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 much more than targeting 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 the relationships between these country-level dimensions and 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, and is even occasionally 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.

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

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 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. This is because 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, 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 salient 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 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.

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[1] According to Google Scholar, KP has about 1,200 citations. This appears to be the most cited article on the welfare state 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).

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

KP 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 because it has: "focused almost exclusively on how to distribute the money available

3

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, and indices of targeting and redistribution derived from the LIS.

They show redistributive budget size correlates positively with redistribution and negatively with

poverty and inequality. Further, low-income targeting is negatively associated with redistributive

budget size and redistribution, and positively associated with 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."

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 matters 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)

(Béland 2010; 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 recently available LIS, KP encouraged a lot of subsequent

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

4

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).

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. Most importantly, 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.

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[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.

Though there is substantial variation among rich democracies, there is even greater variation when incorporating developing countries. Even though developing countries are often not democracies, they have social policies, their public has preferences, and public support matters even in non-democracies (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, development economics, and 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 the "take up" of welfare transfers (Currie 2004; Van Oorschot 1991). Such research advantageously identifies 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 often constrains the intended generosity of welfare programs (Piven and Cloward 1993; Soss et al. 2011). Thus, actually received transfers provide an essential complement to program criteria. Partly, this is because measures based on program criteria are forced to concentrate selectively on a few easily measured programs such as unemployment or old age insurance. Unfortunately, spending on different programs is not highly 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 comprehensively measures a wide variety of cash and near cash transfers, one can capture much more of the 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 build on 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 define each dimension conceptually and operationally and hypothesize how each relates to poverty and redistribution preferences. To make the dimensions more concrete, Table 1 displays countries exemplifying each dimension.

[TABLE 1 ABOUT HERE]

Extensity

We rename and slightly revise KP's "redistributive budget size" under the label "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, Swedish 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. Across all Swedish households, transfers average 49 percent

7

of household income.[4] By contrast, transfers only average 7 percent of household income in

Colombia. 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 a 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 of 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 and inequality.[5] Indeed, 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, poverty and

inequality mechanically decline (Huber and Stephens 2012). Also, as B grows relative to A, public

transfers crowd out private pensions and transfers, further reducing poverty and inequality (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

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[4] The 49 percent includes the entire population while Table 1 only displays select points in the distribution. Sweden's low-income households' transfers exceed income because we measure transfers before taxes (e.g. on social insurance pensions). Below, we discuss the need for research indexing transfers by taxation (Ferrarini and Nelson 2003).
[5] Despite Esping-Andersen's criticisms of effort, there are many similarities between extensity and his concept 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).

states make political choices about which "needs" receive public support. Welfare states politically choose to automatically spend money on the unemployed or elderly, and not to spend money automatically on other risk groups. Thus, by raising effort/extensity in response to particular needs, welfare states make political choices to recognize those needs and socialize those risks. If countries choose to socialize more risks, greater extensity and lower poverty should result. Conversely, when welfare states fail to recalibrate or update programs to face new demographic and economic risks, lower extensity and greater poverty should result (Béland 2010; Hacker 2004). 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 targeted specifically for single mothers, best explains cross-national variation in single mother poverty.[6]

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.

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

We expect extensity to be positively associated with redistribution preferences. The literature on path dependency shows large welfare states set expectations that reinforce the popularity of social policy (Huber and Stephens 2001; KP 1998; Nelson 2007; Pierson 1996; Sachweh and Olafsdottir 2010; Svallfors 2007). This is partly because high extensity means a larger pool of beneficiaries and stakeholders. This pool has 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 a concentration index that assesses the distribution of transfers on the pre-transfer income distribution (Kakwani and Subbarao 2007; KP 1998). Targeting is typically justified because it is efficient. With tight budgets, targeting focuses scarce resources on the neediest (Blank 1997; Greenstein 1991), and avoids "leakage" – when the affluent or middle class are the primary beneficiaries (Tullock 1997). Some claim 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.

The literature, including KP, often 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. Indeed, Table 1 shows that in 2002, Guatemalan high-income households received about 3.2 times more transfers than middle-income households and about 6.7 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, while advocates highlight targeting's efficiency, there are several unanticipated ways it ultimately devotes less resources to actual assistance. Targeting requires monitoring and screening, which 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; Soss et al. 2011). Contrary to the targeting efficiency literature, others argue targeting counterproductively discourages work and poverty-reducing behavior by forcing unreasonable choices between employment and welfare (Edin and Lein 1997).

As explained by KP, targeting is unpopular and should be negatively associated with redistribution preferences (also 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 on efficiency grounds often ignore its unpopularity and the resulting feedbacks into politics (Blank 1997; Saez 2006; but see Sen 1995). As a result, even within the targeting efficiency literature, there has been little rebuttal to claims that targeting is unpopular and weakens redistribution preferences.

11

In a broader sample of countries, the effects of low-income targeting might differ from a 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 really define universalism, but refer to "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 through different stages of life.

Accordingly, we define universalism as homogeneity across the population in benefits, coverage and eligibility.[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 (Béland 2010). If the guarantee provides the same 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

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[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.

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 heterogeneous benefits across the pre-transfer income distribution, there can also be heterogeneity by sex, 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, then C2 would be more universal even though C1 has identical low-income targeting. Indeed, as demonstrated below, universalism and low-income targeting do not correlate in a way that indicates they capture the same dimension. Thus, universalism distinctively involves homogeneity of benefits in general, and across any and all categories (not just the pre-transfer income distribution).

Table 1 compares transfers across urban and rural areas to illustrate universalism. In the Czech Republic, both rural and urban households receive transfers of about 59,000 kronor. In Mexico, rural households receive less than 3,000 pesos of transfers and urban households receive 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.

According to KP, universalism should reduce poverty more than targeting (also 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 transfers, increasing equality (Huber and Stephens 2012). Further, universalism should be negatively

13

associated with 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 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 since the 19th century reduced work disincentives because more people 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.

A key reason universalism should be negatively associated with poverty is because it is popular (Esping-Andersen 1990, 1999; Gingrich and Ansell 2012; KP 1998; Nelson 2007; Rothstein and Uslaner 2005; Skocpol 1992). Similar to extensity, universalism should feed back into the politics of social policy. Universalism implies all are equal stakeholders and constituencies of beneficiaries, who have an interest in and normative commitment to maintaining social policies (Pierson 2004). Also, universalism lessens the stigma of being a recipient (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. We analyze the rich democracies separately to be comparable with KP. One reasonable reading is that the paradox of redistribution was only intended to apply to rich democracies. Indeed, Huber and Stephens (2012) show it takes 20 years for a democracy to enable public opinion, preferences and parties to cohere for/against social policy. So, while we propose it is valuable to assess generalizability in a broader sample, it is also appropriate to analyze rich democracies separately. If readers are skeptical about extending KP's paradox to a broader sample, the analyses of rich democracies provide a direct comparison with KP.

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).

