2004). This includes monetary social insurance, monetary universal transfers, and (monetary and non-monetary) social assistance. Like KP (see their fn. 6), we are not able to include services. We measure disposable household income after taxes and transfers

[9] For example, the underlying India Human Development Survey has questions on public employee, old age, widows and disability pensions; scholarships; the national maternity scheme; the Annapurna scheme; the value of land received from the government; assistance to build housing, latrines, and cookstoves; ration cards; and income from any government source (see p.15 of http://ihds.umd.edu/IHDS_files/ihdshhq.pdf).

using the standardized LIS variable "DHI." Transfers and income are equivalized by dividing by the square root of the number of household members. Population-weights are also used.

Extensity is the mean of transfers as a percent of disposable household income. As noted above, this is basically KP's "redistributive budget size." We differ only in that KP measure transfers as a percent of pretax gross income. We contend transfers matter relative to ultimate disposable income after taxes and transfers. Plus, in several countries, income data is only available post-tax (i.e. net not gross). *Low-Income Targeting* is the Kakwani concentration coefficient of transfers based on the distribution of pre-transfer equivalized household income.[10] This is the same measure KP use. The Kakwani index ranges from -1, which indicates the poorest person receives all transfers, to +1, which indicates that the richest person receives all transfers. We reverse code the index such that +1 is maximal low-income targeting. *Universalism* is calculated as 1 over the coefficient of variation of transfers. This measures the homogeneity in transfer amount received across the population.

In analyses available upon request, we experimented with interactions of the three dimensions. For example, Esping-Andersen (1999: 79) argues social democracies are more egalitarian because of the "fusion" of generosity and universalism. Also, KP (1998: 672) imply that redistribution is a function of the interaction of low-income targeting and extensity. Nevertheless, all interaction effects were insignificant for both dependent variables.

[10] An alternative calculates the ratio of the poor's mean transfers over the mean transfers of the non-poor (Marx et al. 2012). However, such ratios are perhaps more useful when measuring targeting on a binary group (e.g. single mothers, Brady and Burroway 2012). Also, there is a circularity for poverty as the level of transfers defines the size and composition of the poor (and thus affects both the definition of who is poor as well as the level of transfers in each group). Nevertheless, such a binary ratio is positively associated with our index (r=.75 in 39 countries and r=.34 in 21 rich democracies).

Individual-Level Measures for Poverty Analyses

The first dependent variable is relative *poverty* (poor=1). One is poor if s/he resides in a household with less than 50% of the median equivalized income after taxes and transfers. This is the same measure KP used. This measure follows the vast majority of international poverty research and better captures prevailing conceptualizations of poverty, such as social exclusion and capability deprivation (Brady 2009; Moller et al. 2003; Rainwater and Smeeding 2004). Relative poverty is most relevant to the paradox of redistribution and more consistent with redistribution preferences. The paradox is concerned with relative deprivation and inequality within a given income distribution. It is also difficult to construct a cross-nationally reliable absolute measure across this diverse of a set of countries.[11] Further, absolute poverty is likely driven by economic development not dimensions of transfers. Thus, although absolute poverty could be explored in future research, we concentrate on relative poverty.

Following previous research (e.g. Brady and Burroway 2012; Moller 2008), the models adjust for individual- and household-level variables. Both *age and age*^{2} are in years for the lead earner in the household. Family structure is measured with binary variables for *single mother, female head no children,* and *male head no children.* Married and single father households are the reference.[12] We also include the number (*#*) *of children* and the number of elderly (*# over 64)* in the household. With secondary degree or some college as the reference, education of the lead earner is measured with binary variables for less than a secondary degree (*low education)* and

[11] For instance, one could construct a threshold appropriate for distinguishing poor from non-poor in the U.S. and then convert to other countries using purchasing power parity. However, such a threshold would be far too high for e.g. China, India and even Eastern Europe. Even a threshold constructed for the median country would not capture poverty in the richer and poorer countries. Further, basic needs thresholds like one or two dollars per day would identify almost no poor people in many countries.

[12] These categories are collapsed due to the small number of single fathers in many countries.

university degree or higher (*high education)*. Finally, we measure household employment with binary variables for *no workers in HH* and *multiple workers in HH* (one worker=reference).

Individual-Level Measures for Redistribution Preferences Analyses

The data on redistribution preferences are from the ISSP's 2006 "role of government" module. The ISSP is a set of standardized, nationally representative surveys from several dozen countries. The second dependent variable is the question: "On the whole, do you think it should or should not be the government's responsibility to reduce income differences between rich and poor?" Response categories were originally ordinal as: "definitely should be, probably should be, probably should not be, and definitely should not be." These are collapsed into the binary of should be (1) and should not be (0).[13] We concentrate on this question for four reasons. First, it most directly assesses the preference for redistribution (Cusack et al. 2008), which is paramount for KP. Second, international scholars have mainly focused on the responsibility questions and even on this particular question. Third, the alternative ISSP spending questions are relative to each country's current spending, which makes them less cross-nationally comparable and conflates attitudes about government responsibilities with perceptions of efficacy and efficiency of government programs and taxation (Svallfors 2006: 82). Finally, redistribution preferences are substantively important. Past research shows this question predicts party affiliation (Cusack et al. 2006), and the aggregate of this question is associated with inequality (Kelly and Enns 2010, Lupu and Pontusson 2011) and welfare generosity (Brooks and Manza 2007). In the ISSP, redistribution preferences also significantly increase the odds of Left party affiliation.

[13] In addition to ample precedent in the literature, we dichotomize for three reasons. First, it is unlikely that "definitely" and "probably" have consistent meanings cross-culturally. Second, there is little meaningful variation between probably or definitely should not be. Third, the ordinal versions fail the parallel regression test in ordinal logit models.

Following previous research (Brooks and Manza 2007; Cusack et al. 2008; Margalit 2013; Sachweh and Olafsdottir 2010; Svallfors 2006, 2007), the models adjust for several individual-level variables.[14] *Age* and *age*^{2} are in years. With secondary degree or some college as the reference, we include indicators for less than a secondary degree (*low education*) and a university degree or higher (*high education*). *Female* is coded as one. Marital status is measured with binary variables for *never married, divorced,* and *widowed* (married=reference). We also include household size (*HH size)* and a binary indicator for *children in the HH*. Binary indicators for *suburb/town* and *rural* are in reference to urban. Labor market status is measured with binary variables for *part-time employment, unemployed, not in the labor force, self-employment,* and *public employment* (private full-time=reference). To ensure cross-national comparability without currency conversion, *relative income* is measured with country specific z-scores. Finally, with no religious attendance as the reference, we include *low religious attendance* ("less than one a year" or "about once or twice a year") and *high religious attendance* ("several times a year" or more).

RESULTS

Poverty Analyses

We begin with the bivariate associations between poverty and the dimensions of welfare transfers. This allows one to directly compare our patterns with KP. Figure 1 plots the macro-level patterns in poverty in rich democracies (column A) and the broader sample (column B).[15]

[FIGURE 1 ABOUT HERE]

[14] Unfortunately, the ISSP does not make it feasible to use the same individual-level variables as in the LIS poverty analyses.

[15] The correlations are similar if we only include KP's 11 countries. Poverty correlates -.92 with extensity, -.34 with low-income targeting, and -.60 with universalism. This is the case although the countries not included by KP have significantly greater poverty ($t=2.35$, $p<.05$).

As Figure 1 reveals, extensity is strongly negatively correlated with poverty in rich democracies (r=-.77) and the broader sample (r=-.73). Denmark and Sweden have extensity near 50 percent – public transfers are almost half of the average household's income – and poverty rates near 5 percent. By contrast, the U.S.'s extensity is near 25 percent and Peru's extensity is below 10 percent. More than 15 percent of the U.S. and 25 percent of Peru is poor.

Surprisingly, low-income targeting is strongly negatively associated with poverty (r=-.54 in rich democracies and -.82 in broader sample).[16] Contrary to KP, Denmark and the Netherlands concentrate transfers on low-income households and have lower poverty. Israel and Spain are less concentrated on low-income households and have higher poverty. Although often framed as low-income targeted, the U.S. is actually not particularly so (also Kenworthy 2011; Marx et al. 2012). While no rich democracy is below zero on the targeting index, several developing countries have negative values, indicating a bias towards high-income households. For example, China and Colombia concentrate transfers on high-income households and have high poverty.

