The Externalities of Inequality: Fear of Crime and Preferences for Redistribution in Western Europe.*

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Abstract

Why is the difference in redistribution preferences between the rich and the poor high in some countries and low in others? In this paper we argue that it has a lot to do with the rich and very little to do with the poor. We contend that while there is a general relative income effect on redistribution preferences, the preferences of the rich are highly dependent on the macro-level of inequality. The reason for this effect is not related to immediate tax and transfer considerations but to a negative externality of inequality: crime. We will show that the rich in more unequal regions in Western Europe are more supportive of redistribution than the rich in more equal regions because of their concern with crime. In making these distinctions between the poor and the rich, the arguments in this paper challenge some influential approaches to the politics of inequality.

I. INTRODUCTION

The relationship between income inequality and redistribution preferences is a hotly contested topic in the literature on the comparative political economy of industrialized democracies. While some authors maintain that the poor have higher redistribution preferences than the rich (Finseraas 2009; Shayo 2009; Page and Jacobs 2009), others argue that there may not be a negative association between income and redistribution (Moene and Wallerstein 2001; Fehr and Schmidt 2006; Alesina and Glaeser 2004: 57-60).

If we were to look at the preferences of rich and poor in different Western European regions, as we do below, we would observe very significant differences in how apart the rich are from the poor regarding their favored levels of redistribution. These important differences in support for redistribution have received little attention in the existing scholarship and yet they are a most significant element in explanations of outcomes as diverse (and as important) as the generosity of the welfare state, political polarization, varieties of capitalism, etc.

In this paper we show that even after accounting for material self-interest, there is still a great degree of variation in redistribution preferences. We argue that this variation has to do with the preferences of the rich (and not those of the poor) and that they can be explained by taking into account the negative externalities of inequality, namely the relationship between macro inequality and crime. Using comparative survey data, we present a set of empirical tests that support our hypotheses (and provide limited evidence in favor of alternative explanations).

The arguments in this paper challenge some influential approaches to the politics of inequality. These range from those contending that second-dimension issues (particularly cultural and social ones) outweigh economic ones to those emphasizing insurance concerns, social affinity or prospects of upward mobility. We will elaborate on our differences from these approaches in the pages that follow.

II. THE ARGUMENT

This paper's theoretical argument makes three distinct points about the formation of preferences for redistribution. The first one relates to the idea that the level of redistribution preferred by a given individual is fundamentally a function of current income. The second point distinguishes between current tax and transfer considerations and externality-related motivations, and maintains these motivations are long

term and low stakes. As such, they matter most to the rich. We will argue that, if we accept that the influence of current tax and transfer considerations is sufficiently captured by the micro-effect of relative income, macro-levels of inequality will matter to the rich – and only to the rich – because of negative externality reasons. Our third point proposes that the macro-effect of inequality can be explained by different micro-factors and contends that the most important of these is concern for crime, as a most visible negative externality of inequality.

A. Current tax and transfer considerations

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 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. The consensus in the comparative literature on this topic, however, seems to be that there is either no association between market income inequality and redistribution or, contrary to the prediction of the RMR model, less market inequality is associated with more redistribution (Lindert 1996; Moene and Wallerstein 2001; Iversen and Soskice 2009; Alesina and Glaeser 2004; Gouveia and Masia 1998; Rodrigiuez 1999: 57-60).

These findings must be considered with a degree of caution. This is because most of this literature relies on macro-comparative empirical analyses (with redistribution as the dependent variable) and does not pay much attention to individual preferences.[4] When looking at individual data, in fact, there is some support for the argument that relative income influences preferences. Using comparative data, a relative income effect is found in, among others, Bean and Papadakis (1998), Finseraas (2009), and

[4]Even the macro-comparative conclusion is less unambiguous than the consensus in the literature suggests. Milanovic (2000) and Kenworthy and Pontusson (2005) show that rising inequality tends to be consistently associated with more redistribution within countries.

Shayo (2009). Using American data, Gilens (2005), McCarty et al. (2008), and Page and Jacobs (2009) (again, among others) find similar effects.