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

15

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 *j*th

country is represented by eta (η_{ij}) and is a function of country intercepts (β_{0j}), 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 strong correlations between the dimensions of transfers

(see Figure 3), it is essential to be parsimonious at level 2 (Stegmueller 2013).[8]

Nevertheless, multi-level analyses have two major advantages over the macro-level

approach used by KP (also Kenworthy 2011; Marx et al. 2013). First, multi-level models condition

covariation in the dependent and level-2 variables by the individual-level variables. For example,

poverty is clearly associated with marital status, employment, and education. Failing to adjust for

such level-1 characteristics conflates 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 these level-1 characteristics. By

adjusting for the level-1 variables, the models assess the association between the dependent

variables and dimensions of welfare transfers net of the demographic and labor market

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[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. Thus, random coefficients quickly exhaust level-2 units.

compositions of countries. Therefore, a multi-level analysis should result in more accurate estimates of level-2 effects. Second, multi-level models more efficiently estimate level-2 effects than macro-level models. Multi-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 LIS provides the data on the dimensions of transfers. The LIS is an archive of individual-level datasets 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 (updated as of September 2013). 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 for calculating the dimensions of transfers are household public transfers and disposable 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 KP's fn. 6), we are not

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[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).

able to include services. We measure disposable household income after taxes and transfers 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 follows 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 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 so 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. However, none of the interaction effects were insignificant for either dependent variable.

Individual-Level Measures for Poverty Analyses

The first dependent variable, relative *poverty* (poor=1), defines individuals as poor if they reside in a household with less than 50% of the median equivalized disposable income after taxes

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[10] An alternative is the ratio of the poor's mean transfers over the non-poor's mean transfers (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 potential circularity 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=.67 in 39 countries and r=.19 in 21 rich democracies).

and transfers. This measure follows the vast majority of international poverty research (Brady 2009; Moller et al. 2003; Rainwater and Smeeding 2004), and is the same measure KP used. Relative poverty is most relevant to the paradox of redistribution and more consistent with redistribution preferences. The paradox concerns 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.

As we discuss below, KP were also interested in income inequality. However, as income inequality is typically a country-level variable and there is no clear individual level version of this variable, the multi-level models concentrate on poverty. As discussed below, countries' relative poverty levels are very highly correlated with countries' income inequality (r>.9), and income inequality correlates with the dimensions very similarly to 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 university

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[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 in many countries.

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

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 derived from 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 several reasons. First, this question most directly

assesses the preference for redistribution (Cusack et al. 2008), which is paramount for KP's

"identities and interests." Second, international scholars mainly focus on the responsibility

questions and especially this particular question. Third, the alternative questions about spending

preferences are relative to each country's current spending, which makes them less cross-nationally

comparable and conflates attitudes about government responsibilities with perceptions of the

efficacy and efficiency of government programs and taxation (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, we find redistribution preferences significantly increase the odds of Left party

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[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. Third, the ordinal versions fail the parallel regression test in ordinal logit models.

affiliation. Last, while we prefer to focus on this question, Appendix IV displays similar results with a scale of six related ISSP questions.[14]

Following prior research (e.g. Cusack et al. 2008; Svallfors 2006, 2007), the models adjust for several individual-level variables. *Age* and *age²* 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 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 once 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]

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[14] The six questions ask whether it should be the government's responsibility to: "provide a job for everyone who wants one;" "provide a decent standard of living for the unemployed;" "provide a decent standard of living for the old;" "provide decent housing for those who can't afford it;" "provide healthcare for the sick;" and "reduce income differences between rich and poor." Using the underlying ordinal responses, the standardized item alpha is .77.

[15] For extensity (r=-.92) and universalism (r=-.60), the correlations with poverty are similar if only including KP's 11 countries. However, poverty correlates only -.12 with low-income targeting in KP's sample. Rich democracies omitted by KP have greater poverty (p<.16), less extensity (p<.10), and less low-income targeting (p<.02).

[FIGURE 1 ABOUT HERE]

Figure 1 shows 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 – transfers are almost half of the average household's income – and poverty rates near 5 percent. By contrast, extensity is near 25 percent in the U.S. and below 10 percent in Peru. More than 15 percent of the U.S. and 25 percent of Peru is poor.

Surprisingly, and contrary to KP, low-income targeting is negatively associated with poverty (r=-.51 in rich democracies and -.78 in broader sample). The Netherlands and Switzerland concentrate transfers on low-income households and have lower poverty. Israel and Japan 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. 2013). 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.[16] Models 1-4 include the rich democracies, and models 5-8 include the broader sample. We first show separate models for each dimension of transfers, and then combine all three dimensions in one model. Partly because of

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[16] 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.

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]

In the first and third models, extensity and universalism are significantly negative, which is consistent with KP. In the second model, low-income targeting is negatively signed but not quite significant (z=-1.6). Even though it is not quite significant, the negative sign for low-income targeting is inconsistent with KP.

In the third model, extensity remains significantly negative while low-income targeting and universalism are insignificant. The robust significant negative effect of extensity is consistent with KP. For a standard deviation increase in extensity, the odds of poverty are expected to decline by a factor of about 2.3. This effect is larger than the effects of being in a single mother household and having a lead earner with low education. The effect is also similar in magnitude to having a lead earner with high education, but is smaller than having no or multiple workers in the household. That universalism becomes insignificant with extensity in the model is also consistent with KP.[17]

In the broader sample, extensity and universalism remain significantly negative in models by themselves. Targeting remains negatively signed, but is slightly less significant, still contrary to KP. In the combined 8th model, extensity continues to have a significant negative effect and universalism becomes insignificant. Again, the robust negative effect of extensity and the insignificance of universalism net of extensity are consistent with KP. The effect of extensity remains substantively large – larger than the effect of being in a single mother household though smaller than the effect of having a lead earner with low education.

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[17] Because of the challenges of comparing odds ratios across models, we also estimated linear probability models as a robustness check. The results were consistent in the linear probability models (e.g. universalism is initially significant by itself and becomes insignificant when included with extensity).

In this final model, low-income targeting becomes significantly positive. This could be interpreted as consistent with KP. However, this result likely owes to the strong correlations between the three dimensions in the broader sample (see below). Because low-income targeting is negatively signed in models 2 and 6, and insignificant in model 4, the more likely and appropriate interpretation is that low-income targeting is not robustly associated with poverty.[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 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]

Consistent with KP, low-income targeting is negatively correlated with support for redistribution (r=-.30 in rich democracies), and especially in the broader sample (r=-.50). For example, Australia and Switzerland target to low-income households, and have less support for redistribution. By contrast, Taiwan and South Africa target more towards high-income households, and have higher support.

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[18] We assessed robustness by reestimating the models while dropping individual countries. The results were mostly robust. Replicating models 1, 3, 5 and 7, extensity and universalism remain significantly negative in both samples in all models. Though mostly insignificant and usually even less significant, targeting occasionally became significantly negative in models 2 and 6 (e.g. if dropping Australia or the U.S.).

[19] The correlations are different if we only include KP's 11 countries. The proportion supporting redistribution correlates .67 with extensity, .05 with low-income targeting and .43 with universalism. This is partly because rich democracies (with ISSP data) omitted by KP have higher support for redistribution (p<.16), less extensity (p<.10), and less low-income targeting (p<.02).

Contrary to KP, universalism is not correlated 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 children, and the self-employed are significantly less likely to support redistribution. Again, we show separate models for each dimension of transfers and then combine them into one model.