Universalism is also strongly negatively correlated with poverty (r=-.51 in rich democracies and -.75 in the broader sample). Sweden and Norway have high universalism and low poverty, while the U.S. has lower universalism and higher poverty. In the broader sample, countries like Peru and Colombia stand out for very low universalism and high poverty.

Table 2 displays the multi-level models of poverty. We report odds ratios for individual-level variables and standardized odds ratios for the country-level dimensions.[17] Models 1-3 include the rich democracies, and models 4-6 include the broader sample. Across models, and

[16] Again, unlike KP, we reverse code the index of targeting such that high values indicate a greater degree of low-income targeting.

[17] Standardized odds multiply the coefficient by the standard deviation of the independent variable (see Appendix I) and then exponentiate. We interpret the magnitude of odds less than one in terms of inverse odds (-1/odds) or inverse standardized odds.

partly because of the large sample, the individual-level variables are very robustly significant. Poverty is patterned by age, family structure, education and employment.

[TABLE 2 ABOUT HERE]

Consistent with KP, the first three models show extensity and universalism have significant negative effects, and extensity has the largest effect of the dimensions of transfers. For a standard deviation increase in extensity, the odds of poverty decline by a factor of 2.02. This effect is larger than the effects of being a single mother or female lead no child household. The effect of extensity is similar in magnitude to the effect of low education and slightly smaller than the effect of high education. For a standard deviation increase in universalism, the odds of poverty decline by a factor of 1.45. This effect is larger than the effect of an additional child and similar to the effect of being a male lead no child household.

Contrary to KP, low-income targeting exhibits a significant negative effect in rich democracies. For a standard deviation increase in low-income targeting, the odds of poverty decline by a factor of 1.31. This effect is slightly larger than the effect of an additional child. Though low-income targeting has a smaller effect than extensity or universalism, there is surprisingly less poverty with greater low-income targeting.

In the broader sample, extensity and universalism remain significantly negative, though the effects are not quite as large. Extensity continues to have the largest effect of the dimensions of transfers. For a standard deviation increase in extensity, the odds of poverty decline by a factor of 1.49. This effect is similar to being a female- or male-led household with no children, and larger than the effect of an additional child. For a standard deviation increase in universalism, the odds of poverty decline by a factor of 1.34. This effect is larger than the effect of having an additional child.

In the broader sample, low-income targeting is again negatively signed but is not significant. Still, the lack of a significant positive effect remains inconsistent with KP. Because low-income targeting is insignificant in the multilevel model for the broader sample, the initial strong negative macro-level association in Figure 1 (r=-.82) appears to be partly explained by population composition. This illustrates the value of adjusting for individual characteristics with multi-level models.[18]

Redistribution Preferences Analyses

Figure 2 displays the bivariate associations between the proportion supporting redistribution and the dimensions of transfers.[19] Unlike poverty, extensity is not very correlated with redistribution preferences. In rich democracies there is no relationship (r=-.02) as countries with the highest extensity (Sweden and Denmark) have similar support for redistribution as countries with the lowest extensity (Japan and the U.S.). In the broader sample, there is only a weak negative association (r=-.19). Thus, despite extensity's strong inverse relationship with poverty, it is not associated with redistribution preferences.

[FIGURE 2 ABOUT HERE]

Low-income targeting is negatively associated with support for redistribution (r=-.28 in rich democracies), and especially in the broader sample (r=-.51). For example, Australia and Switzerland target more to low-income households, and have less support for redistribution. By

[18] We assessed robustness by reestimating the models while dropping individual countries. The results were mostly robust. Extensity and universalism remain significantly negative in both samples in all models. Targeting was always significantly negative for rich democracies. Though mostly insignificant, targeting rarely became significantly negative in the broader sample.

[19] The correlations are different if we only include KP's sample of 11 countries. The proportion supporting redistribution correlates .67 with extensity, .13 with low-income targeting and .43 with universalism. This is partly because the countries not included by KP have significantly higher support for redistribution (t=2.52, p<.01), significantly less extensity (t=-2.55, p<.05), and significantly less low-income targeting (t=-3.46, p<.01).

contrast, Taiwan and South Africa target more to high-income households, and have higher support for redistribution.

Universalism is not very associated with support for redistribution (r=.07 in rich democracies and -.15 in broader sample). Among countries with the greatest support, there are highly universal (Hungary) and non-universal countries (Taiwan). Among countries with the least support, there are highly universal (Czech Republic) and non-universal countries (U.S.).

Table 3 shows the models of redistribution preferences. Consistent with past research, several individual-level variables are significant. Females, the never married, those with larger households, the less-educated, the unemployed and public employees are significantly more likely to support redistribution. Respondents with higher incomes or with children, and the self-employed are significantly less likely to support redistribution.

[TABLE 3 ABOUT HERE]

In rich democracies, extensity and universalism are not significantly associated with redistribution preferences. As in Figure 2, the two dimensions that predict poverty are unrelated to redistribution preferences in rich democracies. The lack of a significant positive effect for universalism is not consistent with KP.

In rich democracies, low-income targeting is negatively signed but not quite significant (z=-1.63). However, low-income targeting becomes statistically significant if we omit either Japan (z=-3.05) or the U.S. (z=-2.09). Such a negative relationship between low-income targeting and redistribution preferences is consistent with KP.

In the broader sample, extensity and universalism remain insignificant. Low-income targeting is now significantly negatively associated with redistribution preferences. For a standard deviation increase in low-income targeting, redistribution preferences decline by factor

of 1.48. This effect is larger than the effects of any individual-level variable except low education (for which it is comparable). Consistent with KP, the greater the degree of low-income targeting, the less support there is for redistribution.[20]

Relationships Between Dimensions

Central to the paradox of redistribution are the relationships between the dimensions of transfers. Figure 3 shows the bivariate associations between the dimensions. KP claimed universalism leads to extensity, and we find these two are strongly positively correlated in both samples ($r>.7$). Because both significantly reduce poverty, it could be that universalism's effects are mediated by extensity – as KP claimed. However, as the two are so highly correlated, this is difficult to sort out in multi-level models with this limited number of countries. Still, the complementarity of universalism and extensity confirms KP.

[FIGURE 3 ABOUT HERE]

The heart of KP's paradox is a tradeoff between low-income targeting and extensity. KP (1998: 672) write, "the greater the degree of low-income targeting, the smaller the redistributive budget [i.e. extensity]. . . it is impossible to maximize both the degree of low-income targeting and budget size." Thus, low-income targeting should undermine extensity. Surprisingly, however, Figure 3 reveals low-income targeting and extensity are very positively correlated. Contrary to KP, countries targeting transfers to low-income households have more extensity (also Marx et al. 2012). This is partly because countries with high-income targeting have very

[20] The results were mostly robust when dropping one country at a time. Extensity remains insignificant in 39 of 41 models, but is significantly negative if we drop South Korea or the U.K. from the broader sample. As noted, targeting is occasionally significantly negative in rich democracies. Targeting remains significantly negative in all 25 models in the broader sample. Universalism remains insignificant in both samples omitting any country.

low extensity. As countries incorporate the poor into social policies, and transfers become less high-income targeted, the welfare state tends to grow.

As discussed above, scholars often treat targeting and universalism as opposites. We explain that the opposite of low-income targeting is high-income targeting, not universalism. Also, we contend that while targeting measures the concentration of transfers specifically across the pre-transfer income distribution, universalism measures the homogeneity of transfers generally across the population. Therefore, we propose universalism is distinct from low-income targeting. Figure 3 reveals low-income targeting and universalism are not negatively associated as would be expected if they were simply opposites. In fact, low-income targeting and universalism are moderately positively associated in rich democracies and strongly positively associated in the broader sample. In rich democracies, there is little association if we omit Japan (r=.10). Therefore, among countries with at least a modicum of low-income targeting or universalism, there is no relationship between universalism and low-income targeting.

