From Preferences to Voting: Redistribution and Left Parties in Industrialized Democracies*

Draft

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Abstract

While a significant literature in political economy has recently focused on the relationship between income and risk, on the one hand, and redistribution preferences, on the other, it is unclear whether these preferences have any influence over political behavior. In this paper we argue that redistribution preferences are indeed a most significant determinant of voting. We test our theoretical claims with data from Western Europe and the US and show that voting for redistributive parties is highly dependent on individual levels of demand for redistribution. The poor (and, to a lesser degree, those exposed to more risk) are more supportive of redistribution and, we contend, these redistribution preferences make them more likely to vote for redistributive parties. Our analysis goes beyond previous research by explicitly studying this preference mechanism in a potential-outcomes framework. We disentangle the direct and indirect effects of income and risk (as well as other factors) to obtain estimates of their effects on voting through preferences.

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I. Introduction

Most analysts would agree that an individual's relative income (i.e., whether she is rich or poor) affects her redistribution preferences. But why should we care about redistribution preferences in the first place? We argue that the (often implicit) model behind much of comparative politics and political economy starts with redistribution preferences. These redistribution preferences affect how individuals behave politically and their behavior in turn affects the strategies of political parties and the policies of governments. In this paper, we will focus on perhaps the most momentous potential consequence of redistribution preferences: voting.

Inequality and redistribution have seen a resurgence in academic interest in recent times. This is particularly the case in the US, where Bartels (2009) has shown the spectacular increase in inequality over the past 35 years to be the product of policy choices in a political system dominated by partisanship and particularly receptive to the preferences of the wealthy. Hacker and Pierson (2011) coincide not only in the appreciation of the attention that policy-makers pay to the rich but also about the fact that politics is the main factor behind inequality ("American politics did it").

The connection between inequality and political behavior, however, remains unclear. A number of observers would deny that income and inequality are significant determinants of voting.[1] Some analysts would agree that an individual's income affects her political behavior,[2] but they would not necessarily agree on the reasons why this is the case. This paper's analysis addresses one of the implications of most arguments about the importance of economic circumstances to political outcomes. If income and risk matter to individual political behavior, it seems reasonable to assume that they do so through their influence on redistribution preferences. These redistribution preferences may (or may not) then be reflected on party positions and, eventually, government policy.

While a voluminous political economy literature has emerged on the influence of income and risk on preferences, we know much less about whether these preferences do in fact affect political behavior at all. Most political economy arguments start from the assumption that an individual's position in the income distribution determines her preferences for redistribution. The most popular version of this approach is the theoretical model proposed by

[1] See, for example, Green, Palmquist, and Schickler (2004) and Lewis-Beck (2009) or, more recently, Achen and Bartels (2016).

[2] There is an influential literature in political science on how pocketbook issues and class (both closely related to income) influence voting. See Downs (1957), Key (1966) or Fiorina (1981) on pocketbook issues and Lipset (1983), Evans (1999) or Brooks and Manza (1997) on class.

Romer (1975) and developed by Meltzer and Richard (1981). And there is some evidence supporting the argument that relative income (whether an individual is rich or poor) influences preferences for redistribution. A relative income effect is found in the US by, among others, Gilens (2005), McCarty, Poole, and Rosenthal (2008), and Page and Jacobs (2009). Using comparative data, Bean and Papadakis (1998), Finseraas (2009), and Shayo (2009) (again, among others) find similar effects. Do these preferences translate into political behavior?

As we will show below, redistribution preferences (which are in turn affected by relative income and exposure to risk) are an important factor affecting voting behavior in Western Europe. Our arguments add to those prevalent in the literature in three important ways. First, we provide an argument and convincing evidence that income and risk are in fact significant determinants of voting. Second, most political economy models link individual income (or exposure to risk) to policy outcomes making the essential assumption that there is a relationship between preferences and voting. We specify explicitly the theoretical mechanisms that determine preferences and party choice, and test them empirically. Third, much of the recent debate about the lack of redistributive policies in industrialized democracies has centered around the perception that second-dimension issues are disproportionately important to the poor. Perhaps the most well-known example of this is the contention that cultural, religious and social values outweigh economic concerns for the American working class in some states (see Frank 2004 and, more recently, Hersh and Nall 2015). The implication of these arguments is that the solution to the puzzle affecting (the lack of) redistribution in industrialized democracies concerns demand. We show in this paper that this may not be the case. We find the poor (and those exposed to risk) to be uniformly in favor of redistribution and therefore uniformly more likely to vote for redistributive parties. The puzzle of redistribution may have more to do with supply (what parties do, the effects of electoral institutions, etc) than with demand.

II. Argument

Our theoretical argument proceeds in two stages. First, we address the formation of preferences for redistribution, explaining why income and risk are important determinants of demand for redistribution. Second, we detail the influence of redistribution preferences on voting choices. We argue that those who are supportive of redistribution will be more likely to vote for redistributive parties.

II.A. Inequality and redistribution preferences

The first side of our argument involves the relationship between individual levels of income and redistribution preferences. Political economy approaches that start from the assumption that an individual's position in the income distribution determines her preferences for redistribution are often inspired by the theoretical model proposed by Romer (1975) and developed by Meltzer and Richard (1981). To recapitulate very briefly, the RMR model assumes that the preferences of the median voter determine government policy and that the median voter seeks to maximize current income. If there are no deadweight costs to redistribution, all voters with incomes below the mean maximize their utility by imposing a 100% tax rate. Conversely, all voters with incomes above the mean prefer a tax rate of zero. When there are distortionary costs to taxation, the RMR model implies that, by increasing the distance between the median and the mean incomes, more inequality should be associated with more redistribution.

While it is the case that the rich support redistribution less than the poor almost everywhere, the strength of this relationship is hardly consistent (Dion 2010; Dion and Birchfield 2010; Beramendi and Rehm 2014). It is clear that the idea that material self-interest determines redistribution preferences should not be limited to a measure of present income. In the words of Alesina and Giuliano, "(e)conomists traditionally assume that individuals have preferences defined over their lifetime consumption (income) and maximize their utility under a set of constraints. The same principle applies to preferences for redistribution. It follows that maximization of utility from consumption and leisure and some aggregation of individual preferences determines the equilibrium level of taxes and transfers" (2011: 1).

Because of the potential to define material self-interest inter-temporally (as lifetime consumption/income), this approach extends the more direct focus on effects of contemporary relative income (as in Romer 1975 and Meltzer and Richard 1981) and opens the door to arguments about about social insurance and risk (as in Sinn 1995; Moene and Wallerstein 2003; Iversen and Soskice 2001; Rehm 2009; Mares 2003), and about social mobility and life-cycle profiles (Rueda and Stegmueller 2017; Alesina and Giuliano 2011; Haider and Solon 2006; Benabou and Ok 2001).

Arguments about the importance of insurance are most relevant to our focus in this paper. They have emphasized the importance of risk in determining redistribution and insurance preferences. In this vein, Rehm (2009, 2016) argues that, while income captures redistribution preferences, occupation characteristics capture risk exposure and insurance motivations. In a highly influential article, Iversen and Soskice (2001) argue that exposure to risk is inversely related to the portability of individual skills. While we agree with Iversen and Soskice that individual expected utility (across a range of possible labor market stages) is a key factor in determining redistribution preferences, we do not emphasize the difference between general and specific skills here. Instead, we will use Rehm's occupational unemployment as our proxy for exposure to risk.

As in the Meltzer-Richard model, our argument implies that a rise in income will reduce the demand for distribution. Our argument also implies that the immediate pocketbook consequences of inequality are fully contained in the individual income distance changes produced by this inequality shift. In other words, the tax and transfer consequences of inequality (and their effects on individual demands for redistribution) are picked up by individual income changes. As in Rehm (2009, 2016), we argue that while income captures redistribution preferences, occupational unemployment captures risk exposure and insurance motivations. The higher the risk an individual is exposed to, the more supportive of redistribution she will be.