[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=-1.8). However, low-income targeting becomes statistically significant if we omit either Japan (z=-2.4) or the U.S. (z=-2.3) and near significant if we omit the Netherlands (z=-1.91). Such a negative relationship between low-income targeting and redistribution preferences is consistent with KP.

In the third model, none of the three dimensions is significantly related to redistribution preferences. The lack of a significant negative effect for targeting and the lack of a significant positive effect for universalism are not consistent with KP. That said, caution is appropriate as this model includes only 16 countries and the three dimensions are fairly highly correlated (see below).

25

In the broader sample, extensity and universalism remain insignificant in separate models (models 5 and 7) and in the combined model 8. Low-income targeting is now significantly negatively associated with redistribution preferences by itself (model 6) and in model 8. For a standard deviation increase in low-income targeting, redistribution preferences are expected to decline by factor of about 1.5. This effect is comparable to the effect of having low education and larger than the effects of any other individual-level variable. 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 indeed, these two are strongly positively correlated in both samples (r>.7). Recall both are significantly negatively associated with poverty, but universalism becomes insignificant when included in the same model with extensity. Thus, also consistent with KP, universalism's relationship with poverty may be mediated by extensity.

[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 positively correlated. Contrary to KP, countries targeting transfers to low-income households have more extensity (also Marx et al. 2013). This is partly because countries with high-income targeting have very low extensity. As countries

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[20] The results were mostly robust when dropping one country at a time. Extensity remains insignificant in almost all replications of models 1 and 4, 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.

incorporate the poor into social policies, and transfers become less high-income targeted, the welfare state tends to grow. However, even among rich democracies, which all exhibit at least a moderate level of low-income targeting, there is a positive association between low-income targeting and extensity (r=.45). Therefore, contrary to KP, we find no evidence of a tradeoff between low-income targeting and extensity.

As discussed above, scholars often treat targeting and universalism as opposites. We view high-income targeting, not universalism, as the opposite of low-income targeting. Also, we contend that while targeting measures the concentration of transfers specifically across the pre-transfer income distribution, universalism measures the generalized homogeneity of transfers across the entire population (regardless of income). Therefore, we see universalism as 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 simply not associated in rich democracies (r=.07). Once countries reach a modicum of universalism or low-income targeting, there is no relationship between the two. More surprisingly, Figure 3 shows universalism and low-income targeting are quite positively associated in the broader sample (r=.67).

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 is the low universalism of developing countries with high-income targeting. For example, India and Colombia have very low universalism and target transfers to high income households. By contrast, Uruguay, Brazil, and South Africa are more universal and also somewhat target 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

27

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. Therefore, Appendix III displays the correlation
between the dimensions of transfers and the Gini index of income inequality.[21] The results are quite
similar to the results for relative poverty (see Figure 1). Extensity, low-income targeting and
universalism are all negatively correlated with the Gini in both samples. This is not surprising as the
Gini correlates strongly with relative poverty (r>.9 in both samples).

Second, though they did not supply any empirical evidence on the matter, 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. As noted above, Appendix IV displays similar results with a scale of six related ISSP
questions. One further test of these claims is if universalism enhances and low-income targeting
undermines support for Leftist parties. Appendix IV also shows analyses of the ISSP's Left-Right
party affiliation measure (see Appendix IV for details). The results show that none of the three
dimensions is significantly related to Left-Right party affiliation in either the rich democracies or

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[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, analysts often 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 at the individual-level means totally different things for those at the
top and bottom of the distribution. Fourth, measuring redistribution typically requires omitting retirees, while we are
interested in the entire population. Finally, there have been several critiques of redistribution measures, for among other
things, conflating between- and within-person redistribution (i.e. between working years and retirement) (Bergh 2005;
Brady 2009; Kelly 2005; Marx et al. 2012).

broader sample. Thus, contrary to expectations, we find no evidence that three dimensions contribute to this measure of an individual's affiliation with partisan political coalitions.

Another way to evaluate this mechanism is to test whether these 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. We are cautious with these results as we have a limited number of level-2 units for random coefficients models (Stegmueller 2013). Consistent with KP, low-income targeting steepens the income slope in both rich democracies and the broader sample. When transfers are low-income targeted, higher income individuals are even more significantly opposed to redistribution. Also, somewhat consistent with KP, there is a positively signed interaction between income and universalism ($z \sim 1.6$). However, contrary to KP, extensity significantly steepens the income slope in the broader sample (nearly significantly negative in rich democracies). In sum, the dimensions of transfers may influence political coalitions via cross-level interactions with income. However, some of the patterns are contrary to KP's expectations.

DISCUSSION

This study revisits KP's classic: "The Paradox of Redistribution." KP contend universalism more effectively reduces poverty than targeting because universalism encourages and targeting undermines political support for larger welfare states. Further, targeting and universalism affect poverty because of their effects on extensity, which ultimately reduces poverty. We use LIS data to measure the extensity, low-income targeting, and universalism of welfare transfers actually

received. We slightly revise KP's measures of extensity and low-income targeting and construct a novel measure of universalism. 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.

Several of our results confirm and extend KP. Poverty is significantly negatively associated with extensity, and to a lesser extent 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, and there is some evidence that extensity mediates the relationship between universalism and poverty. It illustrates the significance of KP that these conclusions are robust two decades later in a sample of rich democracies and even with a much broader sample of developed and developing countries.

There are also key differences between our results and KP. Low-income targeting is not robustly positively associated with poverty. Indeed, in the macro-level correlations, low-income targeting is surprisingly negatively associated with poverty. Low-income targeting is significantly positive in one model for poverty (adjusting for extensity and universalism in the broader sample), but is negatively signed in other models. Moreover, universalism is not associated with redistribution preferences. We also find extensity is not significantly related to redistribution preferences, and if anything, extensity significantly increases 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.

What explains the differences between our results and KP? One reason is that, as described in footnotes 15 and 18, KP's sample of rich democracies is much more selective than our sample of rich democracies. However, even in KP's sample of 11 rich democracies, low-income targeting is not positively associated with poverty (r=-.12, see fn. 15) and not negatively associated with redistribution preferences (r=.05, see fn. 18). Instead, another reason is that countries have evolved over time such that some of KP's patterns no longer hold. Working independently, Marx and colleagues (2013) and Kenworthy (2011) demonstrate with repeated cross-sections that KP's positive relationship between low-income targeting and redistribution weakened after the 1980s. For example, the U.S. has become less low-income targeted than it was in the 1980s, and it maintains high poverty and inequality. By contrast, Denmark has become more low-income targeted than it was in the 1980s, and it maintains low poverty and inequality. Therefore, even if the paradox of redistribution was true in the 1980s, the evidence for it has weakened over time.

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 are negatively associated with poverty but are unrelated to redistribution preferences. Indeed, extensity has the largest and most robust effect for poverty, and has a large effect relative to well-studied individual-level predictors of poverty. Yet, extensity is not at all associated with redistribution preferences. Low-income targeting is negatively associated with redistribution preferences, but is not robustly related to poverty. Therefore, what is salient to poverty is not related to redistribution preferences, and what is salient to redistribution preferences is not robustly related to poverty.