Still, how can countries be both low-income targeted and universal? Countries like Denmark simultaneously concentrate transfers on low-income households, and cover all risk groups, and all categories of residents. This combination is one of the sources of Denmark's high extensity. What is really driving the association though are the low universalism developing countries with high-income targeting. For example, Peru is very non-universal and targets transfers to high income households. By contrast, Uruguay is more universal and also slightly targets transfers to low-income households. Therefore, highly extensive and universal welfare states are much more low-income targeted than the low extensity and low universalism developing countries. As countries move away from high-income targeting by expanding extensity and universalism, this normally requires more low-income targeting.

Supplementary Analyses

The appendices include a series of supplementary analyses. First, KP are concerned with income inequality as well as relative poverty. [21] Therefore, Appendix III displays the correlation between the dimensions of transfers and the gini index of income inequality. The results are similar to the results for relative poverty (see Figure 1). This is not surprising as the gini correlates strongly with relative poverty (r=.9 in both samples). Extensity, low-income targeting and universalism are all strongly negatively correlated with the gini in both samples.

Second, a central mechanism in the paradox of redistribution is the "political coalitions that different welfare state institutions generate" (KP: 663). According to KP, universalism increases support for redistribution and low-income targeting undermines support because these dimensions feed back into political coalitions. One test of these claims is if universalism enhances and low-income targeting undermines support for Leftist parties. Using the ISSP, Appendix IV shows universalism as well as extensity are not significantly associated with Left party affiliation. Low-income targeting is significant in both samples. Surprisingly, however, the effects of low-income targeting are positive. Contrary to expectations, as low-income targeting increases, the average respondent is more likely to affiliate with Leftist parties.

[21] KP also display correlations with redistribution. We do not examine redistribution for several reasons. First, and most importantly, pre-tax income is not even available for many LIS datasets, so despite claims to measure redistribution as the difference between pre-fisc and post-fisc income, scholars often actually measure pre-transfer posttax "net" income. Second, individual-level (as opposed to country-level) redistribution is problematic as individual-level pre-fisc income is endogenous to transfers (Bergh 2005; Jesuit and Mahler 2010). For example, households receiving high transfers do not need as much labor income. Third, redistribution is difficult to measure at the individual-level as it means totally different things for those at the top and bottom of the income distribution. Fourth, measuring redistribution typically requires omitting retirees, while we are interested in the entire population. Finally, there have been many persuasive recent critiques of redistribution measures (Bergh 2005; Brady 2009; Kelly 2005; Marx et al. 2012). For instance, redistribution measures conflate between- and within-person redistribution (i.e. between working years and retirement).

Another way to evaluate this mechanism is to test if the dimensions predict cross-class differences in redistribution preferences. One implication of KP is that the effect of income for redistribution preferences should vary depending on dimensions of transfers. In extensive and universal welfare states, the income slope should be flatter while in low-income targeted welfare states, the income slope should be steeper. Appendix V estimates multi-level logit models with random coefficients for individual-level income and with income interacted with dimensions of transfers.[22] We urge caution with these results as we have a limited number of level-2 units for random coefficients models (Stegmueller 2013). Consistent with KP, in rich democracies and the broader sample, low-income targeting steepens the income slope. When transfers are low-income targeted, higher income individuals are even more significantly opposed to redistribution. However, contrary to KP, extensity and universalism (in the broader sample only) also significantly steepen the income slope. With high extensity and universalism, the effect of income appears to be even more negative for redistribution preferences.

In sum, the dimensions of transfers may influence political coalitions via Left party affiliation and cross-level interactions with income. However, some of the patterns are contrary to KP's expectations.

DISCUSSION

This study revisits KP's highly influential classic: "The Paradox of Redistribution." KP contend universalism reduces poverty while targeting worsens poverty because universalism encourages and targeting undermines coalitions for larger welfare states. Further, targeting and universalism affect poverty because of their effects on extensity, which ultimately reduces

[22] In analyses available upon request, we also tested for cross-level interactions of income and dimensions in models of Left party affiliation. However, none of the interactions was significant.

poverty. We use LIS data to measure the extensity, low-income targeting, and universalism of welfare transfers actually received. We update and expand on KP's sample of rich democracies while also analyzing a broader sample of developed and developing countries. This allows us to test the generalizability of KP's arguments for a much larger share of the world's population and countries approximately two decades after the data KP analyzed. Using multi-level models, we examine how these three dimensions of transfers are related to individual-level poverty and redistribution preferences. By incorporating redistribution preferences, we analyze the identities and interests theorized but unobserved by KP

In some ways, our results confirm and extend KP. Poverty is significantly negatively associated with extensity and universalism. Redistribution preferences are significantly negatively associated with low-income targeting. Low-income targeting also appears to increase income differences in redistribution preferences. Further, universalism and extensity are very strongly correlated. These results all confirm KP. It illustrates the significance of KP that these conclusions are robust two decades later and with much broader samples of developed and developing countries.

There are also key differences between our results and KP. As described in footnotes 15 and 19, these differences partly result from KP's sample being much more selective. These differences likely also result from using data from the 2000s instead of the mid-1980s. Still, low-income targeting does not increase poverty. Indeed, in the multi-level models for rich democracies and in the macro-level correlations (especially in the broader sample), low-income targeting is surprisingly negatively associated with poverty. Moreover, universalism is not associated with redistribution preferences. We also find extensity is not significantly related to redistribution preferences, and that extensity and universalism increase income differences in

redistribution preferences. Finally, low-income targeting is surprisingly positively associated with extensity and universalism in the broader sample. This is largely because several developing countries have high-income targeted transfers and very little extensity or universalism.

Altogether, the results lead us to revise the paradox of redistribution into two new paradoxes. First, there is a clear mismatch between what reduces poverty and what matters to redistribution preferences. Extensity and universalism reduce poverty but are unrelated to redistribution preferences. Indeed, extensity has the largest effect for poverty of the dimensions, and has a large effect relative to commonly studied individual-level predictors of poverty. Yet, extensity is not remotely associated with redistribution preferences. Low-income targeting undermines redistribution preferences, but is not robustly related to poverty. Therefore, what is most important to poverty is not popular, and what is unpopular either does not matter to poverty or actually reduces it.

Second, in the broader sample, the dimensions that reduce poverty (extensity and universalism) are positively correlated with the one dimension (low-income targeting) that undermines support for redistribution. At least in developing countries, this could result in a counterproductive feedback dynamic. As developing countries increase their extensity and universalism, they are likely to increase low-income targeting. This increased low-income targeting is likely to then weaken redistribution preferences, and weakened redistribution preferences should undermine the political coalitions supporting extensity and universalism. Thus, the dimensions that best reduce poverty in developing countries correlate with the one dimension that undermines support for redistribution. Greater effectiveness at reducing poverty may come at the expense of reduced popularity of redistribution.

Beyond these paradoxes, this article makes other contributions to inequality and social policy literatures. Primarily, our paper contributes to the growing literature on the contextual and institutional sources of poverty and inequality (Kelly 2005; Kristal 2010; Moller et al. 2003; Moller 2008; Pribble et al. 2009; Scruggs 2008). The extensity and universalism of transfers have large effects on poverty rivaling the effects of well-established individual-level predictors of poverty. This further demonstrates that the stratification of individual life chances should be contextualized within national-level policies. Secondly, the finding that low-income targeting influences redistribution preferences confirms institutionalist arguments about feedback effects and path dependency. Even though extensity and universalism are insignificant, the findings for low-income targeting support the literature on how welfare states influence welfare attitudes (Brooks and Manza 2007; Fernandez and Jaime-Castillo 2013; Larsen 2008; Sachweh and Olafsdottir 2010). Because low-income targeted policies shape preferences, this likely influences the coalitions driving the subsequent politics of social policy (Pierson 1996; Nelson 2007; Skocpol 1992), though as the supplementary analyses show, the exact nature of these relationships is often unanticipated and needs further scrutiny.

Third, we inform the emerging literature on social policy in developing countries. Mares and Carnes (2009) point out the literature still lacks basic descriptive information about social policies in developing countries and has not fully mapped the differences between developed and developing countries (also Rudra 2007). Our study is one of the first to utilize all available LIS countries. Buttressing the emerging literature, developing countries tend to be less extensive and universal, and more high-income targeted than rich democracies. At the same time, the results show more commonalities than differences across developed and developing countries. While the strength of associations differ, the dimensions are mostly signed in the same direction across

rich democracies and the broader sample. Therefore, the dimensions of transfers appear to have similar consequences in developed and developing countries.