II.B. Redistribution preferences and vote choice

In the second stage of our argument, we argue for the relevance of redistribution preferences to voting.[3] We therefore follow a well-established literature on the relationship between economic considerations and political behavior. As mentioned above, most political economy arguments start from the assumption that an individual's redistribution preferences affect her political choices (see Romer 1975 and Meltzer and Richard 1981). The literatures on economic voting and class voting are based on similar arguments. Like authors

[3] As mentioned above, an important literature posits that, in some cases, the poor are diverted from the pursuit of their material self-interest. Perhaps the most well-known example of these arguments is the contention that second-dimension issues (particularly cultural and social ones) outweigh economic ones for the American working class. Frank (2004) and the critique in Bartels (2006) are good illustrations of this debate, but so is the emphasis on cosmopolitanism as a determinant of vote in Gelman et al. (2008). More comparatively, the important contribution in Shayo (2009) to the political economy of identity formation follows a similar logic. Shayo's theoretical model emphasizes two identity dimensions: economic class and nationality. As a result of status differences, the poor are more likely than the rich to identify with the nation rather than their class in high inequality countries. Because they take group interests into account, moreover, the poor who identify with the nation are less supportive of redistribution than the poor who identify with their class. While not denying that moral and cultural issues are important to voting Democrat in the US, we emphasize the importance of redistribution preferences. McCarty et al. find that income is an extraordinarily good predictor of partisanship and voting even among conservative Christians in the US (2008: 100-101). In the same vein, we will show below that the influence of income and risk through redistribution preferences are a powerful predictor of voting even when controlling for the influence of other channels of influence (such as values).

in the economic voting tradition (e.g., Duch and Stevenson 2008), our argument posits that there is a relationship between an individual's economic interests and her likelihood to vote for a particular party. Class voting analyses (e.g., Evans and de Graaf 2013 and Evans 1999) emphasize the effects of socio-economic cleavages on political preferences, but their focus on occupational factors is largely compatible with our arguments. Our approach is also related to a recent literature that emphasizes risks and skills as determinants of preferences. While this literature associates unemployment vulnerability with skill profiles (e.g, Cusack, Iversen, and Rehm 2006), we highlight the direct effects of redistribution preferences (regardless of skills).

Like the traditional economic voting literature (Downs 1957) we conceive of voters as instrumental rational actors. Individuals will vote following a comparison of what they gain or lose from the policies proposed by each party. In the words of Duch and Stevenson, we assume that "voters rationally derive expected utilities for competing political parties and that these determine their vote choice" (2008: 9). As in the pioneering work of Kramer (1971) and Fair (1978), we consider that economic well-being (and therefore redistribution and insurance) is a significant factor affecting a voter's utility function.

A substantial literature debates the issue of how exactly economic considerations enter a citizen's vote choice function. Two main approaches can be distinguished, one emphasizing *sanctioning* and the other focusing on *selection* (here, we follow the analysis provided in Duch and Stevenson 2008). The *sanctioning model* is characterized by the consideration that voters are narrowly retrospective and mostly motivated by punishing or rewarding incumbents (see the classic works of Kramer 1971, Key 1966 and Fiorina 1981). Focusing on moral hazard, i.e., the risk of rent-seeking by incumbents if not punished for bad economic outcomes, Barro (1973) and Ferejohn (1986) also belong within this tradition. The *selection/competency model* argues that voters gather more information to assess the likely economic outcomes associated with competing political alternatives. Downs (1957) and Stigler (1973) are classical examples of this approach but we would argue that this is also the understanding of voting underlying Meltzer and Richard (1981) and subsequent political economy treatments of redistribution and voting (Persson and Tabellini 2000). While not incompatible with sanctioning, our argument more clearly implies a selection logic. We propose that individuals who are in favor of redistribution and insurance will identify the party more likely to promote equality and therefore be more likely to vote for it.

More specifically, in our analysis we consider voting to be a discrete choice. By this we mean a decision made over a set of exclusive and exhaustive choices (see Duch and Stevenson 2008: 39). Each voting choice (i.e., the parties a voter can select) offers some utility with regards to the voter's redistribution preferences. It is the contribution of these individual redistributive preferences to the voting choice that matters to the main focus of our paper, but our approach can be described in more general terms. Like Alvarez, Nagler, and Willette (2000: 240), we assume that each individual obtains some utility from each party, and that the individual votes for the party offering the highest utility. The utility of each party is understood to be a function of a set of systematic components (specific to the voter, to the party and to the election) and a random disturbance. The parameters in these random utility models are often estimated with multinomial probit techniques using distance variables as the predictors. These variables reflect the spatial distance between a respondent's position on an issue (in our case, redistribution) and the respondent's view of each party's position on the same issue (for examples of this approach see Alvarez and Nagler 1995, 1998). In the analysis of American data below, we use an explicit measure for redistributive distance (we provide the details below). In our analysis of European data, we lack information on the respondent's views of each party's position and we use party manifesto information on party positions instead.[4] In both cases, we explore individual vote choice as an unobserved vector of probabilities associated to the redistributive positions of different parties.

The intuition linking redistributive preferences to voting choice explained above is pretty straightforward, but it has arguably not received enough attention in the existing Comparative Political Economy literature.[5] The equilibrium in most political economy models is achieved by individuals deriving their preferences over optimal fiscal policy based on their income position (or their occupational or labor market position), which are then "aggregated into an economywide policy via the collective choice mechanism in place" (Drazen 2000: 312). Thus, the two central concepts are citizens' redistribution preferences (or ideal points) and vote choices (the collective choice mechanism). The traditional modes of empirical analysis have then been (i) to explore the influence of income on voting and (ii) to relate income to economy-wide outcomes, such as spending (see, e.g., the summaries of

[4]We sacrifice an explicit measure of spatial distance distance on redistribution here to maximize the coverage of countries and years in our analysis.

[5]This is also the case in the American Politics literature (McCarty, Poole, and Rosenthal 2008). Two clear illustrations of this are major recent works on partisan identification and voting by Green, Palmquist, and Schickler (2004) and Lewis-Beck (2009) (the more recent contribution by Achen and Bartels (2016) could also be added here). Both analyses underplay the importance of income (and, even more so, its connection to redistribution preferences). To the extent that income and redistribution preferences are considered in this literature, it is through the prism of "class voting." (Lewis-Beck (2009) finds class to have become less significant a determinant of voting in presidential elections, while Manza and Brooks (1999) find the class cleavage to be stable from 1952 to 1996.But this approach is quite distinct from the political economy arguments that we present in this paper.

empirical research in Persson and Tabellini 2000 and Mueller 2003). This, however, simply assumes that our central argument – the relationship between preferences and voting – is indeed the mechanism at work. This paper's contribution is to specify explicitly the theoretical mechanisms that determine preferences and party choice, and to test them empirically.

II.C. Defining the causal channels

To make transparent how we think about these two relationships conceptually, we explicitly define our hypothesized mechanisms (Robins 2003; VanderWeele and Vansteelandt 2009; Imai et al. 2011). We do so using the potential outcomes framework, since it allows for a transparent notation of our quantities of interest. We hasten to add that using causally defined quantities does not automatically imply that resulting estimates are causal. The (many) limits of observational data analysis still apply. Rather, in our view (and that of Imai, Keele, and Yamamoto 2010) the key benefit of clearly defining mechanisms in a potential outcomes framework is that it lays bare the identifying assumptions needed. We state these assumptions explicitly and conduct sensitivity analyses to see how robust our results are against violations.

Start with a scenario where individual i ($i = 1, \ldots, N$) receives some level of income, w_i and faces some level of risk/occupational unemployment, z_i. Our individual prefers a certain level of redistribution, which is a function of her income and occupational unemployment risk, which we write as $R_i(z_i, w_i | x_{1i})$. Possibly confounding variables (individual and contextual characteristics) are denoted by x_{1i}. At election time she casts her vote based on her redistribution preferences and on a number of other factors. We write this vote function as $V_i(z_i, w_i, R_i(z_i, w_i | x_{1i}) | x_{2i})$. Again, we allow for a set of possible confounders, x_{2i}. Note that income and occupational unemployment risk appear twice: as factors changing preferences (which in turn shape vote choice) and as factors directly shaping vote choice (via possibly infinitely many other possible channels).

To understand the role of, for example, income, examine a (counterfactual) shift in income from w_i to w_i'. Holding everything else constant, the *total unit effect* of income on vote choice is given by (we omit possible confounders for clarity):

$$TE \equiv V_i(z_i, w_i, R_i(z_i, w_i)) - V_i(z_i, w_i', R_i(z_i, w_i')) \quad (1)$$

This is the expected (counterfactual) difference in the probability of voting for a redistributive party as a result of changing income. It results from the combination of the systematic effects of changing preferences and all other factors, which are not relevant to our argument.

To understand *how* income shapes voting *via* preferences it is not enough to look at disparate sets of regression coefficients (of, say, income on preferences, and preferences on voting). Rather, we need to explicitly state our hypothesized mechanism. We define this *indirect effect* following Pearl (2001) as:

$$IE \equiv V_i(z_i, w_i, R_i(z_i, w_i)) - V_i(z_i, w_i, R_i(z_i, w_i')). \tag{2}$$

This is the effect a change in income has on vote choice via redistribution preferences *only*. By fixing income and only changing preferences, we isolate our preference mechanism and eliminate the impact of competing mechanisms (Imai et al. 2011: 769). In other words, it is a strict statistical expression of our hypothesized income–preference nexus *net* of alternative channels (such as, for example, second dimension concerns).