31

Second, in the broader sample, the dimensions that are negatively associated with 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 then is likely to weaken redistribution preferences, and weakened redistribution preferences are likely to undermine the preferences and politics 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 undermining public support for redistribution.

Beyond these paradoxes, this study contributes to inequality and social policy literatures. Primarily, it 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 of transfers has 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 is negatively associated with 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; 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 and encourage further research on social policy in developing countries. Mares and Carnes (2009) point out that the literature still lacks basic descriptive information about social policies in developing countries and has not fully mapped differences between developed and developing countries (also Rudra 2007). Our study is one of the first to utilize all LIS countries. Buttressing the emerging literature, developing countries tend to be less extensive, less universal, and more high-income targeted than rich democracies. At the same time, the results show several commonalities across developed and developing countries. While the strength of associations differ, the dimensions mostly have the same signs 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 rises mechanically as the population ages, unemployment increases, or a greater share of the population 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 extensity is salient because it tracks how much the average household's income is socialized and comes from relatively 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 four directions for future research. First, though multi-level models arguably 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 examine variation over-time within countries to control for stable unobserved differences between countries. Second, it would be valuable to expand the outcomes studied. We examine poverty, redistribution preferences, as well as income inequality, a scale of government's responsibility, and Left party affiliation. Still, one could link the country-level measures of dimensions to other datasets to examine other 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 both income/poverty and 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 more into account. Fourth, though it is one of the most widely used concepts in the literature, scholars rarely define or measure universalism. This paper presents one strategy for doing so, and invites research clarifying the conceptual definition of universalism and advancing its measurement.

This study revisits KP's paradox of redistribution by using improved methods and data and refining measures of three 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 with each other, 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	Guatemala	371.14 Q	774.43 Q	2485.96 Q

	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)	(7)	(8)
Extensity	.494***			.442***	.673***			.502**
	(-6.04)			(-3.48)	(-4.12)			(-3.38)
Low-Income Targeting		.850		1.042		.848		1.452*
		(-1.63)		(.46)		(-1.44)		(2.62)
Universalism			.690**	1.088			.749**	1.005
			(-2.96)	(.52)			(-3.32)	(.04)
Age	.903***	.903***	.903***	.903***	.951***	.951***	.951***	.951***
	(-80.65)	(-80.63)	(-80.66)	(-80.64)	(-62.57)	(-62.58)	(-62.58)	(-62.58)
Age^2	1.001***	1.001***	1.001***	1.001***	1.001***	1.000***	1.001***	1.000***
	(55.17)	(55.15)	(55.17)	(55.16)	(40.48)	(40.48)	(40.48)	(40.49)
Single Mother	1.646***	1.646***	1.646***	1.646***	1.618***	1.618***	1.618***	1.618***
	(45.28)	(45.27)	(45.27)	(45.28)	(60.33)	(60.32)	(60.32)	(60.32)
Female Lead No Children	1.677***	1.677***	1.677***	1.677***	1.490***	1.490***	1.490***	1.490***
	(44.81)	(44.81)	(44.81)	(44.81)	(45.18)	(45.17)	(45.17)	(45.17)
Male Lead No Children	1.421***	1.421***	1.421***	1.421***	1.445***	1.445***	1.445***	1.445***
	(27.29)	(27.28)	(27.28)	(27.29)	(36.00)	(35.99)	(36.00)	(36.00)
# Children	1.244***	1.244***	1.244***	1.244***	1.246***	1.246***	1.246***	1.246***
	(73.34)	(73.34)	(73.34)	(73.34)	(148.69)	(148.70)	(148.69)	(148.70)
# Over 64	.555***	.555***	.555***	.555***	.770***	.770***	.770***	.770***
	(-60.12)	(-60.11)	(-60.12)	(-60.11)	(-48.45)	(-48.45)	(-48.45)	(-48.46)

41

Table 2 Continued…

Low Education	2.092***	2.092***	2.092***	2.092***	2.621***	2.621***	2.621***	2.621***
	(87.50)	(87.52)	(87.52)	(87.50)	(161.05)	(161.07)	(161.06)	(161.06)
High Education	.419***	.419***	.419***	.419***	.381***	.381***	.381***	.381***
	(-88.65)	(-88.64)	(-88.64)	(-88.65)	(-113.30)	(-113.30)	(-113.30)	(-113.31)
No Workers in HH	5.502***	5.502***	5.502***	5.502***	3.616***	3.616***	3.616***	3.616***
	(150.25)	(150.24)	(150.24)	(150.24)	(170.81)	(170.80)	(170.81)	(170.81)
Multiple Workers in HH	.184***	.184***	.184***	.184***	.281***	.281***	.281***	.281***
	(-189.40)	(-189.41)	(-189.41)	(-189.40)	(-237.09)	(-237.09)	(-237.09)	(-237.09)
N	1,064,628	1,064,628	1,064,628	1,064,628	1,973,625	1,973,625	1,973,625	1,973,625
Countries	21	21	21	21	38	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)	(7)	(8)
Extensity	937			1.070	.815			.948
	(-.59)			(-.30)	(-1.53)			(-.29)
Low-Income Targeting		.843		.818		.666**		.672**
		(-1.78)		(-1.44)		(-3.33)		(-2.81)
Universalism			1.010	.951			.897	1.034
			(.10)	(-.25)			(-.85)	(.20)
Age	1.021**	1.021**	1.021**	1.021**	1.015*	1.015*	1.015*	1.015*
	(2.86)	(2.86)	(2.86)	(2.86)	(2.39)	(2.40)	(2.39)	(2.40)
Age^2	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(-1.91)	(-1.91)	(-1.90)	(-1.91)	(-1.48)	(-1.49)	(-1.48)	(-1.49)
Female	1.249**	1.249**	1.249**	1.249**	1.196**	1.196**	1.196**	1.196**
	(5.69)	(5.68)	(5.69)	(5.68)	(5.54)	(5.53)	(5.54)	(5.53)
Never Married	1.165**	1.165**	1.165**	1.165**	1.183**	1.182**	1.183**	1.182**
	(2.67)	(2.67)	(2.67)	(2.68)	(3.51)	(3.49)	(3.50)	(3.50)
Divorced	1.132	1.133	1.133	1.133	1.117	1.116	1.117	1.116
	(1.80)	(1.81)	(1.81)	(1.81)	(1.95)	(1.93)	(1.95)	(1.93)
Widowed	1.078	1.078	1.078	1.078	1.009	1.007	1.010	1.010
	(.78)	(.79)	(.79)	(.79)	(.13)	(.09)	(.13)	(.09)
HH Size	1.122**	1.121**	1.122**	1.121**	1.082**	1.081**	1.082**	1.082**
	(5.63)	(5.60)	(5.65)	(5.60)	(5.37)	(5.33)	(5.38)	(5.33)
Children in HH	.855**	.855**	.854**	.855**	.899*	.899*	.899*	.900*
	(-2.80)	(-2.79)	(-2.81)	(-2.79)	(-2.39)	(-2.39)	(-2.40)	(-2.39)
Rural	1.076	1.077	1.075	1.077	1.102*	1.104*	1.101*	1.104*
	(1.33)	(1.35)	(1.32)	(1.35)	(2.17)	(2.20)	(2.16)	(2.20)
Suburb	1.162**	1.162**	1.162**	1.162**	1.121**	1.122**	1.121**	1.122**
	(3.24)	(3.24)	(3.23)	(3.23)	(2.99)	(3.02)	(3.00)	(3.02)