It is important to point out that we emphasize that income should affect preferences for redistribution across the entire income distribution. We argue that the intensity of redistribution preferences increases with distance from the mean, i.e., an individual in, say, the 10th percentile of the income distribution benefits more from the RMR redistributive scheme (lump-sum payments financed by a linear income tax) than an individual in the 30th percentile. As a result, we expect the former individual to have stronger preferences for redistribution than the latter. Note, that in this paper we follow most of the current literature and define redistribution as taxes and transfers and income as present-day income.[5]

B. Externality-related motivations

The possibility that motivations unrelated to current tax and transfer may influence redistribution preferences has received increasing amounts of attention in the recent political economy literature. As we will document below, support for redistribution is widespread in Western Europe and extends into income groups whose support for redistribution could not possibly be motivated by short-term tax and transfer maximization alone. We will also show that while support for redistribution by the poor is quite constant, support by the rich is shaped by different macro-levels of inequality. In the section below, we will explain in more detail the reasons why crime is a significant externality of inequality but we start now by clarifying the relationship between current tax/transfer considerations and concerns for the negative externalities associated with inequality.

As in the Meltzer-Richard model, our argument implies that a rise in inequality that increases the distance between an individual's income and the mean will change

[5] In other words we exclude arguments based on intertemporal perspectives. 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" (2011: 93). Because of the potential to define economic material self-interest inter-temporally (as lifetime consumption/income), this approach opens the door to arguments about social insurance and risk (Moene and Wallerstein 2003; Rehm 2009; Iversen and Soskice 2001; Mares 2003) and about social mobility and life-cycle profiles (Alesina and Giuliano 2011; Benabou and Ok 2001; Haider and Solon 2006). We will explore some of the implications of defining economic self-interest inter-temporally in the empirical analysis below (as robustness checks for our findings), but our theoretical starting point is that current tax and transfer considerations are captured by relative income (the difference between an individual's present income and the mean in her country).

her distribution preferences. More importantly, our argument also implies that the current pocketbook consequences of inequality are fully contained in the individual income distance shifts produced by this inequality rise. In other words, the tax and transfer consequences of inequality are picked up by individual income changes.

Macro levels of inequality, however, can indirectly affect the individual utility function implicit in the previous paragraph. Following Alesina and Giuliano (2011), we can think about this utility function as one in which individuals care not only about their current tax and transfers but also about some macro measure of income distribution.[6] If macro inequality produces economic externalities, we would expect individual preferences to be affected. Of consequence to this paper's argument, this model allows for even the rich to be negatively affected by macro inequality and, therefore, for them to support redistribution for purely self-interested reasons.

We are not the first authors to recognize the externalities of inequality as a specific case of a more general model of support for redistribution with macro inequality concerns as well as individual tax and transfer considerations.[7] Perhaps the clearest example is the literature on externalities of education, which connects average levels of education with aggregate levels of productivity (see, for example, Nelson and Phelps 1966, Romer 1990 and Perotti 1996). This framework proposes that, with imperfect credit markets, more inequality means more people below an income level that would allow them to acquire education. The rich, in this case, would support redistribution because of the benefits of a higher education average. But, to our knowledge, we are the first to emphasize crime as the key explanatory factor behind the affluent's support for redistribution.

The paragraphs above suggest that both current tax and transfer and externality considerations matter to redistribution preferences. To integrate the arguments about these two distinct dimensions, however, we will argue that a hierarchy of preferences exists. We propose that poor people value redistribution for its immediate tax and transfer consequences. The redistributive preferences of the rich, on the other hand,

[6] As suggester by Alesina and Giuliano (2011), different individuals may be affected by different kinds of inequality. For simplicity, in this paper we focus on the Gini coefficient, which is the most commonly used measure of inequality in the political economy literature.

[7] The literature in economics and political economy has identified a number of other externalities. If we assume the poor to be less educated, a less effective democracy has been considered a negative externality of inequality by authors like Milton Friedman (1982). There is also some research connecting inequality and environmental degradation (Boyce 1994). And see Beramendi (2012) for an analysis of the externalities of regional inequality.

are less significantly affected by current tax and transfer considerations. For the rich, the negative externalities of inequality can become more relevant.

We conceive of the solution to the negative externalities of inequality as both time-horizon and stakes related. The possibility that the poor have shorter-term motivations than the rich has been explored in the economics and sociology literature before. In economics, the poor have been argued to be more constrained in their investment decisions than the rich (explaining the lower likelihood by the poor to invest in long-term objectives like increasing human capital or saving for retirement).[8] Complementarily, sociological research has illustrated that lower social class (itself closely related to low income) leads to shorter time horizons (see, for example, O'Rand and Ellis 1974). It is also reasonable to argue that the relative importance of receiving benefits is greater for the poor than the relative importance of paying taxes is for the rich. This difference can be illustrated as follows. From 2001 to 2005, the relative size of benefits (including public pensions) for households in the bottom decile of the distribution represented 71.7% of household disposable income in Western European countries.[9] For households in the top decile of the distribution, on the other hand, market income was reduced by just 27.7% after subtracting taxes.[10] We expect that, as the stakes of redistribution decline, longer-term considerations related to inequality and crime will increase. We therefore argue in this paper that longer time horizons and lower stakes (in relation to current tax and transfer considerations) mean that the negative externalities of inequality will be more important to the rich.