The remaining effect of changes in income on vote choice not transmitted via preferences is termed the *direct effect* and represents how income affects vote choice in ways that are not considered in our model (i.e., all mechanisms other than redistribution preferences):

$$DE \equiv V_i(z_i, w_i, R_i(z_i, w_i)) - V_i(z_i, w_i', R_i(z_i, w_i)). \tag{3}$$

The previous discussion lays out the definition of our key quantities and is independent of the specific statistical model used to estimate it (cf. Imai, Keele, and Yamamoto 2010). Its value lies not only in stating clearly what we want to know, but also in making explicit the central identifying assumptions needed to estimate these quantities (VanderWeele 2010; Imai et al. 2011).[6] The first is the standard assumption that, after conditioning on included observables, there are no unobserved confounders that change with treatment (e.g., income) and affect vote choice (V_i) or preferences (R_i). The second assumption concerns the mediating variable, namely redistribution preferences. It requires that no unobserved confounders affect both V_i and R_i after conditioning on observables (x_{1i}, x_{2i}). Since *both* assumptions have to be made jointly to estimate mediated effects, Imai et al. (2011) refer to them as 'sequential ignorability.' In our empirical application, as in any analysis having to rely on observational data, we accept that these conditions are likely to be violated to some degree. We therefore use sensitivity analyses to gauge how increasingly severe violations of these identifying assumptions influence our results.

[6]The usual assumptions of standard regression models still apply. What we focus our discussion on are additional assumptions needed to decompose different mechanisms.

We describe the statistical model used to estimate the quantities described above in Section III. Let us emphasize again that this setup provides a rather strict test of our hypotheses. We test if income and risk systematically shapes vote via redistribution preferences while allowing for (an unspecified number of) other channels by which risk and income could be linked to vote choice.

III. Data and Statistical Specification

III.A. Data sources

For our Western European analysis, we use data from six waves of the European Social Survey (ESS), collected between September 2002 and December 2013. It is a large scale multi-country survey administered bi-annually in European countries starting in 2002.[7] Its target population are all individuals aged 15 or over, residing in private households (regardless of nationality, language, citizenship or legal status). The ESS provides a measure of income that is applied consistently over countries and survey waves, and which provides enough detail for us to construct a usable measure of an individuals' income distance to the national mean. We select countries who participated in at least two rounds. For each election between 1999 and 2013, we match the corresponding waves from the ESS. If multiple waves were available, we use the one closest to the last election. Table A1.1 in the appendix shows survey fieldwork periods and election dates for waves included in our analysis.

The influence of redistribution preferences is the main focus in this paper's analysis of voting. For this reason, it is of paramount importance that the voting data coincides with the redistribution preferences data. As explained in more detail below, respondents are asked about the parties they voted for in the previous national election. At the time of the survey, these elections have taken place in the past while redistribution preferences are measured in the present. It is important therefore to restrict the analysis to ESS waves when this coincidence of data is reasonable.[8] This also requires special attention to when the surveys were actually conducted. The ESS surveys are fielded over a period of months, often starting at the end of the wave year and running into the following one. In the analysis, we only include ESS surveys when a national election has been held the same year of the wave or the year before (so that redistribution preferences are plausibly connected with

[7] For more information see `www.europeansocialsurvey.org/`.

[8] The same considerations apply to measures of relative income and risk, which are part of the redistribution preferences estimation.

voting behavior). We also eliminate surveys that were conducted in months that include an election (and therefore may contain voting choices for different elections depending on the respondent's interview date). In practical terms, this means the analysis matches macro-micro data for 41 elections held in 14 countries, namely Austria, Belgium, Germany, Denmark, Spain, Finland, Great Britain, Ireland, Italy, the Netherlands, Norway, Portugal, Sweden, and Switzerland.

For our analysis of the United States, we use data from the American National Election Study (ANES) Time series studies administered bi-annually to a sample of a cross-section of eligible voters in the US. We select surveys starting in 1982 (when our redistribution preference measure becomes available) and ending in 2004. We only keep the subset of individuals with available responses on both their government-spending ideal points and perceived party positions. This leaves us with 5,260 individuals.[9]

III.B. Measures

Vote choice Our main dependent variable is an individual's choice to vote for a redistributive party. Recall from our theoretical intuitions above that we model voting as a discrete choice influenced by the distance between an individual's redistribution preferences and the redistributive positions of the parties she can vote for. This approach requires us to define whether a party is redistributive or not. In the US context, this is straightforward enough. Our dependent variable translates into a respondent choosing the Democratic Party (which consistently offers relatively more redistributive policy positions) over the Republican alternative.

In the Western European multi-party context, this issue is more complex. On the one hand, we could use a party 'label' as the indicator of redistributive position. In this approach, a 'left' party would be considered a redistributive party by virtue of its ideology and its commitments to historically meaningful groups of voters. The existence of stable ideological and historical connections between parties and some social groups "not only creates easily identifiable choices for citizens, it also makes it easier for parties to seek out their probable supporters and mobilize them at election time" (Powell 1982: 116). To the extent that party labels are used as information shortcuts by voters to capture a party's redistributive position, this is an attractive strategy. In the analysis below, we classify parties as 'left' if their party family (as recorded by the Comparative Manifesto Project, CMP) is either socialist/social

[9]Multiple imputation does not yield substantively different results. Results available from the authors.

democratic or communist.[10]

Labels, ideology and history, however, are not enough. Elections need to be contested and they inevitably revolve around issues, like redistribution, that give political meaning to partisan attachments and social divisions (Dalton 2002: 195). Moreover, in our analysis of Western Europe, simply classifying parties based on their label might not constitute a proper operationalization of the concept of redistributive voting, since country- as well as election-specific factors influence parties' position on redistribution.

We therefore construct an alternative dependent variable based on of how much redistribution a party proposes in their electoral platform. Using data from the Comparative Manifesto Project (Budge et al. 2001) and its 2016 update (Volkens et al. 2016), we calculate the extent to which parties favor state involvement in the economy – a measure of redistributive politics proposed by Benoit and Laver (2006, 2007).[11] It is calculated from parties' statements to multiple economic topics (represented by "quasi sentences" in the CMP data set), which are combined into a measure of a party's policy position as the balance of positive (P) to negative (N) statements following Lowe et al. (2011): $\theta = \log \frac{P+.5}{N+.5}$. Parties can occupy any position on this scale, but more extreme positions need considerable more relative emphasis, yielding a magnitude scaling of policy positions.[12] This yields interval level information on the redistributive policies of almost all European parties, with smaller values of θ implying a more pro-redistributive position.[13]

For the following analyses, we create a binary variable indicating if the party that an individual has chosen favors redistributive policies. We classify a party as redistributive if it occupies a policy position below the country-election specific redistribution policy mean, in other words, when it proposes more redistribution than the (hypothetical) average party.[14] This is the preferred strategy, since the interval level measure of party policy does not imply that zero is a centrist position and therefore the mean is the preferred reference point (cf. Lowe et al. 2011: 131). Our measure therefore takes into account what constitutes redistributive policy in a country- and election-specific manner.

[10] For a more detailed review of party families, see Mair and Mudde (1998).

[11] One should note that using the CMP's simple "left-right" measure is misleading, since it carries surplus meaning which is not related to redistribution, such as positions on "traditional morality" (Huber and Stanig 2008).

[12] A small constant (.5) is added to prevent problems with low numbers of quasi sentences. The resulting party measure is insensitive to a range of choices (.1 . . . 1).

[13] Some small, extreme parties are not represented in the data set, since the CMP contains no information on their position. An example is the National Democratic Party (NPD) in Germany, a nationalistic, extreme right party. However, the number of survey respondents that chose those parties is generally negligible.

[14] In a robustness test, we use the country-election median policy position instead, which is more robust when small parties take extreme policy positions.