Table 3 Continued…

Low Education	1.621**	1.623**	1.620**	1.623**	1.503**	1.503**	1.502**	1.503**
	(10.00)	(10.03)	(9.99)	(10.02)	(10.30)	(10.30)	(10.28)	(10.31)
High Education	.902*	.902*	.902*	.902*	.857**	.858**	.858**	.858**
	(-2.11)	(-2.12)	(-2.11)	(-2.12)	(-3.72)	(-3.70)	(-3.71)	(-3.71)
Part-Time	1.096	1.098	1.096	1.098	1.107*	1.108*	1.107	1.108
	(1.51)	(1.54)	(1.52)	(1.54)	(1.96)	(1.98)	(1.96)	(1.98)
Unemployed	1.478**	1.477**	1.477**	1.477**	1.166*	1.162	1.165*	1.163*
	(3.18)	(3.18)	(3.18)	(3.18)	(1.99)	(1.95)	(1.98)	(1.95)
Not in Labor Force	1.008	1.008	1.008	1.008	1.059	1.059	1.059	1.059
	(.15)	(.16)	(.15)	(.16)	(1.34)	(1.34)	(1.34)	(1.34)
Self-Employment	.713**	.713**	.713**	.713**	.722**	.723**	.723**	.723**
	(-5.75)	(-5.76)	(-5.76)	(-5.76)	(-6.86)	(-6.86)	(-6.85)	(-6.86)
Public Employment	1.329**	1.330**	1.328**	1.330**	1.338**	1.338**	1.337**	1.338**
	(6.28)	(6.30)	(6.26)	(6.29)	(7.54)	(7.55)	(7.53)	(7.54)
Relative Income	.696**	.696**	.696**	.696**	.768**	.768**	.768**	.768**
	(-16.47)	(-16.45)	(-16.48)	(-16.45)	(-15.84)	(-15.85)	(-15.86)	(-15.84)
Low Religious Attendance	.884**	.883**	.884**	.883**	.905**	.904**	.904**	.904**
	(-2.81)	(-2.83)	(-2.81)	(-2.83)	(-2.72)	(-2.74)	(-2.72)	(-2.74)
High Religious Attendance	.925	.925	.926	.925	.992	.991	.991	.992
	(-1.27)	(-1.28)	(-1.26)	(-1.27)	(-.16)	(-.18)	(-.17)	(-.17)
N	15,887	15,887	15,887	15,887	26,752	26,752	26,752	26,752
Countries	16	16	16	16	25	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.
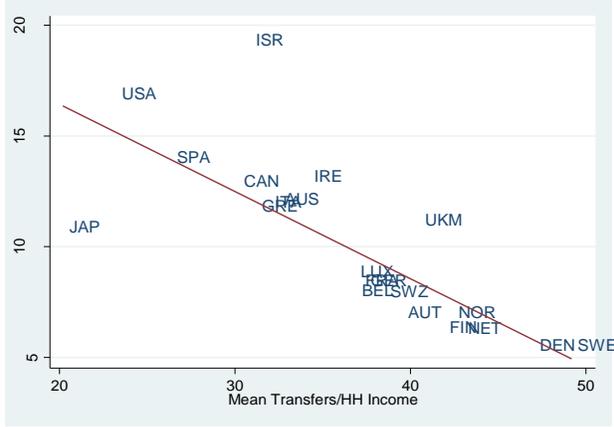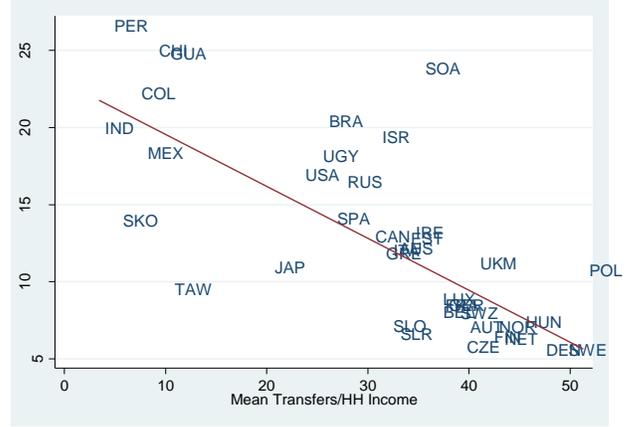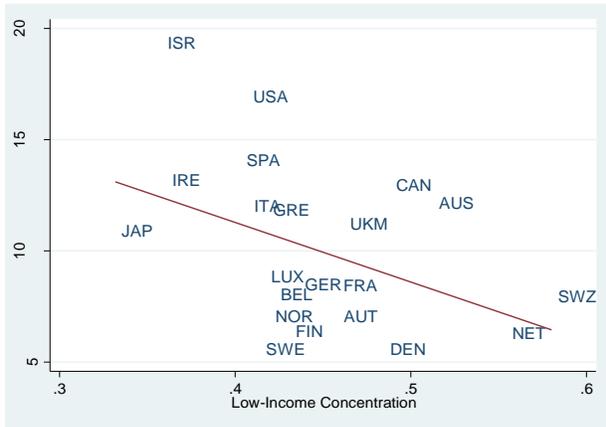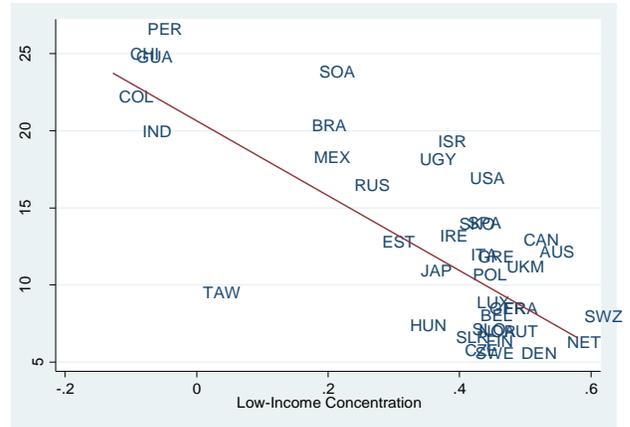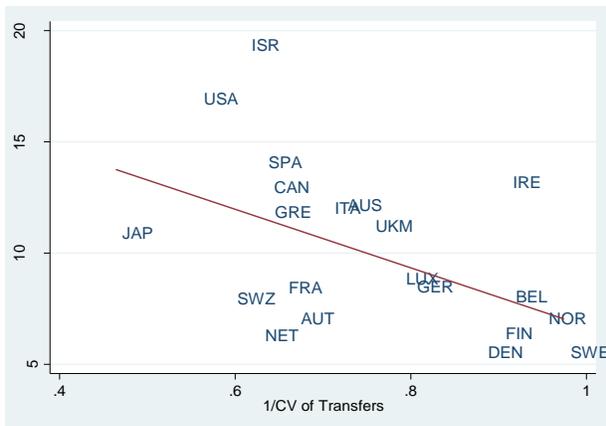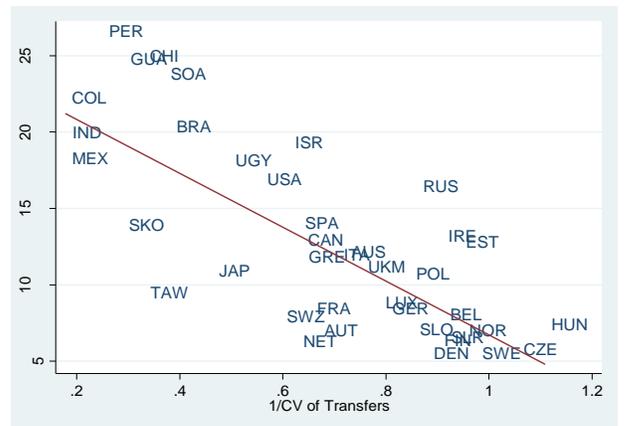
Figure 2. Macro-Level Bivariate Associations Between Proportion Supporting Redistribution and Dimensions of Welfare Transfers.