Finally, our study suggests welfare effort remains an essential measure of social policy. KP and much of the welfare state literature has critiqued and even abandoned welfare effort, even though KP found extensity (redistributive budget size) plays the key role in reducing poverty. By contrast, recent LIS studies of rich democracies show welfare effort predicts poverty and inequality quite well (Brady 2009; Moller et al. 2003), and we also find extensity is the paramount dimension for poverty. Perhaps the simplest dimension of transfers, extensity mechanically rises as the population ages, unemployment increases, or a greater share has recognized needs. Thus, extensity is vulnerable to the same critique of conflating generosity and need. Still, the evidence that extensity and effort reduce poverty suggests scholars may have abandoned welfare effort prematurely. We propose this is because extensity and effort track how much the average household's income is socialized and comes from more equally distributed transfers rather than less equally distributed market income. Further, the definition of recognized need reflects political choices about which groups are protected and which risks are socialized.

Beyond the points above, we recommend three directions for future research. First, though arguably the multi-level models examine how exogenous national-level factors affect individual-level outcomes, the present study is cross-sectional. Therefore, we cannot offer causal evidence for the dimensions of transfers. Future research could use multiple waves of the LIS and ISSP to examine variation over-time within countries to control for stable unobserved differences between countries. Second, it would be valuable to expand the outcomes studied. One could link the country-level measures of dimensions to other datasets to examine political outcomes like voting. Beyond poverty, scholars could also investigate the attainment of income

across the distribution. An advantage of our approach is that scholars can use the same datasets to analyze income/poverty as they do to construct country-level measures of transfers. While high quality measures of welfare states are often scarce for developing countries (Huber and Stephens 2012), one can calculate our measures for any LIS country-year. To that end, Appendix VI provides the code for replicating the dimensions of transfers. Third, we assess transfers without taxation, even though social insurance and other transfers may be taxable. A few recommend indexing transfers to taxation based on tax rates for other income sources (e.g. Ferrarini and Nelson 2003), though the literature has not yet converged on a strategy. Still, future research could evaluate these dimensions taking taxation into account.

This study revisits KP's paradox of redistribution by using improved methods and data and refining and developing measures of dimensions of transfers. We conclude that extensity and universalism are most important to poverty, and low-income targeting is most important to redistribution preferences. By contrast, extensity and universalism are not related to redistribution preferences and low-income targeting is not robustly associated with poverty. Because low-income targeting, universalism and extensity are positively associated, we propose a revision for the paradox of redistribution into two new paradoxes. First, there is a mismatch between what reduces poverty and what leads to support for redistribution. Second, in developing countries, the dimensions that best reduce poverty correlate with the one dimension that undermines support for redistribution. Like KP, these new paradoxes present a host of interesting questions for literatures on politics, social policy, poverty and inequality.

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Table 1. Example Countries and Dimensions of Welfare Transfers.

	Country	***Low-Income HHs***	***Middle-Income HHs***	***High-Income HHs***
High Extensity	Sweden	120.2%	50.8%	17.8%
Low Extensity	Colombia	.7%	7.5%	19.0%
Low-Income Targeting	Australia	4,612.75 A$	5,421.90 A$	2,579.44 A$
High-Income Targeting	China	48.56 ¥	210.37 ¥	3229.02 ¥

	Country	***Rural***	***Urban***
High Universalism	Czech Republic	58,417.76 kr	59,043.36 kr
Low Universalism	Mexico	2,993.81 p	4,384.41 p

Note: See methods section of text for details. Low-income households are defined as below 40% of median income. Middle-income households are defined as between 95% and 105% of median income. High-income households are defined as more than two times greater than median income. Extensity cells are equivalized transfers as percent of equivalized income. Targeting cells are raw currency. Universalism cells are equivalized transfers.

Table 2. Mutli-Level Logit Models of Poverty: Standardized Odds Ratios for Welfare Dimensions and Odds Ratios for Individual-Level Variables.

	Rich Democracies			***Broader Sample***		
	(1)	(2)	(3)	(4)	(5)	(6)
Extensity	.494***			.673***		
	(-6.04)			(-4.12)		
Low-Income Targeting		.763**			.818	
		(-2.82)			(-1.75)	
Universalism			.690**			.749**
			(-2.96)			(-3.32)
Age	.903***	.903***	.903***	.951***	.951***	.951***
	(-80.65)	(-80.63)	(-80.66)	(-62.57)	(-62.59)	(-62.58)
Age^2	1.001***	1.001***	1.001***	1.001***	1.001***	1.001***
	(55.17)	(55.15)	(55.17)	(40.48)	(40.49)	(40.48)
Single Mother	1.646***	1.646***	1.646***	1.618***	1.618***	1.618***
	(45.28)	(45.28)	(45.27)	(60.33)	(60.32)	(60.32)
Female Lead No Children	1.677***	1.677***	1.677***	1.490***	1.490***	1.490***
	(44.81)	(44.81)	(44.81)	(45.18)	(45.17)	(45.17)
Male Lead No Children	1.421***	1.421***	1.421***	1.445***	1.445***	1.445***
	(27.29)	(27.28)	(27.28)	(36.00)	(35.99)	(36.00)
# Children	1.244***	1.244***	1.244***	1.246***	1.246***	1.246***
	(73.34)	(73.34)	(73.34)	(148.69)	(148.69)	(148.69)
# Over 64	.555***	.555***	.555***	.770***	.770***	.770***
	(-60.12)	(-60.11)	(-60.12)	(-48.45)	(-48.45)	(-48.45)

Table 2 Continued…

Low Education	2.092***	2.092***	2.092***	2.621***	2.621***	2.621***
	(87.50)	(87.51)	(87.52)	(161.05)	(161.07)	(161.06)
High Education	.419***	.419***	.419***	.381***	.381***	.381***
	(-88.65)	(-88.63)	(-88.64)	(-113.30)	(-113.30)	(-113.30)
No Workers in HH	5.502***	5.502***	5.502***	3.616***	3.616***	3.616***
	(150.25)	(150.24)	(150.24)	(170.81)	(170.80)	(170.81)
Multiple Workers in HH	.184***	.184***	.184***	.281***	.281***	.281***
	(-189.40)	(-189.41)	(-189.41)	(-237.09)	(-237.09)	(-237.09)
N	1,064,628	1,064,628	1,064,628	1,973,625	1,973,625	1,973,625
Countries	21	21	21	38	38	38

*** p < 0.001, ** p < 0.01, * p < 0.05.

Notes: The numbers in parentheses are z-scores. Constants not shown. Odds between .999 and 1.0 were rounded to .999. The references are male, married, secondary education, and one worker in HH.

Table 3. Multi-Level Logit Models of Redistribution Preferences: Standardized Odds Ratios for Welfare Dimensions and Odds Ratios for Individual-Level Variables.

	Rich Democracies			***Broader Sample***		
	(1)	(2)	(3)	(4)	(5)	(6)
Extensity	937			.815		
	(-.59)			(-1.53)		
Low-Income Targeting		.849			.677**	
		(-1.63)			(-3.34)	
Universalism			1.010			.897
			(.10)			(-.85)
Age	1.021**	1.021**	1.021**	1.015*	1.015*	1.015*
	(2.858)	(2.861)	(2.856)	(2.387)	(2.394)	(2.387)
Age^2	1.000	1.000	1.000	1.000	1.000	1.000
	(-1.906)	(-1.912)	(-1.904)	(-1.482)	(-1.490)	(-1.481)
Female	1.249**	1.249**	1.249**	1.196**	1.196**	1.196**
	(5.690)	(5.682)	(5.690)	(5.537)	(5.531)	(5.541)
Never Married	1.165**	1.165**	1.165**	1.183**	1.182**	1.183**
	(2.669)	(2.670)	(2.674)	(3.507)	(3.486)	(3.504)
Divorced	1.132	1.132	1.133	1.117	1.116	1.117
	(1.804)	(1.807)	(1.811)	(1.950)	(1.927)	(1.952)
Widowed	1.078	1.078	1.078	1.009	1.007	1.010
	(0.784)	(0.783)	(0.790)	(0.127)	(0.0983)	(0.132)
HH Size	1.122**	1.121**	1.122**	1.082**	1.081**	1.082**
	(5.628)	(5.586)	(5.651)	(5.370)	(5.314)	(5.377)
Children in HH	0.855**	0.856**	0.854**	0.899*	0.899*	0.899*
	(-2.802)	(-2.781)	(-2.813)	(-2.394)	(-2.389)	(-2.398)
Rural	1.076	1.077	1.075	1.102*	1.105*	1.101*
	(1.331)	(1.345)	(1.317)	(2.167)	(2.227)	(2.158)
Suburb	1.162**	1.162**	1.162**	1.121**	1.123**	1.121**
	(3.237)	(3.236)	(3.230)	(2.993)	(3.050)	(2.997)