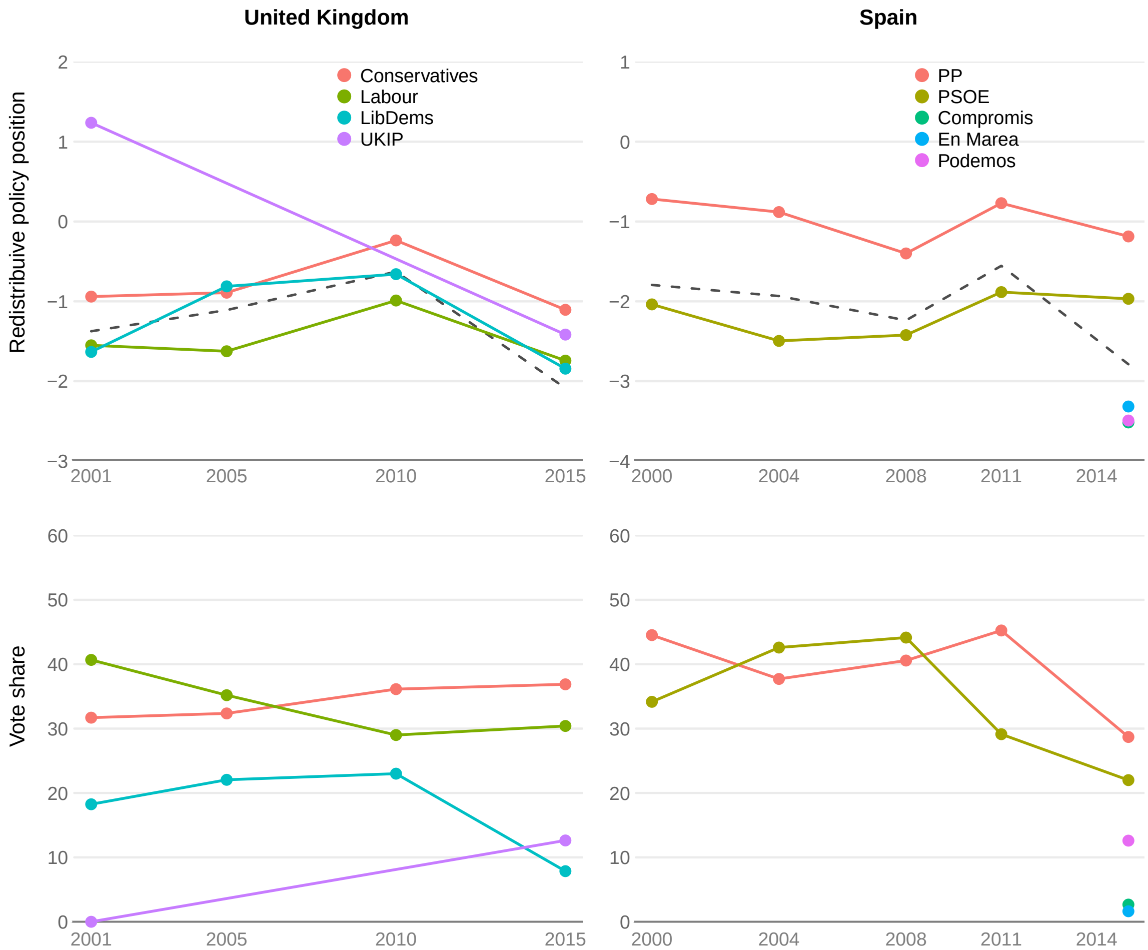

FIGURE I
Policy positions and vote shares of selected parties in the UK and Spain.
The upper panel of this figure shows the position of selected parties on the redistribution dimension, where more negative values implying more preferred redistribution. The dashed line shows the average position of all parties in a given election. The lower panel shows parties' vote shares.

As we will briefly show below, the distinction between 'redistributive' and 'left' parties turns out to be a significant one when we consider the recent success of populist parties. While the commitment of traditional 'left' parties to redistribution has generally been assumed, the preferred economic policies of populist right parties are not particularly clear. In the pioneering work of Kitschelt and McGann (1995), the radical right was considered a fusion of neoliberalism (on the traditional economic dimension) and authoritarianism (on the values/culture dimension). The free-market orientation of the populist right, however, has been questioned (see Ivarsflaten 2005 and De Lange 2007). Mudde (2007) (among others) argues that second dimension issues (ethno-nationalism, opposition to cosmopolitanism and globalization, etc) more than economic policy define populist Right parties and Rovny (2013) shows that these parties often aim to attract voters by blurring their position on the economic dimension. As argued by Afonso and Rennwald (Forthcoming), the redistributive strategies of populist Right parties span "from libertarian to socialist, with different shades of welfare chauvinism in-between."

Figure I illustrates some of the issues outlined above. We have chosen two countries (the UK and Spain) and present both the redistributive positions and the vote percentages of some selected parties.[15] The figure conveys several interesting ideas. First, it illustrates what selected parties are more or less redistributive compared to the country-election mean (indicated by the dashed line). Using our first classification of redistributive parties (the 'left' parties in the socialist/social democratic or communist families), Labour, PSOE and Podemos (but not its related regional parties like En Marea) are considered redistributive throughout the period under analysis. The Figure makes clear, however, that using the second classification (parties occupying more redistributive positions than the country-election mean) both Labour and PSOE are not considered redistributive in the 2015 elections. In Spain, for example, the more redistributive positions of Podemos and related regional parties pull the country-election mean down in the Figure, which makes PSOE propose less redistribution than the (hypothetical) average party in that election. Second, the relatively stable and not particularly redistributive positions of the main 'left' parties (Labour and PSOE) are correlated with decreasing levels of electoral support after the beginning of the Great Recession. In the UK, Labour reaches its highest vote percent during our period of analysis in 2010. In Spain, PSOE does the same in the 2010 election. Third, in the 2015 Spanish election, the more redistributive Podemos-related parties obtain a significant amount of electoral support. In the UK, UKIP becomes much more redistributive from 2001 to 2015, even though it still does not cross the theshold to be considered a redistributive party. In an unsytematic but suggestive manner, UKIP's redistributive switch is associated with a move from negligible voter support in 2001 to more than 12% of votes in 2015.

Preferences Our measure of redistribution preferences in Western Europe is an item commonly used in individual level research on preferences (e.g., Rehm 2009). It elicits a respondent's support for the statement "the government should take measures to reduce differences in income levels" measured on a 5 point agree-disagree scale with labeled answer categories

[15]While the analysis below considers all parties in the CMP data, we use only a sample here to illustrate our points.

("Strongly agree" to "Strongly disagree"). The left part of Figure II shows a histogram for our pooled European sample. We reverse the scale such that higher values represent support for redistribution.[16] It shows that Western Europe is characterized by a rather high level of popular support for redistribution. More than two thirds of ESS respondents either agree or strongly agree with the statement that the government should take measure to reduce income differences. Explicit opposition is much less widespread. In our US analysis, we follow Ashok, Kuziemko, and Washington (2015) and use an item containing the following statement: "Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Others think the government should just let each person get ahead on their own." Respondents are then asked to place themselves on a 7-point scale with labeled end-points, ranging from "Government see to job and good standard of living" to "Government let each person get ahead on their own". The distribution of responses is shown in the right part of Figure II.

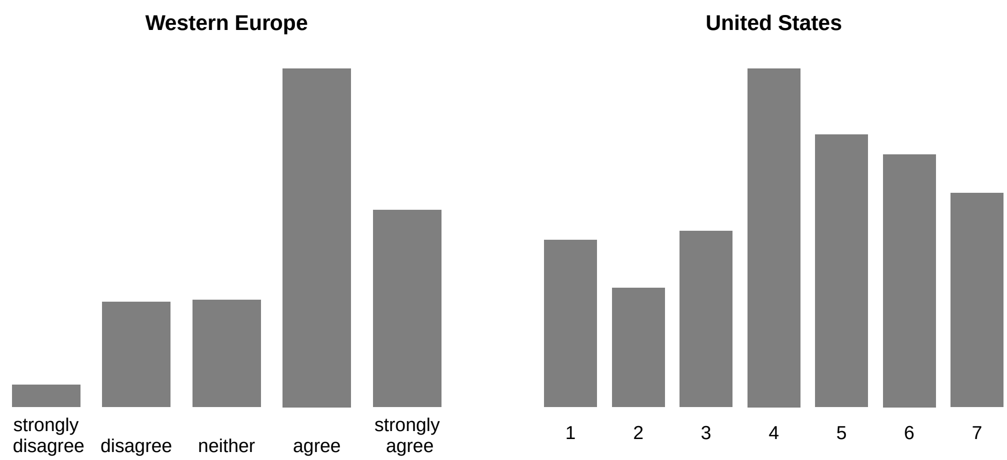

FIGURE II
Distribution of redistribution preferences in Western Europe and United States.

In the theory section, we explained how the parameters in discrete choice voting models are often estimated using distance variables as the predictors. In the American analysis below, we have a measure explicitly capturing the spatial distance between a respondent's position on redistribution (as described above) and the respondent's view of each party's position on the same issue. In the analysis of Western European data, we use redistribution preferences as a predictor of redistributive party voting.[17]

[16]All descriptive results are weighted for survey design characteristics.
[17]We should mention that, for the theoretical reasons outlined in the previous section of this paper, the de-

Income distance Our central measure of an individual's material position is the distance between the income of respondents and the mean income in their country (at the time of the election). In other words, we calculate income distance as a respondent's income minus the country-year income mean.[18]

The ANES captures income using an item asking a respondent to place his or her family's total market income in one of at least 22 income bands with boundaries varying throughout the years. To create a measure of income that closely represents our theoretical concept, income distance, we follow the American Politics literature and transform income bands into their midpoints (e.g., Hout 2004).[19] We impute the open-ended top income category by assuming that the upper tail of the income distribution follows a Pareto distribution (e.g., Kestenbaum 1976, Kopczuk, Saez, and Song 2010). Finally, for each respondent, we calculate the distance between her assigned and the national mean income in a given year.