a) RICH DEMOCRACIES (N=16)
Extensity (r=-.02)

b) BROADER SAMPLE (N=25)
Extensity (r=-.19)
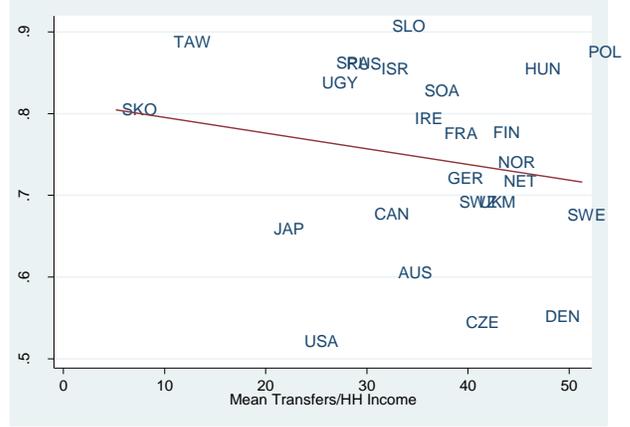
Low-Income Targeting (r=-.30)
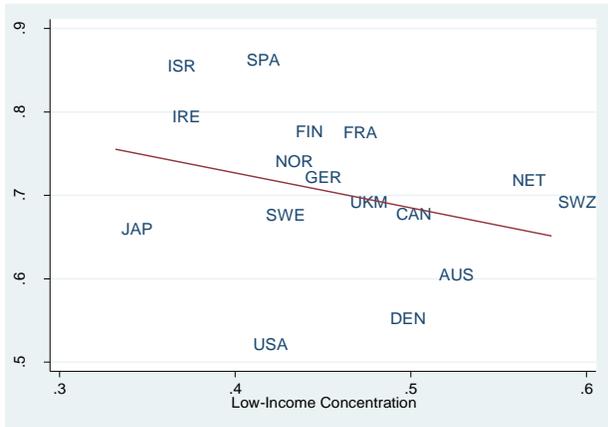
Low-Income Targeting (r=-.50)

Universalism (r=.07)
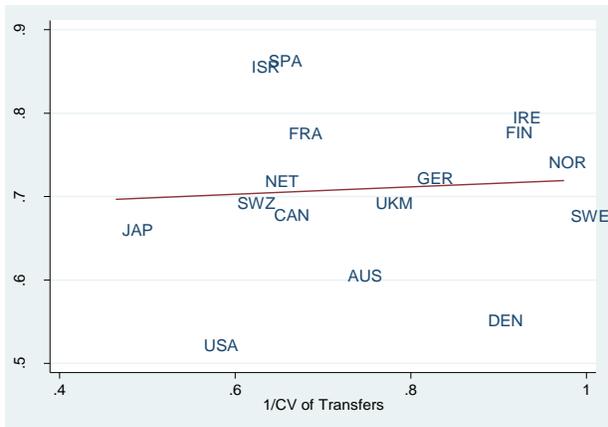
Universalism (r=-.15)
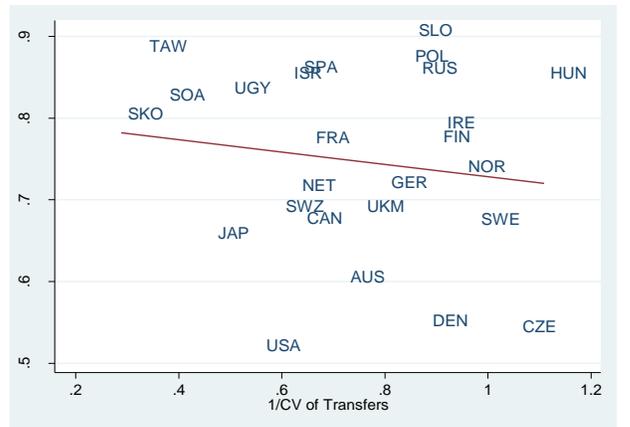
Figure 3. Macro-Level Bivariate Associations Between Dimensions of Welfare Transfers.

a) RICH DEMOCRACIES (N=21)

Extensity & Universalism (r=.71)

Extensity & Low-Income Targeting (r=.45)