Table 3 Continued…

Low Education	1.621**	1.623**	1.620**	1.503**	1.502**	1.502**
	(10.00)	(10.03)	(9.989)	(10.30)	(10.29)	(10.28)
High Education	0.902*	0.902*	0.902*	0.857**	0.858**	0.858**
	(-2.111)	(-2.120)	(-2.112)	(-3.720)	(-3.706)	(-3.714)
Part-Time	1.096	1.097	1.096	1.107*	1.108*	1.107
	(1.514)	(1.528)	(1.517)	(1.961)	(1.972)	(1.956)
Unemployed	1.478**	1.478**	1.477**	1.166*	1.161	1.165*
	(3.181)	(3.181)	(3.180)	(1.988)	(1.935)	(1.976)
Not in Labor Force	1.008	1.008	1.008	1.059	1.059	1.059
	(0.151)	(0.156)	(0.148)	(1.338)	(1.344)	(1.337)
Self-Employment	0.713**	0.713**	0.713**	0.722**	0.722**	0.723**
	(-5.754)	(-5.753)	(-5.756)	(-6.860)	(-6.862)	(-6.852)
Public Employment	1.329**	1.331**	1.328**	1.338**	1.339**	1.337**
	(6.282)	(6.309)	(6.263)	(7.544)	(7.566)	(7.529)
Relative Income	0.696**	0.696**	0.696**	0.768**	0.768**	0.768**
	(-16.47)	(-16.45)	(-16.48)	(-15.84)	(-15.85)	(-15.86)
Low Religious Attendance	0.884**	0.883**	0.884**	0.905**	0.904**	0.904**
	(-2.809)	(-2.822)	(-2.808)	(-2.716)	(-2.732)	(-2.724)
High Religious Attendance	0.925	0.926	0.926	0.992	0.990	0.991
	(-1.274)	(-1.274)	(-1.260)	(-0.155)	(-0.194)	(-0.173)
N	15,887	15,887	15,887	26,752	26,752	26,752
Countries	16	16	16	25	25	25

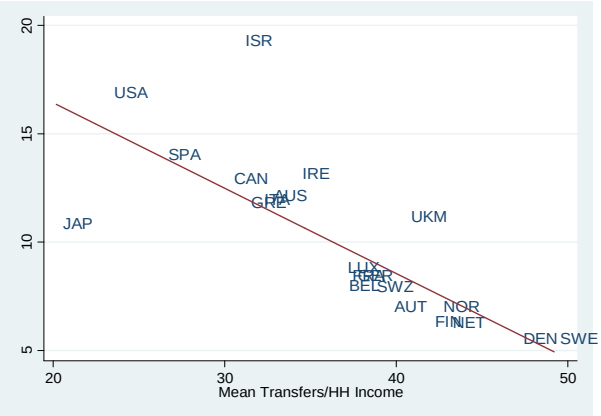
*** p < 0.001, ** p < 0.01, * p < 0.05.

Notes: The numbers in parentheses are z-scores. Constants not shown. Odds between .999 and 1.0 were rounded to .999. The references are male, married, no children, urban, secondary education, full-time, private sector, and no religious attendance.

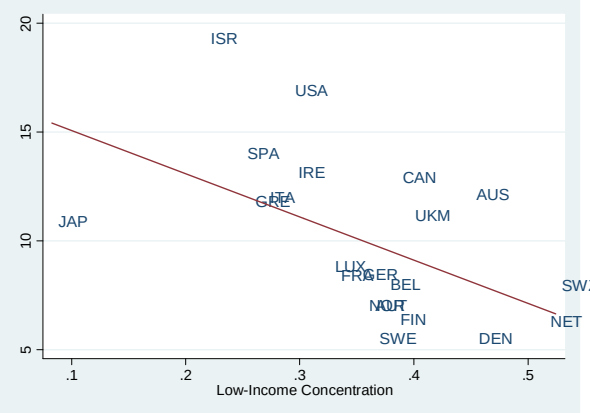
Figure 1. Macro-Level Bivariate Associations Between Poverty Rate and Dimensions of Welfare Transfers.

a) RICH DEMOCRACIES (N=21)

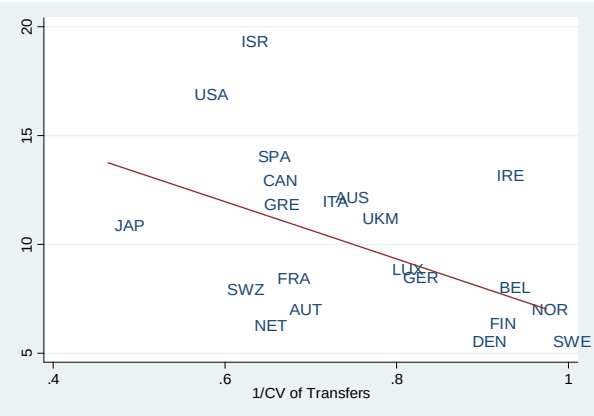
Extensity (r=-.77)



Low-Income Targeting (r=-.54)

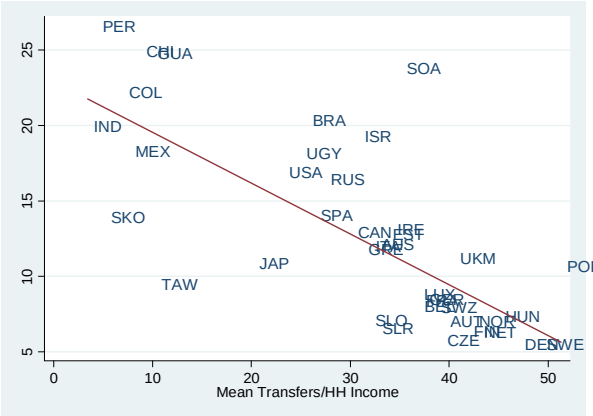


Universalism (r=-.51)

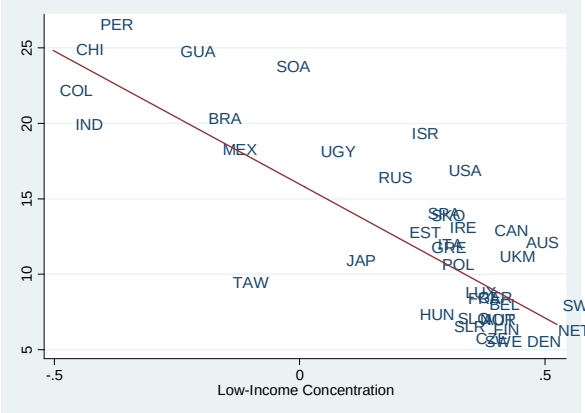


b) BROADER SAMPLE (N=39)

Extensity (r=-.73)



Low-Income Targeting (r=-.82)



Universalism (r=-.75)