In our European analysis, we rely on the same strategy. The ESS captures income by asking respondents to place their total net household income into a number of income bins giving yearly, monthly, or weekly figures.[20] We transform these categories into (country-year-specific) mid-points and impute the open-ended top income category from the Pareto distribution. The purchasing power of a certain amount of income varies across the countries included in our analysis. Simply put, it could be argued that the meaning of being Eur 10,000 below the mean is different in Sweden than in the United Kingdom.[21] Thus, for each country and each year, we convert a country's currency into PPP-adjusted constant 2005 US dollars. Finally, we calculate the distance of a respondent's income to the country-year mean.

pendent variable in our analysis needs to be voting choice (and not the actual redistributive position of different parties). We come back to this issue in the robustness section.

[18]This represents a simple centering, which leaves the *distribution* of incomes unchanged. However, it takes into account that mean incomes differ over countries. For example, in 2004, the mean income (after PPP adjustment) in Sweden is 32,721, while in Austria it is 36,122. Note that using untransformed income yields the same pattern of substantive results.

[19]For example, this means that the third income category in 2000 ($10,000 to $14,999) becomes mid-point 12,500, while the third-to-last category ($185,000 to $194,999) becomes 190,000. We conducted a robustness test to show that alternative mid-points do not lead to substantively different results.

[20]The exact question wording is: "Using this card, if you add up the income from all sources, which letter describes your household's total net income? If you don't know the exact figure, please give an estimate. Use the part of the card that you know best: weekly, monthly or annual income.". The wording of this question between 2008 and 2012 is a bit different, but the meaning remains the same. In these surveys, "after tax and compulsory deductions" replaces "net." From 2002 to 2006 the ESS used 12 income bands common to all countries, while starting in 2008 it used 10, based on each country's income deciles.

[21]And more importantly, it could be argued that the bulk of rich or poor people would be concentrated in the wealthiest (or most unequal) countries, therefore distorting our results.

Occupational risk To operationalize a respondent's exposure to occupational unemployment risk we follow the suggestion of Rehm (2005) and use the unemployment rate of his or her occupation. In our Western European analysis, occupational unemployment rates are calculated from Labor Force Surveys and matched to ISCO-1d major occupational groups, while in the US they are matched to DOT occupational titles. For more details, see Rehm (2009, 2011).

Individual characteristics Our models below include a number of individual characteristics to adjust for observable differences between individuals. We refrain from including a large set of variables, since many are arguably post-treatment (to income and occupational risk). We include age (in years), gender (an indicator for female), years of schooling, labor force status, and household size. We explore the impact of other variables in a robustness section.

III.C. Statistical specification

We now describe how we model individuals' vote choices and how they are shaped by (endogenous) redistribution preferences. Let V_{ijt} represent observed vote choice of individual i ($i = 1, \ldots, n_{jt}$) in geographical unit j ($j = 1, \ldots, J$) at time point (survey year) t ($t = 1, \ldots, T$). When analyzing Western Europe, the geographical units are countries, while in the US analysis they are the individual states.

In a decision theoretic formulation, an individual will vote for a party if the utility derived from that choice, V^*_{ijt}, exceeds that of the alternative. In our setting we have a simplified choice set (Redistributive vs. non-redistributive, Democrat vs. Republican), so that we observe $V_{ijt} = 1$ if $V^*_{ijt} > 0$ (and zero otherwise). Our measures of preferences are the categorical survey items described above and denoted by R_{ijt}. For simplicity, we treat them as continuous.[22]

We want to model the role of income distance and occupational risk in shaping preferences and how preferences themselves influence vote choice. Thus we jointly estimate the following two equations:

$$R_{ijt} = \beta_1 w_{ijt} + \beta_2 z_{ijt} + x'_{ijt}\delta^R + \xi_j \lambda_t + \epsilon^R_{ijt} \tag{4}$$

$$V^*_{ijt} = \alpha R_{ijt} + \gamma_1 w_{ijt} + \gamma_2 z_{ijt} + x'_{ijt}\delta^V + \nu \xi_j \lambda_t + \epsilon^V_{ijt} \tag{5}$$

Here, redistribution preferences are a function of income distance, w_{ijt} and occupational

[22]Using a more complex latent variable model for ordinal outcomes does not make a substantive difference to our results.

risk, z_{ijt}, captured by β'. Preferences then enter the vote choice equation, with their effect captured by α. In the Western European case they enter as R_{ijt}, while in our US analysis, they are the relative squared distance between a respondent's preferred policy position and the (perceived) position of each party, $R^*_{ijt} = (R_{ijt} - R^P_{ijt})^2$.

Income distance and occupational risk are also allowed to shape vote choice directly (in addition to their 'indirect' effect via preferences); their role is captured by the two γ coefficients. Both equations also include a vector of individual controls, x_{ijt}, with associated coefficients δ^R and δ^V, respectively.

The reader may have noted that our specification does not include country-level variables. In order to adjust for macro-level confounders, we include both geography-specific constants, ξ_j, and survey-year-specific constants, λ_t. Note, that, in contrast to the commonly used setup, they are not assumed independent but specified as interactive effects (Bai 2009). This has important implications for how to understand the effect of time-specific shocks. In the commonly used two-way specification (e.g., independent time and country effects), time effects are assumed to be common shocks, i.e., they affect all cross-sectional units the same. In contrast, we allow these shocks to be of different magnitude in different geographical units. Our model thus exhaustively adjusts for country- or state-level confounders as well as for election-level confounders (since it uses $J \times T$ degrees of freedom). This is why our model specifications do not include country-level variables.[23] Note that we allow country-level unobservables to affect preferences and vote choice differently by including a scale factor ν in equation (5).[24]

Finally, residuals ϵ_V and ϵ_R are both zero-mean normally distributed. While the variance of ϵ_R is freely estimated, the variance of ϵ_V is fixed to one to identify the probit equation.[25]

With estimates from our joint preference and vote model in hand, we can calculate the direct and indirect (counterfactual) effects specified in equations (2) and (3). Appendix A2 shows how these are derived from our model estimates.

[23] Note that we implement ξ_j and λ_t as the Bayesian version of the classical 'fixed effects' model. We allow covariates to be related to country-/state- and time-specific effects by employing the Chamberlain-Mundlak device (an orthogonal projection of ξ_j, λ_t on covariate group averages, where groups are defined by the cross-section of J and T; cf. Mundlak 1978; Chamberlain 1982). See Rendon (2012) for an extended discussion of fixed and random effects in a Bayesian context.

[24] We can test if effects do indeed differ between preferences and choices by testing if $\nu = 1$. Such a specification is clearly rejected.

[25] We specify Cov(ϵ_R, ϵ_V)=0 conditional on all covariates, preferences, and country-year-specific constants. A model allowing for residual dependence (using a parameter expanded inverse Wishart covariance prior, with scale matrix $\boldsymbol{I}_2$ and 3 df.) yields a negligible covariance of −0.006.

Estimation We estimate our model using MCMC sampling. This allows us to obtain the full posterior distribution of not just the model parameters, but also all derived quantities, such as indirect effects, and sensitivity simulations. We assign uninformative priors to all model parameters.[26] All identification is classical.

IV. Results

IV.A. Western European sample

Table I shows estimates and derived quantities from our model for the European analysis. In panel (A) we show the relevant parameter estimates from equations (4) and (5), omitting other estimates for reasons of space. We divide the results in Table I into two columns, the first one uses the 'left' label definition of voting for a redistributive party and the second one uses the definition of voting for parties that are more redistributive than the country-election mean. The distance of a respondent's income to the country average has the expected negative impact on preferences and is clearly statistically different from zero. The same is true for its coefficient in the vote choice equation. We find similar relationships regarding occupational risk. While individuals belonging to occupational groups with higher unemployment rates have a stronger preference for redistributive policies, however, occupational risk has an insignificant effect on voting (when using either definition of redistributive party). It should be noted that this effect of occupational risk is *net* of income by construction (we orthogonalize both variables). The last parameter of interest in equation (5) is α, the effect of endogenous preferences on the probability of choosing a party proposing redistributive polices (relative to other parties in a particular election). We find clear evidence for a strong link between a respondent's preferences and her party choice. What these results tell us is that (i) both income distance and risk shape preferences, (ii) income distance in turn increase the probability of voting for a party offering redistributive policies, and (iii) that there also is a direct effect of income and risk on vote choice (not due to their effect on preferences).