The implications of this paper's argument are summarized in Figure 1. We expect the negative externalities of inequality to be associated with less support for redistribution. Since we argue that for the poor externality concerns are trumped by current tax and transfer incentives, redistribution preferences converge regardless of the macro-level of inequality as income declines. Thus, the redistribution preferences of an individual with low income v_i in a low inequality region w_j, denoted $R(v_i, w_j)$, and in a high inequality region $R(v_i, w'_j)$ do not differ by much. In contrast, we expect more macro inequality to promote concerns for its negative externalities for the rich, so that redistribution preferences of a rich individual in a low inequality region $R(v'_i, w_j)$ differ starkly from those in high inequality regions $R(v'_i, w'_j)$.

[8] See, for example, Lawrance (1991) or Dynan et al. (2004).

[9] Even in Greece, where this component is the lowest, it amounted to a 44% of disposable income. Authors' calculations based on EUROMOD tax simulation data from Paulus et al. 2009, Appendix A, Table 2.

[10] Authors' calculations based on EUROMOD tax simulation data from Paulus et al. 2009, Appendix A, Table 3.

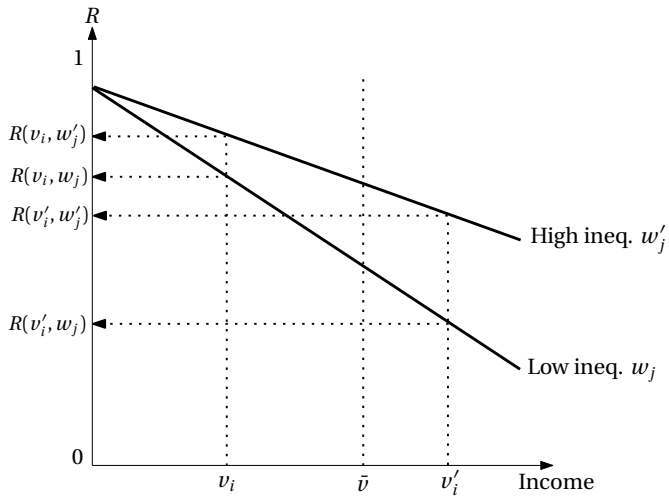

Figure 1: Macro Inequality and Support for Redistribution

C. Macro inequality and fear of crime

We will show below that the association between macro inequality and redistribution preferences summarized in Figure 1 is supported by the empirical evidence and extraordinarily robust. We argue that the effect of macro inequality is channeled by a number of different factors. The most important of this, as mentioned above, is crime, as a most visible negative externality of inequality.

The canonical model for the political economy of crime and inequality was originally developed by Becker (1968) and first explored empirically by Ehrlich (1973). The basic argument is simple (see also Bourguignon 1999 and Sala-i Martin 1996). Assume that society is divided into three classes (the poor, the middle and the rich) with increasing levels of wealth. Assume further that crime pays a benefit, that there is a probability that crime will result in sanction/punishment and that the proportion of "honest" individuals (people who would not consider crime as an option regardless of its economic benefits) is independent of the level of income (and distributed uniformly across classes). It follows from this straightforward framework that rich people for whom the benefit of crime is small in proportion to their initial wealth will very rarely find crime attractive. It also follows that there will always be a proportion of people among the poor who will engage in crime, and that the benefits from crime are proportional to the wealth of the population. The crime rate implied by this simple model would be positively correlated to the extent of poverty and inequality and negatively

correlated to the probability of being caught, the cost of the sanction/punishment, and the proportion of "honest" individuals.[11]