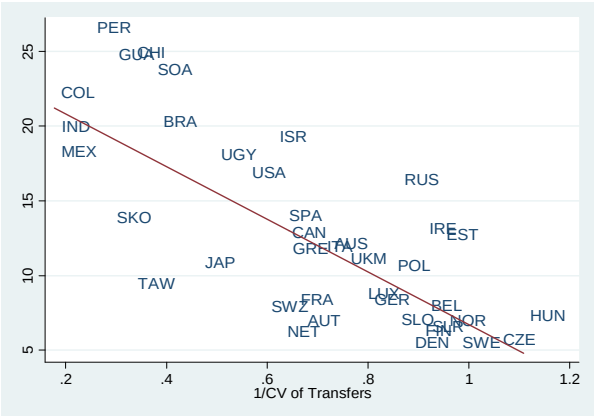
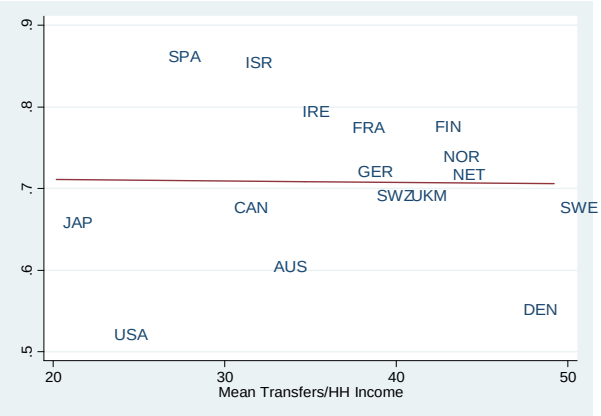


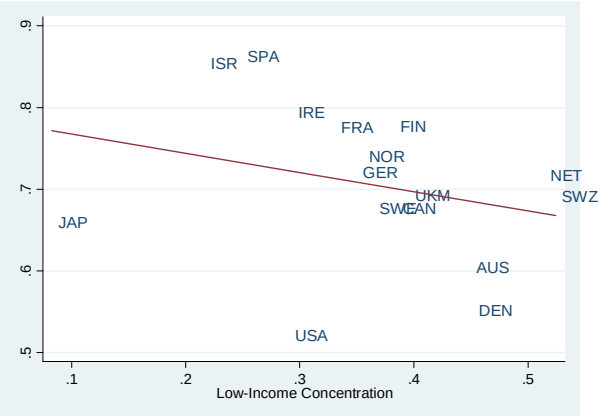
Figure 2. Macro-Level Bivariate Associations Between Proportion Supporting Redistribution and Dimensions of Welfare Transfers.

a) RICH DEMOCRACIES (N=16)

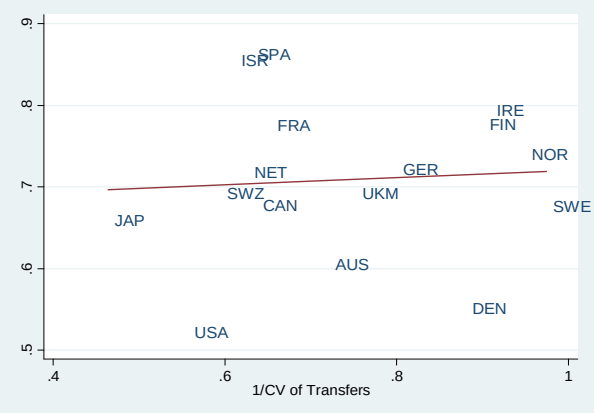
Extensity (r=-.02)



Low-Income Targeting (r=-.28)

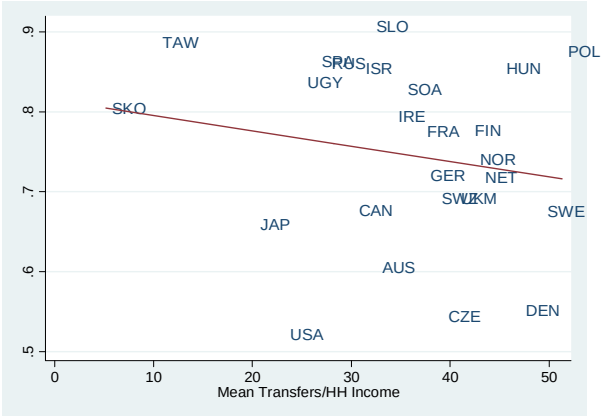


Universalism (r=.07)

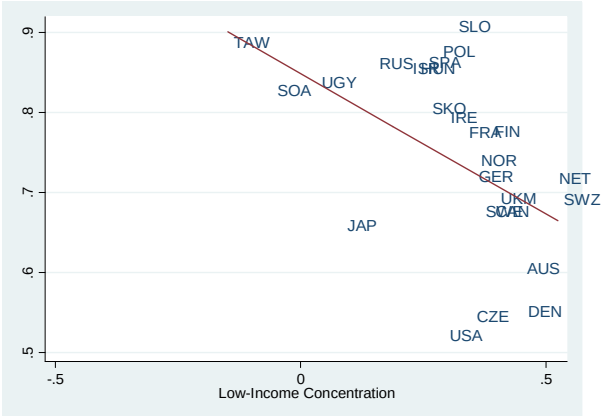


b) BROADER SAMPLE (N=25)

Extensity (r=-.19)



Low-Income Targeting (r=-.51)



Universalism (r=-.15)

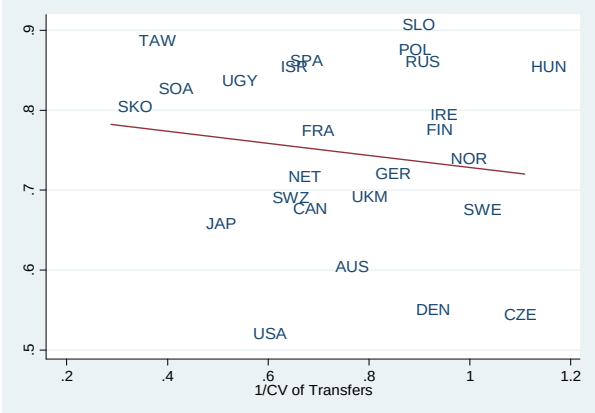
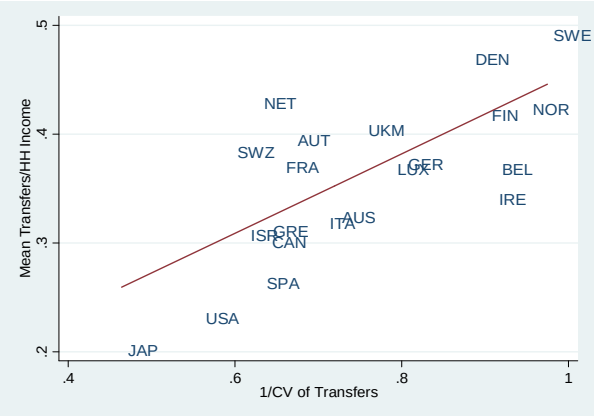


Figure 3. Macro-Level Bivariate Associations Between Dimensions of Welfare Transfers.

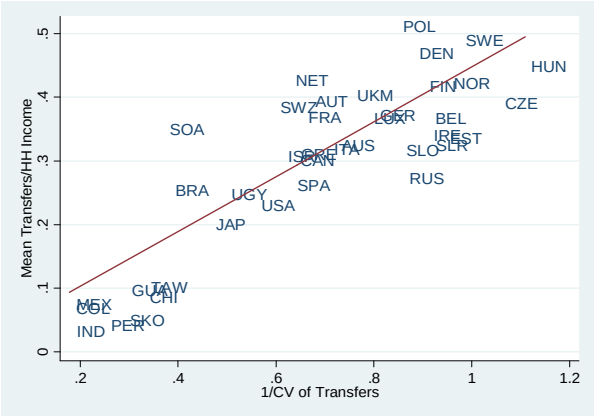
a) RICH DEMOCRACIES (N=21)

Extensity & Universalism (r=.71)

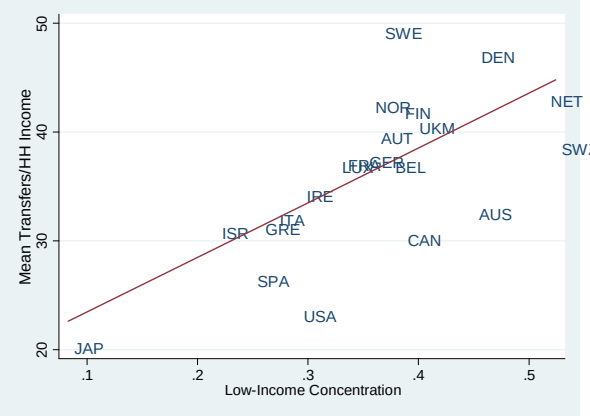


b) BROADER SAMPLE (N=39)