We now turn to a quantitative assessment of how much income and risk shape vote choice *via* preferences (and by how much they do not), by calculating the quantities de-

[26]More explicitly, we set all regression-type parameters to be *a priori* normally distributed mean zero with a standard deviation of 10. For the free variances in the model we use vague inverse Gamma priors (Spiegelhalter et al. 1997), $IG(0.001, 0.001)$. Given our large sample, the data clearly dominate these prior choices.

TABLE I
Model results for redistributive and left parties. Western European sample.

	Social democratic party		Redistributive party	
(A) Coefficient estimates				
	Preferences	Vote	Preferences	Vote
Income distance	−0.163 (0.006)	−0.036 (0.008)	−0.163 (0.006)	−0.080 (0.008)
Occupational risk	0.079 (0.007)	0.076 (0.010)	0.078 (0.007)	0.070 (0.010)
Preferences		0.165 (0.007)		0.252 (0.007)
(B) Effect decomposition				
	Income	Risk	Income	Risk
Indirect effect	−1.055 (0.057)	0.501 (0.049)	−1.280 (0.081)	0.542 (0.060)
Direct effect	−1.407 (0.316)	2.962 (0.379)	−2.403 (0.284)	1.989 (0.294)
Proportion	0.436 (0.061)	0.146 (0.021)	0.349 (0.025)	0.216 (0.029)
(C) Sensitivity analysis for IE [a]				
	Income	Risk	Income	Risk
$\rho \in \{-0.3, \dots, 0\}$	−0.148 (0.037)	0.063 (0.019)	−0.568 (0.044)	0.252 (0.029)
$\rho \in \{0, \dots, 0.3\}$	−1.499 (0.074)	0.677 (0.065)	−1.204 (0.103)	0.491 (0.061)

Note: Based on 16,000 MCMC samples. N=39,005. All continuous inputs are standardized to have mean zero and unit variance. Categorical inputs are mean zero.
a Sensitivity analysis for mediator-outcome confounding, simulated over 100-point grid $\rho \in \{a, \dots, b\}$. Displayed results are averages over 100 simulations. Based on 5,000 MCMC samples.

scribed in equations (2) and (3). Results are shown in Panel (B) of Table I. Note that the metric of both IE and DE is the difference in probability of voting for a redistributive party. We find that both income and risk significantly shape choices via preferences. Looking at our ‘left’ label classification of redistributive parties, an increase in income distance decreases redistributive party choice via preferences by 1.1 (±0.1) percentage points, while its effect on vote choice that is due to factors other than preferences is 1.4 (±0.3) percentage points. Redistribution preferences therefore account for 44% of the total effect of income that we observe. An increase in occupational unemployment risk increases the probability of voting for a redistributive parties via its effect on redistribution preferences by 0.5 (±0.05) percentage points, accounting for 15% of the total effect. Its corresponding effects not due to redistribution preferences is 3.0 (±0.4) percentage points. When focusing on the definition of voting for parties that are more redistributive than the country-election mean, an increase in income distance decreases redistributive party choice via preferences by 1.3 (±0.1) percentage points, while its effect on vote choice that is due to factors other than preferences is 2.4 (±0.3) percentage points. Redistribution preferences therefore account for 35% of the total effect of income that we observe. An increase in occupational unemployment risk increases the probability of voting for a redistributive parties via its effect on redistribution preferences again by 0.5 (±0.06) percentage points, accounting for 22% of the total effect. Its corresponding effects not due to redistribution preferences is 2.0 (±0.3) percentage points.

The value of explicitly defining indirect effects in a potential outcomes framework is that it makes explicit the assumptions needed to estimate them (Imai, Keele, and Tingley 2010; Imai et al. 2011). These assumptions are unlikely to be completely met in an observational analysis such as ours. Thus, the best available strategy is to assess the robustness of our results by conducting sensitivity analyses (Imai, Keele, and Yamamoto 2010; VanderWeele 2010). In panel (C) of Table I we show results of several sensitivity analyses, where we average over 100 increasingly extreme levels of unobserved confounders that affect both preferences and vote choice. The empirical implication of an unobserved confounder affecting both observed values of the mediator and potential outcomes is a correlation between residuals (Imai, Keele, and Yamamoto 2010: 61); in our context $\rho(\epsilon_1, \epsilon_2)$.[27] Simulating this correlation, we evaluate our indirect effect estimates over a 100-point grid with successively increasing correlation $\rho \in \{-r, \ldots, 0\}$ and $\rho \in \{0, \ldots, r\}$, where the limit r is chosen to represent ranges of possible correlations. We use 5,000 Monte Carlo samples (obtained from the posterior distribution of each parameter) to account for estimation uncertainty. We present both averages of estimated indirect effects (putting equal weights on all possible levels of confounding) as well as posterior standard deviations of the distribution of indirect effect estimates.[28]

Our results in panel (C) show how accounting for possible confounding affects our (average) results. To give an idea of the substantive magnitude of the level of confounding we are simulating: a correlation of 0.3 is about the size of the observed correlation between education and income. When accounting for confounders that induce a negative correlation between preferences and vote choice, our indirect effect estimates are reduced: we find that the effect of income via preferences is now less than one seventh of its previous size, although it is still statistically different from zero. The size of the indirect effect of occupational risk (not due to income) is similarly reduced. It also remains statistically different from zero. In contrast, accounting for confounders that induce a positive correlation

[27] See model equations 4 and 5

[28] Our results should be understood as the average of mediated effects over all levels of possible correlations simultaneously. In contrast, in the appendix we calculate mediated effects for each single value of ρ.

between preferences and choices yields indirect effect estimates that are (even) larger than those obtained assuming a correlation of zero. This is particularly the case for the indirect effect of income, which increases by almost 0.5 percentage points.

IV.B. United States sample

Table II shows estimates and derived quantities from our model for the American analysis. The structure of the results table is the same as in Table I, but in this case there is only one definition of redistributive voting (voting for the Democratic Party). In panel (A), as was the case in the European analysis, we find the distance of a respondent's income to the country average to have the expected negative impact on preferences and on vote choice (both clearly statistically different from zero). Using American data, however, occupational risk is an insignificant determinant not only of voting (as in the European results) but also of redistribution preferences. Note that in this analysis the measure of preferences is explicitly about the distance between a respondent's position on redistribution and the respondent's perception of each party's position on the same issue and that, as in the analysis of European data, the effect of occupational risk is *net* of income by construction. Again, the last parameter of interest in the table is the effect of endogenous preferences on the probability of voting for the Democratic Party. The results tell us that (i) income distance (not risk exposure) shapes preferences, (ii) income distance in turn increase the probability of voting for a party offering redistributive policies, and (iii) that there also is a direct effect of income on vote choice (not due to its effect on preferences).

The quantitative assessment of how much income and risk shape vote choice *via* preferences (and by how much they do not) is remarkably similar to our European results. Panel (B) of Table II shows that income (but not occupational risk) significantly shapes choices via preferences. An increase in income distance decreases redistributive party choice via preferences by 2.3 (±0.4) percentage points, while its effect on vote choice that is due to factors other than preferences is 1.1 (±0.9) percentage points. Redistribution preferences therefore account for over 70% of the total effect of income that we observe.

IV.C. Robustness checks

We conduct a number of robustness checks. In order not to have to display a wealth of specifications, we group some of them. In robustness test (1) we include two variables capturing distinct economic characteristics of a respondent: whether he or she is a member

TABLE II
Model results for Democratic vote. United States sample.

(A) Coefficient estimates				
	Preferences		Vote	
Income distance	−0.112	(0.021)	−0.033	(0.026)
Occupational risk	0.020	(0.020)	−0.001	(0.025)
Preferences			0.616	(0.044)
(B) Effect decomposition				
	Income		Risk	
Indirect effect	−2.312	(0.441)	0.411	(0.404)
Direct effect	−1.093	(0.869)	−0.024	(0.814)
Proportion	0.726	(0.270)	0.304[a]	(1.867)
(C) Sensitivity analysis for IE [b]				
	Income		Risk	
$\rho \in \{0,\dots,0.3\}$	−1.573	(0.308)	0.275	(0.308)
$\rho \in \{-0.3,\dots,0\}$	−2.451	(0.453)	0.431	(0.453)

Note: Based on 16,000 MCMC samples. N=5,260. All continuous inputs are standardized to have mean zero and unit variance. Categorical inputs are mean zero.
a Entry is the median of the posterior distribution (which is highly non-normal). Note that the proportion mediated is not very meaningful in this instance.
b Sensitivity analysis for mediator-outcome confounding, simulated over 100-point grid with $\rho \in \{a,\dots,b\}$. Displayed results are averages over 100 simulations. Based on 5,000 MCMC samples.

of a trade union, and whether he is currently unemployed. Test (2) is designed to capture socio-cultural characteristics: religiosity and distinct preferences of individuals living in high-density, urban areas (see, for example, Cho, Gimpel, and Dyck 2006).[29] We include indicator variables for the two dominant religious groups in Western Europe as well as a variable capturing the frequency with which a respondent attends religious services.[30] We also include an indicator equal to one if the respondent lives in a major city or its outskirts and suburbs.