Low-Income Targeting & Universalism (r=.07)
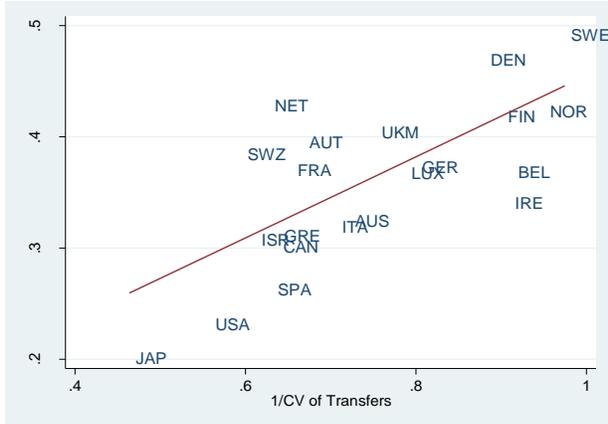
b) BROADER SAMPLE (N=39)
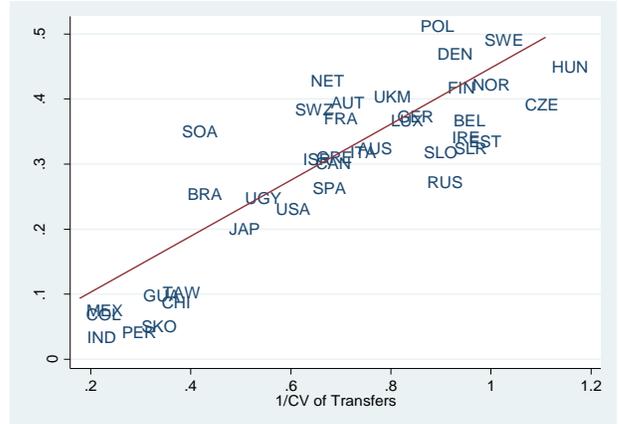
Extensity & Universalism (r=.85)
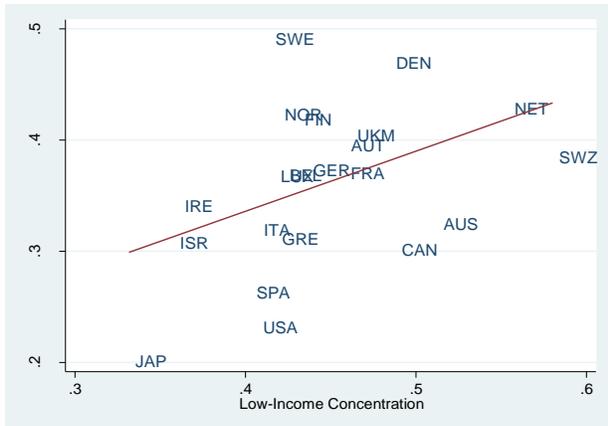
Extensity & Low-Income Targeting (r=.78)
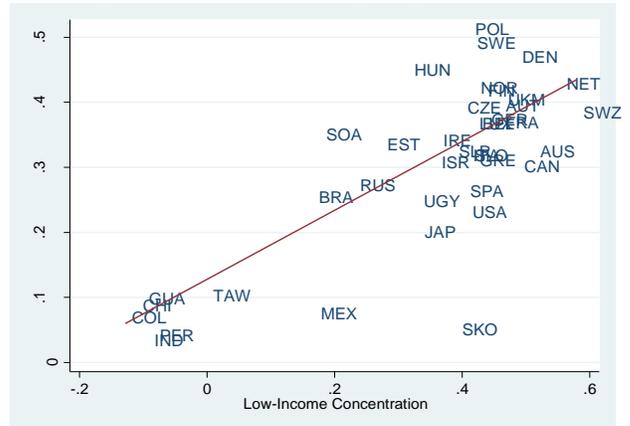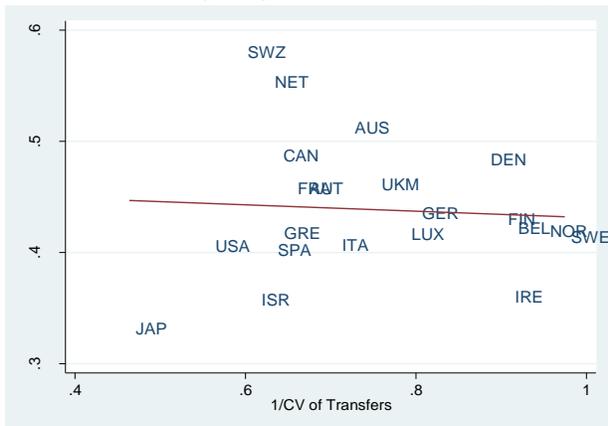
Low-Income Targeting & Universalism (r=.67)
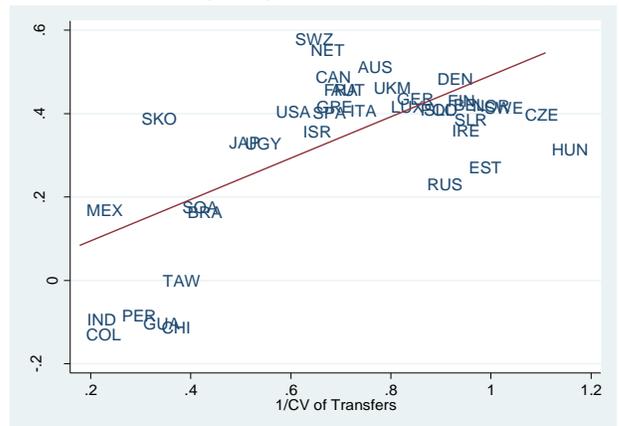
Appendix I. Descriptive Statistics: Means and Standard Deviations in Parentheses.

	LIS-Rich Democracies	*LIS-Broader Sample*	*ISSP-Rich Democracies*	*ISSP-Broader Sample*
Poverty	.119 (.324)	.148 (.355)	--	--
Redistribution Preferences	--	--	.691 .462	.755 (.430)
Extensity	32.841 (9.696)	24.596 (15.649)	35.314 (8.120)	31.949 (11.941)
Low-Income Targeting	.435 (.044)	.270 (.231)	.445 (.063)	.367 (.138)
Universalism	.690 (.147)	.541 (.260)	.730 (.147)	.679 (.225)
Age	45.435 (14.595)	44.963 (14.809)	48.458 (16.423)	47.054 (16.858)
Age^2	2277.331 (1474.291)	2240.971 (1485.272)	2617.908 (1666.871)	2498.294 (1682.351)
Single Mother	.078 (.268)	.066 (.248)	--	--
Female Lead No Children	.096 (.295)	.074 (.261)	--	--
Male Lead No Children	.083 (.275)	.057) (.233)	--	--
# Children	1.156 (1.312)	1.513 (1.620)	--	--
# Over 64	.241 (.574)	.268 (.583)	--	--
Low Education	.218 (.413)	.400 (.490)	.380 (.485)	.403 (.491)
High Education	.338 (.473)	.244 (.430)	.202 (.401)	.179 (.383)
No Workers in HH	.142 (.349)	.135 (.342)	--	--
Multiple Workers in HH	.576 (.494)	.533 (.499)	--	--
Female	--	--	.514 (.500)	.532 (.499)
Never Married	--	--	.224 (.417)	.239 (.427)
Divorced	--	--	.096 (.295)	.092 (.288)
Widow	--	--	.061 (.239)	.082 (.275)
HH Size	--	--	2.778 (1.451)	3.028 (1.660)