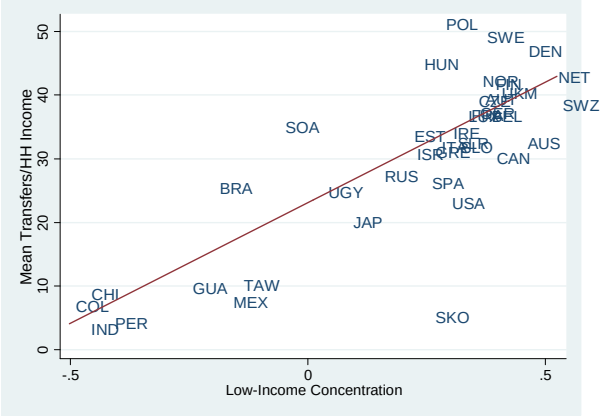
Extensity & Universalism (r=.85)



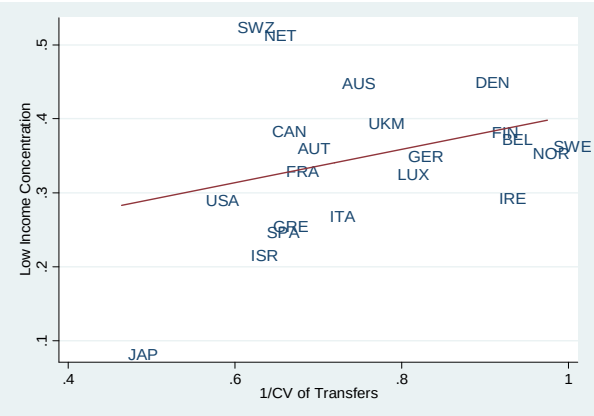
Extensity & Low-Income Targeting (r=.69)



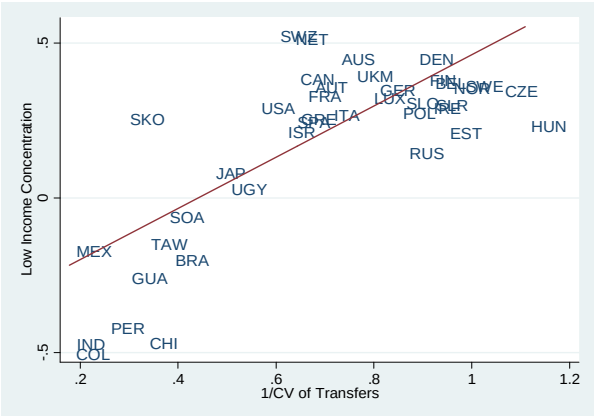
Extensity & Low-Income Targeting (r=.81)



Low-Income Targeting & Universalism (r=.32)



Low-Income Targeting & Universalism (r=.76)



Appendix I. Descriptive Statistics: Means and Standard Deviations in Parentheses.

	LIS-Rich Democracies	***LIS-Broader Sample***	***ISSP-Rich Democracies***	***ISSP-Broader Sample***
Poverty	.119	.148	--	--
	(.324)	(.355)		
Redistribution	--	--	.691	.755
Preferences			.462	(.430)
Extensity	32.841	24.596	35.314	31.949
	(9.696)	(15.649)	(8.120)	(11.941)
Low-Income	.343	.095	.354	.252
Targeting	(.078)	(.339)	(.106)	(.181)
Universalism	.690	.541	.730	.679
	(.147)	(.260)	(.147)	(.225)
Age	45.435	44.963	48.458	47.054
	(14.595)	(14.809)	(16.423)	(16.858)
Age^2	2277.331	2240.971	2617.908	2498.294
	(1474.291)	(1485.272)	(1666.871)	(1682.351)
Single Mother	.078	.066	--	--
	(.268)	(.248)		
Female Lead No	.096	.074	--	--
Children	(.295)	(.261)		
Male Lead No	.083	.057)	--	--
Children	(.275)	(.233)		
# Children	1.156	1.513	--	--
	(1.312)	(1.620)		
# Over 64	.241	.268	--	--
	(.574)	(.583)		
Low Education	.218	.400	.380	.403
	(.413)	(.490)	(.485)	(.491)
High Education	.338	.244	.202	.179
	(.473)	(.430)	(.401)	(.383)
No Workers in HH	.142	.135	--	--
	(.349)	(.342)		
Multiple Workers	.576	.533	--	--
in HH	(.494)	(.499)		
Female	--	--	.514	.532
			(.500)	(.499)
Never Married	--	--	.224	.239
			(.417)	(.427)
Divorced	--	--	.096	.092
			(.295)	(.288)
Widow	--	--	.061	.082
			(.239)	(.275)
HH Size	--	--	2.778	3.028
			(1.451)	(1.660)

Appendix I
Continued…

Children in HH	--	--	.353	.392
			(.478)	(.488)
Rural	--	--	.287	.265
			(.453)	(.441)
Suburb	--	--	.475	.414
			(.499)	(.493)
Part-Time	--	--	.124	.110
			(.331)	(.313)
Unemployed	--	--	.035	.064
			(.183)	(.245)
Not in Labor Force	--	--	.347	.354
			(.476)	(.478)
Self-Employment	--	--	.110	.124
			(.313)	(.329)
Public Employment	--	--	.262	.266
			(.440)	(.442)
Relative Income	--	--	.018	.012
			(1.005)	(1.004)
Low Religious Attendance	--	--	.538	.489
			(.499)	(.500)
High Religious Attendance	--	--	.177	.225
			(.381)	(.417)
N	1,064,628	1,973,625	15,887	26,752

Appendix II. Samples of Countries (All Available in LIS Samples, Except Brazil).

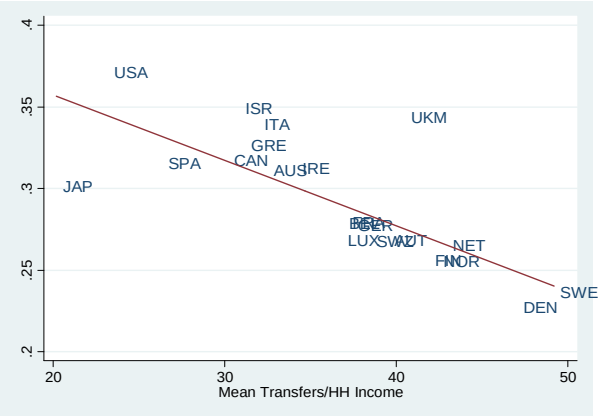
Country	***Abbreviation***	***Year***	***Rich Democracies?***	***ISSP?***
Australia	AUS	2003	Yes	Yes
Austria	AUT	2004	Yes	No
Belgium	BEL	2000	Yes	No
Brazil[a]	BRA	2006	No	No
Canada	CAN	2004	Yes	Yes
China	CHI	2002	No	No
Colombia	COL	2007	No	No
Czech Republic	CZE	2004	No	Yes
Denmark	DEN	2004	Yes	Yes
Estonia	EST	2004	No	No
Finland	FIN	2004	Yes	Yes
France	FRA	2005	Yes	Yes
Germany	GER	2004	Yes	Yes
Greece	GRE	2004	Yes	No
Guatemala	GUA	2006	No	No
Hungary	HUN	2005	No	Yes
India	IND	2004	No	No
Ireland	IRE	2004	Yes	Yes
Israel	ISR	2005	Yes	Yes
Italy	ITA	2004	Yes	No
Japan	JAP	2008	Yes	Yes
Luxembourg	LUX	2004	Yes	No
Mexico	MEX	2004	No	No
Netherlands	NET	2004	Yes	Yes
Norway	NOR	2004	Yes	Yes
Peru	PER	2004	No	No
Poland	POL	2004	No	Yes
Russia	RUS	2000	No	Yes
South Korea	SKO	2006	No	Yes
Slovenia	SLO	2004	No	Yes
Slovakia	SLR	2007	No	No
South Africa	SOA	2008	No	Yes
Spain	SPA	2004	Yes	Yes
Sweden	SWE	2005	Yes	Yes
Switzerland	SWZ	2004	Yes	Yes
Taiwan	TAW	2005	No	Yes
Uruguay	UGY	2004	No	Yes
United Kingdom	UKM	2004	Yes	Yes
United States	USA	2004	Yes	Yes

a. Brazil is only included in the Figures, but is not included in analyses because of missing data on key individual characteristics (e.g. marital status).