Save for one exception, the first two specifications produce rather similar results. Adjusting for union membership and unemployment, as well as religion and urban density,

[29] As argued by Rodden (2010: 322), it is clear that individuals sort themselves into neighborhoods with similar demographic, occupational, income, and ultimately political preferences. Since it has direct effect on both preferences and choices, urban location is therefore not included in our main model.

[30] Note that, as has been argued by Stegmueller (2013), religion affects *economic* preferences and choices. We include it here to capture possible non-economic ("second dimension") considerations.

TABLE III
Robustness checks. Indirect effects with posterior standard deviation in parentheses. Proportion mediated in brackets.

	Western Europe		United States	
	Income	Risk	Income	Risk
Economic variables[a]	−0.978	0.439	−2.274	−1.339
	(0.056)	(0.047)	(0.443)	(0.546)
	[0.412]	[0.139]	[0.708]	[0.958]
Cultural variables[b]	−0.962	0.425	−2.239	0.349
	(0.057)	(0.048)	(0.436)	(0.392)
	[0.389]	[0.140]	[0.676]	[0.230]
Omitted variable bias[c]	−0.364	0.102	−0.498	0.170
	(0.022)	(0.010)	(0.105)	(0.138)
	[0.366]	[0.154]	[0.645]	[0.400]

Note: Western Europe LHS variable is social democratic party vote. Based on 16,000 MCMC samples.

a Indicators for unemployment, union membership, limited contract (WE only).

b Religion (Catholic, Protestant, else) and frequency of church attendance. Indicator for living in urban area.

c Includes control set $\hat{\mathscr{Q}}$ obtained using double selection (Belloni, Chernozhukov, and Hansen 2013) on a predictive system of treatment and outcome equations. See appendix A3.

leads to an almost unchanged indirect effect of income distance on vote choice channeled via redistribution preferences. This holds for both Western Europe (where the dependent variable is social democratic party choice) and the United States. The role of occupational unemployment risk in Western Europe is similarly unchanged. However, when looking at indirect effect estimates for risk in the US, we a significant negative estimate in the first specification (which includes a respondent's current unemployment). In the second specification the sign of the indirect effect is reversed and indistinguishable from zero.

The instability of these results can be taken to suggest that unmodelled confounders affect our results, particularly in the case of risk in the US. In specification (3) we try to limit the scope of omitted variable bias (Chernozhukov, Hansen, and Spindler 2015). To do so we allow for a flexible, possibly highly non-linear and multiplicative, form of control for confounding. We employ a double post-selection strategy (Belloni, Chernozhukov, and Hansen 2013; Belloni et al. 2017) and first construct a high-dimensional vector of controls by allowing functional transforms of observables and their higher order interactions (leading to almost 300 covariate terms). We then select from these controls those that influence vote choice as well as income and occupational risk using the LASSO. For more details see

Appendix A3.

Three key findings emerge from this exercise. First, we find the indirect effect estimate of income distance to be reduced by about 0.7 percentage points in Europe and more than 1.8 percentage points in the US. In both cases they are still clearly different from zero. Furthermore, since the direct effect of income on vote choice is also impacted, the proportion of the total effect of income on voting due to preferences sees far less change. It is reduced by 6 percentage points in Europe and 7 percentage points in the United States. Second, in our Western European sample, the indirect effect estimate of risk decreases as well, but its proportion of the total effect remains virtually unchanged. Third, in the US sample, the indirect effect of risk is substantially reduced and it is not statistically different from zero. In all cases, these results, while different in quantitative magnitude, mirror the findings we obtained in our specification reported in Tables I and II.

V. Conclusion

After APSA feedback...

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Appendices

A1. Descriptive information

Table A1.1
Countries and election included in Western European analysis.

	Survey years						Survey dates	Election dates
	2	4	6	8	10	12		
AT	**x**	x	**x**				02.02.03-30.09.03; 06.01.05-25.04.05; 18.07.07-05.11.07	24.11.02, 1.10.06
BE	**x**	**x**	x	**x**	**x**	x	01.10.02-30.04.03; 04.10.04-31.01.05; 23.10.06-19.02.07; 13.11.08-20.03.09; 11.10.10-06.05.11; 10.09.12-24.12.12	13.6.99, 18.5.03, 10.6.07, 13.06.10, 25.05.14
CH	**x**	**x**	x	**x**	x	**x**	09.09.02-08.02.03; 15.09.04-28.02.05; 24.08.06-02.04.07; 30.08.08-17.04.09; 02.10.10-23.03.11; 01.09.12-22.04.13	24.10.99, 19.10.03, 21.10.07, 23.10.11
DE	**x**	x	**x**	x	**x**	x	20.11.02-16.05.03; 26.08.04-16.01.05; 01.09.06-15.01.07; 27.08.08-31.01.09; 15.09.10-03.02.11; 06.09.12-22.01.13	27.9.98, 22.9.02, 18.9.05, 27.09.09, 22.09.13
DK	**x**	x	**x**	**x**	x	**x**	28.10.02-19.06.03; 20.11.02-16.05.03; 19.09.06-02.05.07; 01.09.08-11.01.09; 20.09.10-31.01.11; 10.01.13-24.04.13	20.11.01, 8.2.05, 13.11.07, 15.09.11
ES	**x**	**x**	x	**x**	x	**x**	19.11.02-20.02.03; 27.09.04-31.01.05; 25.10.06-04.03.07; 05.09.08-31.01.09; 11.04.11-24.07.11; 23.01.13-14.05.13	12.3.00, 14.3.04, 9.3.08, 20.11.11
FI	**x**	**x**	x	**x**	x	**x**	09.09.02-10.12.02; 20.09.04-17.12.04; 18.09.06-20.12.06; 19.09.08-05.02.09; 13.09.10-30.12.10; 03.09.12-02.02.13	21.3.99, 16.3.03, 18.3.07, 17.04.11
GB	**x**	x	**x**	x	**x**	x	24.09.02-04.02.03; 27.09.04-16.03.05; 05.09.06-14.01.07; 01.09.08-19.01.09; 31.08.10-28.02.11; 01.09.12-07.02.13	7.6.01, 5.5.05, 6.5.10
IE	**x**	x	x		**x**	x	11.12.02-12.04.03; 18.01.05-20.06.05; 14.09.06-31.08.07; 20.09.11-31.01.12; 15.10.12-09.02.13	6.6.97, 17.5.02, 24.5.07, 25.02.11
IT	**x**					**x**	13.01.03-30.06.03; 01.06.13-20.12.13	13.5.01, 9.04.06, 13.04.08, 24.02.13
NL	**x**	**x**	x	**x**	**x**	x	01.09.02-24.02.03; 11.09.04-19.02.05; 16.09.06-18.03.07; 08.09.08-28.06.09; 27.09.10-02.04.11; 28.08.12-30.03.13	6.5.98, 15.5.02, 22.1.03, 22.11.06, 9.06.10, 12.9.12
NO	**x**	x	**x**	x	**x**	x	16.09.02-17.01.03; 15.09.04-15.01.05; 21.08.06-19.12.06; 25.08.08-20.01.09; 09.09.10-15.02.11; 14.08.12-08.02.13	10.9.01, 12.9.05, 13.9.09, 8.9.13
PT	**x**	x	**x**	x	**x**	**x**	26.09.02-20.01.03; 15.10.04-17.03.05; 12.10.06-28.02.07; 09.10.08-08.03.09; 11.10.10-23.03.11; 24.10.12-20.03.13	10.10.99, 17.3.02, 20.2.05, 27.9.09, 5.6.11
SE	**x**	x	**x**	x	**x**	x	23.09.02-20.12.02; 29.09.04-19.01.05; 21.09.06-03.02.07, 15.09.08-03.02.09; 27.09.10-01.03.11; 01.10.12-05.05.13	20.9.98, 15.9.02, 17.9.06, 19.9.10

A2. Calculation of direct and indirect effects

This section describes how we calculate direct and total indirect effects (Robins 2003) of equations (3) and (2) from our model estimates obtained from equations (4) and (5).[31] Write our model in simplified form with one covariate of interest (treatment), x_i, a mediating variable (preferences), m_i, and confounders, c_i. We estimate the following system of equations:

$$\text{probit}(y_i) = \beta_0 + \beta_1 x_i + \lambda m_i + \beta_2 c_i \tag{A2.1}$$

$$m_i = \gamma_0 + \gamma_1 x_i + \gamma_2 c_i + \epsilon_{2i}. \tag{A2.2}$$

with

$$\epsilon_{2i} \sim N(0, \sigma^2_{\epsilon_2}) \tag{A2.3}$$

Since our dependent variable is binary, probit(y_i) is the probability of obtaining a positive response (voting Democrat), defined as

$$P(Y_i = 1|m, x, c) = \int_{-\infty}^{\text{probit}(y_i)} f(z; 0, 1)\partial z = \Phi(\text{probit}(y_i)) \tag{A2.4}$$

where $f(z; 0, 1)$ is the standard normal density, and Φ is the CDF of the standard normal distribution.