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 in Figures	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
Slovak Republic	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.
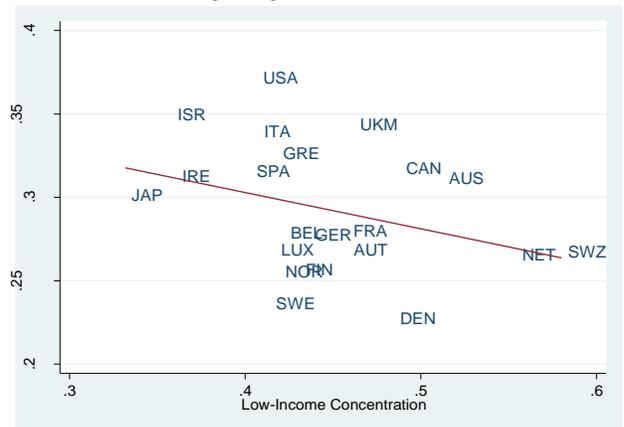
a) RICH DEMOCRACIES (N=21)
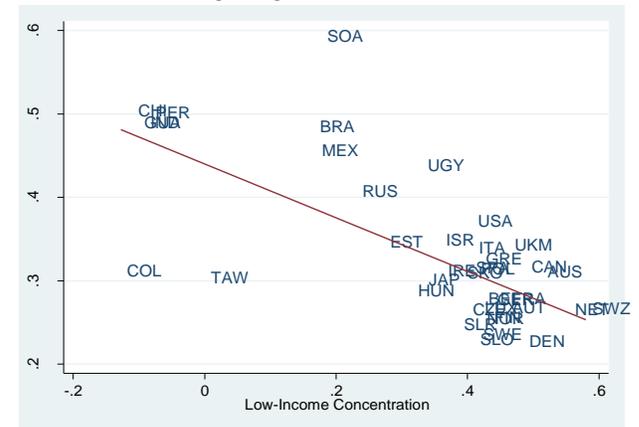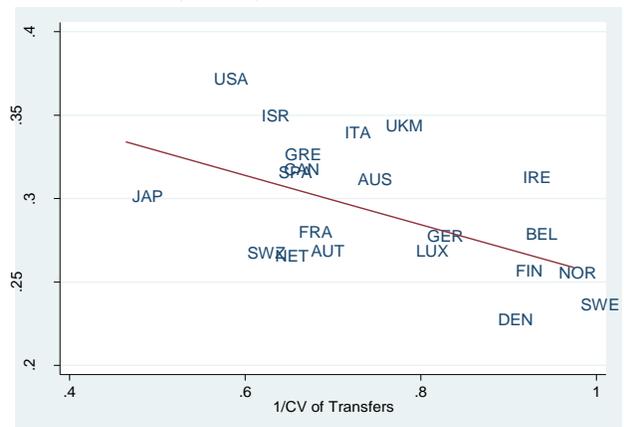Extensity (r=-.75)
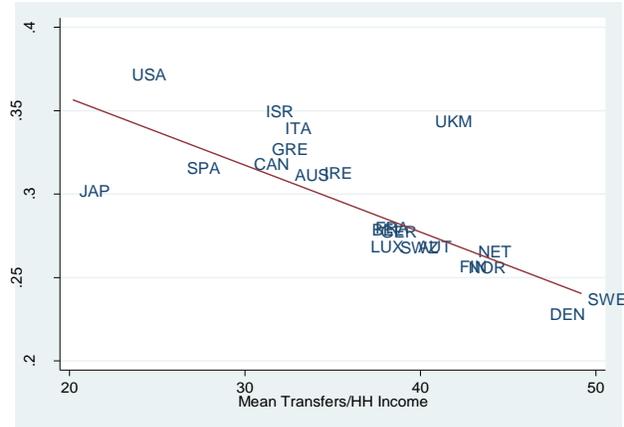
b) BROADER SAMPLE (N=39)
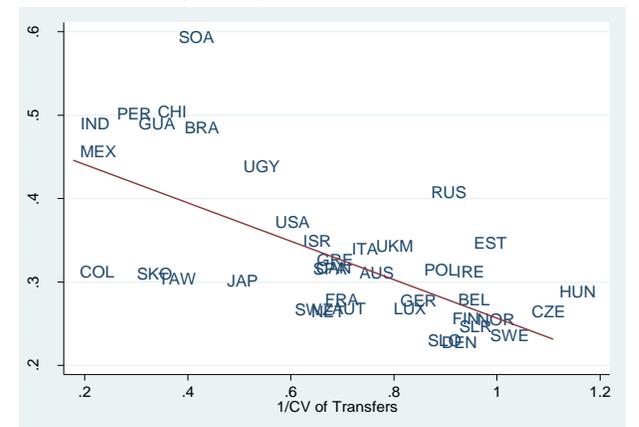Extensity (r=-.59)
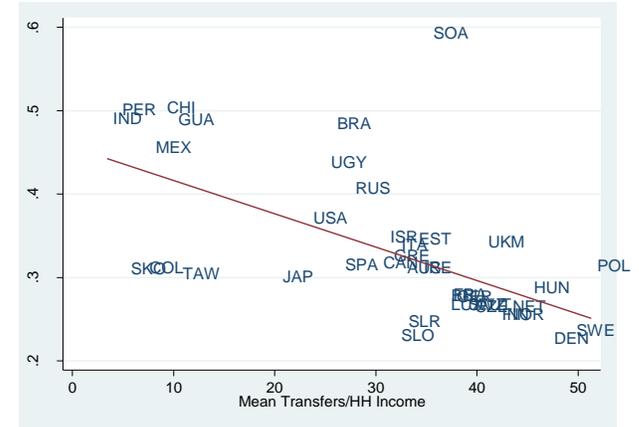
Low-Income Targeting (r=-.34)

Low-Income Targeting (r=-.69)

Universalism (r=-.54)

Universalism (r=-.67)

Appendix IV. Multi-Level Mixed Models of Scale of Government Responsibility and Left-Right Party Affiliation (Individual-Level Variables Not Shown): Coefficients and (Z-scores).

	Scale of Govt. Responsibility		Left-Right Party Affiliation	
	Rich Democracies	*Broader Sample*	*Rich Democracies*	*Broader Sample*
Extensity	-.003	.004	.002	-.002
	(-.24)	(.66)	(.16)	(-.43)
Low-Income Targeting	-1.050	-1.129**	-.383	-.332
	(-1.02)	(-2.76)	(-.32)	(-.74)
Universalism	1.021	.311	.361	.223
	(1.64)	(1.06)	(.51)	(.85)
N	16,529	27,729	11,865	17,889
Countries	16	25	15	23

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

Notes: All individual-level variables from Table 3 are included but not shown. The results are consistent if the models are decomposed into separate models for each dimension. The Left-Right party affiliation variable is coded: 1=Far Right, 2=Right, Conservative, 3=Center, Liberal, 4=Left, Center Left, 5=Far Left. There are less countries in these models because of missing data for Israel and Taiwan. The six questions in the scale of government responsibility ask whether it should or should not be the government's responsibility to: "provide a job for everyone who wants one;" "provide a decent standard of living for the unemployed;" "provide a decent standard of living for the old;" "provide decent housing for those who can't afford it;" "provide healthcare for the sick;" and "reduce income differences between rich and poor." Using the underlying ordinal responses, the standardized item alpha for this scale is .77.

Appendix V. Multi-Level Random Coefficient Logit Models of Redistribution Preferences Interacting Income and Dimensions of Welfare Transfers: Coefficients and (Z-Scores).

	(1) *Rich Democracies*	*(2)* *Broader Sample*	*(3)* *Rich Democracies*	*(4)* *Broader Sample*	*(5)* *Rich Democracies*	*(6)* *Broader Sample*
Income	.935	.050	.270	.153	-.502**	-.414**
	(-.46)	(.56)	(1.35)	(1.76)	(-5.35)	(-4.60)
Income*Dimension	-.008	-.010**	-1.416**	-1.127**	.258	.218
	(-1.92)	(-3.93)	(-3.20)	(-5.30)	(1.64)	(1.56)
Extensity	-.007	-.015				
	(-.51)	(-1.40)				
Low-Income Targeting			-2.496	-2.785**		
			(-1.66)	(-3.15)		
Universalism					.078	-.450
					(.11)	(-.80)
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: All individual-level variables from Table 3 are included but not shown.

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

```
program define welfdim
*drop missing values for disposable household income*
drop if dhi==.

*drop if household weight missing, recode household weight*
drop if hwgt==.
replace hwgt=0.01 if hwgt==0
gen pwt=hwgt*nhhmem

*create, bottom- and top-code equivalized household income*
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)

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

*calculate extensity*
gen exten=100*(transfer/eqinc)

*create pre-transfer (posttax) household income*
gen pretrinc=eqinc-transfer
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
```