Appendix III. Macro-Level Bivariate Associations Between Gini Coefficient and Dimensions of Welfare Transfers.

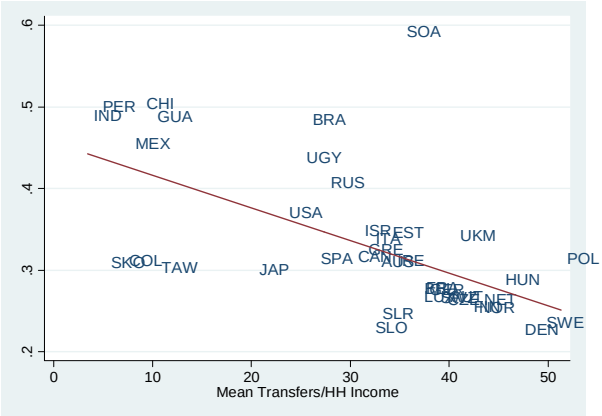
a) RICH DEMOCRACIES (N=21)

Extensity (r=-.75)

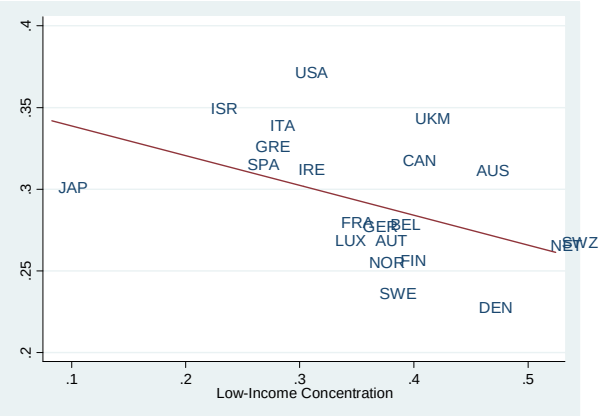


b) BROADER SAMPLE (N=39)

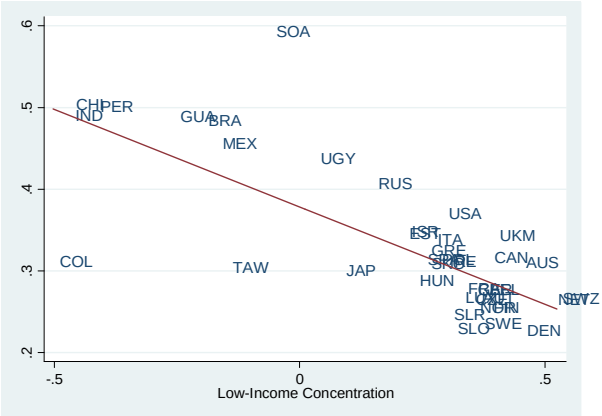
Extensity (r=-.59)



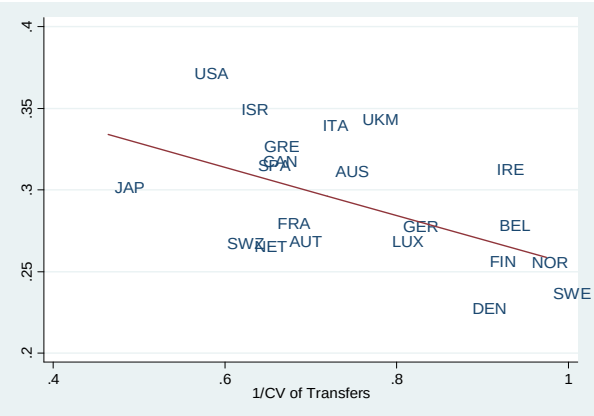
Low-Income Targeting (r=-.47)



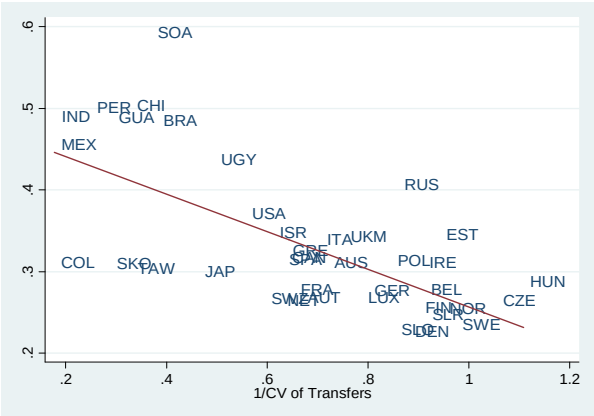
Low-Income Targeting (r=-.75)



Universalism (r=-.54)



Universalism (r=-.67)



Appendix IV. Multi-Level Logit Models of Left Party Affiliation: Standardized Odds Ratios (Individual-Level Variables Not Shown).

		Rich Democracies		***Broader Sample***
	(1)	(2)	(3)	(4)
Extensity	1.799			.931
	(1.80)			(-.06)
Low-Income Targeting		2.220**		3.435***
		(2.83)		(3.66)
Universalism			1.522	1.224
			(1.26)	(.50)
N	16,669	16,669	16,669	27,948
Countries	16	16	16	25

*** p < 0.001, ** p < 0.01, * p < 0.05.

Notes: The numbers in parentheses are z-scores. The reference for the dependent variable is all other parties and no party. All individual-level variables from Table 2 are included but not shown. The results are consistent if model 4 is decomposed into separate models for each dimension.

Appendix V. Multi-Level Random Coefficient Logit Models of Redistribution Preferences Interacting Income and Dimensions of Welfare Transfers: Odds Ratios (Other Individual-Level Variables Not Shown).

	(1) *Rich Democracies*	*(2)* *Broader Sample*	*(3)* *Rich Democracies*	*(4)* *Broader Sample*	*(5)* *Rich Democracies*	*(6)* *Broader Sample*
Income	.935	1.058	.949	.948	.760	.951
	(-.46)	(.63)	(-.55)	(-1.11)	(-1.53)	(-.44)
Income*Dimension	.992*	.990**	.427**	.439**	.899	.721*
	(-2.01)	(-3.98)	(-3.35)	(-5.54)	(-.44)	(-2.13)
Extensity	.993	.985				
	(-.52)	(-1.44)				
Low-Income Targeting			.238	.130**		
			(-1.55)	(-3.20)		
Universalism					1.099	.637
					(.13)	(-.81)
N	15,887	26,752	15,887	26,752	15,887	26,752
Countries	16	25	16	25	16	25

** $p < 0.01$, * $p < 0.05$.

Notes: The numbers in parentheses are z-scores. All individual-level variables from Table 2 are included but not shown.

Appendix VI. Code for Generating Dimensions of Welfare Transfers in Luxembourg Income Study.

```
program define welfdim
drop if dhi==.

drop if hwgt==.
replace hwgt=0.01 if hwgt==0

gen pwt=hwgt*nhhmem

gen eqinc=dhi/(sqrt(nhhmem))
qui sum eqinc
gen botlin=0.01*_result(3)
replace eqinc=botlin if eqinc<botlin
quietly sum eqinc, de
gen toplin=10*_result(10)

gen transfer=hit-hitp
replace transfer =hits+hitsu+hitsa if transfer==.
replace transfer= transfer/(sqrt(nhhmem))
replace transfer=0 if transfer<0

gen exten=100*(transfer/eqinc)

gen pretrinc=dhi-transfer
replace pretrinc= pretrinc/(sqrt(nhhmem))
replace pretrinc=0 if pretrinc<0

*Extensity is mean of exten*
tabstat exten [w=pwt], stats (mean)

*Low-Income Targeting is -1 multiplied times concentration coefficient generated here*
sgini transfer [aweight=pwt], sortvar(pretrinc)

*Universalism is 1/CV of transfer*
tabstat transfer [w=pwt], stats (cv)

end

*Example of country*
use $au03h, clear
keep dhi hwgt nhhmem hit hitp hits hitsu hitsa
welfdim
```