Take the general expression used in the formulas for direct and indirect effects (eq. (3) and (2)), $E(Y(x, M(x'))|C = c)$. As these quantities are not expressed conditional on M,

[31] Imai, Keele, and Tingley (2010); Imai et al. (2011) call these average causal mediated effects for the treated and average direct effects for the control, while Pearl (2001) calls them total natural indirect effects and pure natural direct effects. See Imai, Keele, and Tingley 2010 and Muthen and Asparouhov 2014 for an extended discussion on their computation.

we need to integrate over M:[32]

$$E(Y(x,M(x'))|C=c) = \int_{-\infty}^{\infty} E(Y|C=c,X=x,M=m) \times f(M|C=c,X=x')\partial M \quad \text{(A2.5)}$$

$$= \int_{-\infty}^{\infty}\int_{-\infty}^{\text{probit}(y_i)} f(z;0,1)\partial z \times f(M;\gamma_0+\gamma_1 x'+\gamma_2 c, \sigma^2_{\epsilon_2})\partial M \quad \text{(A2.6)}$$

$$= \int_{-\infty}^{\text{probit}(x,x')} f(z;0,1)\partial z. \quad \text{(A2.7)}$$

Here, probit(x,x') is given by:

$$\text{probit}(x,x') = [\beta_0+\beta_1 x+\beta_2 c+\lambda(\gamma_0+\gamma_1 x'+\gamma_2 c)]/\sqrt{var(x)} \quad \text{(A2.8)}$$

where the variance $var(x)$ is given by

$$var(x) = \lambda^2\sigma^2_{\epsilon_2}+1. \quad \text{(A2.9)}$$

Indirect effect Denote two values of a treatment by x and x' (e.g., low vs. high income). The indirect effect (eq. 2) is :

$$E[(Y(x',M(x'))-Y(x',M(x))|C] = \quad \text{(A2.10)}$$

$$\int_{-\infty}^{\infty} E[Y|C=c,X=x',M=m]\times f(M|C=c,X=x')\partial M \quad \text{(A2.11)}$$

$$-\int_{-\infty}^{\infty} E[Y|C=c,X=x',M=m]\times f(M|C=c,X=x)\partial M. \quad \text{(A2.12)}$$

Expressed in terms of equation A2.4 the indirect effect is calculated as:

$$\Phi(\text{probit}(x',x')) - \Phi(\text{probit}(x',x)) \quad \text{(A2.13)}$$

This is equivalent to the formula given in Imai, Keele, and Tingley 2010, appendix F.

[32]The last equality is obtained by variable transformation and a change of order of integration.

Direct effect The direct effect (eq. 3) is

$$E[Y(x', M(x)) - Y(x, M(x))|C] = \tag{A2.14}$$

$$\int_{-\infty}^{\infty} (E[Y|C=c, X=x', M=m] - E[Y|C=c, X=x, M=m]) \times f(M|C=c, X=x)\partial M. \tag{A2.15}$$

Expressed in terms of equation A2.4 it is calculated as:

$$\Phi(\text{probit}(x', x)) - \Phi(\text{probit}(x, x)). \tag{A2.16}$$

Substantive interpretation of these quantities rests on a number of assumptions. We discuss these and conduct sensitivity analyses (Imai, Keele, and Tingley 2010; Imai, Keele, and Yamamoto 2010).

A3. Double-post-selection LASSO estimation

To relax our modeling assumptions, we report robustness test (3) in Table III that builds on the double-post-selection strategy proposed by Belloni et al. (Belloni, Chernozhukov, and Hansen 2013; Belloni et al. 2017). Specifically, this model setup aims to reduce the possible impact of omitted variable bias by accounting for a large number of confounders in the most flexible way possible. This can be achieved by moving beyond restricting confounders to be linear and additive, and instead considering a flexible, unrestricted (non-parametric) function. This leads to the formulation of the following partially linear model (we omit FEs and subscripts for grouping structures for notational parsimony)

$$V_i^* = \alpha R_i + \gamma D_i + g(x_i) + e_i, \quad E(e_i|D_i, x_i = 0) \tag{A3.1}$$

Here, V_i^* is the vote propensity of each respondent and $D_i = \{w_i, z_i\}$ are the “treatments” income and occupational risk. The function $g(x_i)$ captures the possibly high-dimensional and nonlinear influence of confounders. The utility of this specification as a robustness test stems from the fact that it imposes no a priori restriction on the functional form of confounding variables. A second key ingredient in a model capturing biases due to omitted variables is the relationship between the treatment(s) and confounders. Therefore, we consider the following auxiliary treatment equations

$$D_i = m(x_i) + u_i, \quad E(u_i|x_i = 0) \tag{A3.2}$$

which relates treatment to a set of covariates x_i. The function $m(x_i)$ summarizes the confounding effect and creates omitted variable bias.

The next step is to create approximations to both $g(\cdot)$ and $m(\cdot)$ by including a large number (p) of control terms $q_i = P(x_i) \in \mathbb{R}^p$. These control terms can be transforms of covariates, higher order interaction terms, etc. Even with an initially limited set of variables, the number of control terms can grow large, say $p > 200$. To limit the number of estimated coefficients, we assume that g and m are approximately sparse (Belloni, Chernozhukov, and Hansen 2013) and can be modeled using s non-zero coefficients (with $s \ll p$) selected using regularization techniques, such as the LASSO (see Tibshirani 1996; see Ratkovic and Tingley 2017 for a recent exposition in a political science context):

$$V_i^* = \alpha R_i + \gamma D_i + q_i \kappa_{g0} + r_{gi} + e_i \tag{A3.3}$$

$$D_i = q_i \kappa_{m0} + r_{mi} + u_i \tag{A3.4}$$

Here, κ_{g0}, and κ_{m0} are coefficient vectors for the selected covariates and r_{gi} and r_{mi} are approximation errors.

However, before proceeding we need to consider the problem that variable selection techniques, such as the LASSO, are intended for prediction, not inference. In fact, a "naive" application of variable selection, where one keeps only the significant q variables in equation (A3.3) fails. It relies on perfect model selection and can lead to biased inferences and misleading confidence intervals (see Leeb and Pötscher 2008). Thus, we express our problem as one of prediction by substituting the auxiliary treatment equation (A3.4) for D_i in equation (A3.3) yielding a reduced form equation so that now *both* equations in this system are amenable to high-dimensional selection techniques.

Note that using this two equation setup is also necessary to guard against variable selection errors. To see this, consider the consequence of applying variable selection techniques to the vote equation only. In trying to predict V with q, an algorithm (such as LASSO) will favor variables with large coefficients but will ignore those of intermediate impact. However, omitted variables that are strongly related to one or both of the treatments can lead to large omitted variable bias in the estimate of γ even when the size of their coefficient in the outcome equation is moderate. The Post-double selection estimator suggested by

Belloni, Chernozhukov, and Hansen (2013) addresses this problem, by basing selection on *both* reduced form equations. Let $\hat{\mathcal{Q}}_1$ be the set of controls selected by LASSO of V_i on q_i; and let $\hat{\mathcal{Q}}_2$ be the set of controls selected by LASSO of D_i on u_i. Then, the set of control variables, $\hat{\mathcal{Q}}$, used in our analysis reported in specification (3) of Table III is constructed by $\hat{\mathcal{Q}} = \hat{\mathcal{Q}}_1 \cup \hat{\mathcal{Q}}_2$. Note that this strategy is robust to moderate selection mistakes. (Belloni, Chernozhukov, and Hansen 2014).[33]

Responsible for the usefulness of this robustness check is the indirect LASSO step selecting the D-control set. It finds controls whose omission leads to "large" omitted variable bias and includes them in the model. Any variables that are not included ("omitted") are therefore at most mildly associated to D_i and V_i^*, which decidedly limits the scope of omitted variable bias (Chernozhukov, Hansen, and Spindler 2015).

[33]For a very general discussion see Belloni et al. (2017).