Following this framework, the intuition that crime is related to inequality is easy to understand. With more inequality, the potential gain for the poor from engaging in crime is higher and the opportunity cost is lower. Some early empirical analyses supported this intuition (Ehrlich 1973; Freeman 1983),[12] but the evidence is not unambiguous. However, while we have described above the relationship between inequality and objective levels of crime, it is fear of crime by the affluent that matters most to our argument. We do understand that, as shown by a well-established sociological literature, fear of crime does not exactly reflect the objective possibility of victimization. As early as 1979, DuBow et al. showed that crime rates reflect victimization of the poor (more than the rich) and that fear levels for particular age-sex groups are inversely related to their victimization (elderly women having the lowest victimization rates but the highest fear of crime, young men having the opposite combination). While we do model explicitly the determinants of fear of crime in the empirical analysis we develop below (and show that macro inequality is a significant one), we are not interested in them *per se*.[13] Our argument simply requires rich individuals to believe that there is a connection between macro inequality and crime (following the intuitive logic of the Becker model summarized above). This connection makes sense even if the affluent have concerns for crime that are disproportionately high given their objective probability of victimization.[14]

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[11]Note that there is an implicit temporal side to this economic approach to crime, it involves the probability of being caught (in the future) for a crime being committed or not (in the present). Arguably, "the core message of the economic model of criminal behavior is that it can be discouraged by raising its expected 'price'" (Lee and McCrary 2005: 1). This, in turn, makes the importance of the price of crime crucially depend on how much potential offenders discount their future welfare. For an explicit temporal model, see, for example, Davis (1988).

[12]More recently, Fajnzylber et al. (2002b) use panel data for more than 37 industrialized and non-industrialized countries from the early 1970s until the mid-1990s to explore the relationship between inequality and violent crime. They find crime rates and inequality to be positively correlated within countries and, particularly, between countries. See also Mehlum et al. (2005) for cross country evidence.

[13]We consider fear of crime the equivalent of a *subjective* assessment of victimization. The higher the fear of crime, the more likely an individual will be to consider himself/herself a potential crime victim. This assessment, we argue, is correlated to macro inequality. The higher the levels of macro inequality, therefore, the more likely the individual considers himself/herself to be a potential crime victim. But this is still a probabilistic assessment which will be less certain/relevant than concerns about current income.

[14]The individuals who are most likely to consider committing crime, i.e., the poor, are generally thought to have very high discount rates (Wilson and Herrnstein 1985; Katz et al. 2003). Since the

To anticipate some of our empirical choices below, two additional observations are needed about our argument that macro inequality influences individual concerns about crime as a negative externality. The first one is about the level of macro inequality. Our theoretical argument proposes that the importance of inequality emerges from its relationship to crime as a negative externality. This implies that the relevant level of macro inequality should be one at which a visible connection to crime could be made by individuals. We therefore move away from national data and use regional levels of inequality in the analysis below. Unlike more aggregate levels, regional inequality is both visible and proximate enough to plausibly be related to fear of crime by rich individuals. While it would be good to use even more disaggregated units (like neighborhoods, as in some crime research) the availability of the data at our disposal limits what we can do.

Our argument also implies that rich individuals who are concerned about crime (because they live in unequal areas) are more likely to support redistribution. We assume the affluent's concern for crime to be causally connected to macro inequality, and higher redistribution to be perceived as one of the solutions to the problem. It is clear that other solutions are possible. Most importantly, the affluent may demand protection as a solution to crime (rather than redistribution as a solution to its cause). Recall that objective crime rates in Becker's model is negatively correlated to the probability of being caught and the cost of the sanction/punishment.[15] While we

likelihood of committing crime is not the focus of our analysis (the determinants of concern for crime are, and particularly the role of inequality), we do not address the price of criminal behavior. But the high discount of future welfare by the poor (who are more likely to consider committing crime in this economic framework), is indeed the basis of our theoretical argument. We adopt the concern for future welfare from this political economy approach to crime but, as suggested above, change the focus to the likelihood that an individual considers himself/herself to be a potential crime victim.

[15] As argued by Alesina and Giuliano (2011), the implicit assumption in the kind of argument made in this paper is that it should cost less to the rich to redistribute than to increase spending on security (i.e., policing, incarceration, etc). This is not an unreasonable assumption. Perhaps the topic of incarceration in the US, since it is the focus of a large literature, is the best illustration. The cost of incarceration is high. In his widely cited 1996 paper, Freeman calculated that crime control activities cost 2 percent of GDP. Also, incarceration costs often crowd out spending on social policy (for a state comparison within the US, see Ellwood and Guetzkow 2009). And, while in the short run incarceration reduces unemployment (and the costs of unemployment benefits or active labor market policy), in the long run the costs increase substantially as ex-inmates find themselves in need of public assistance and are often confined to casual or illegitimate employment (e.g., Western 2006). More explicitly, Donohue and Siegleman (1998) find that diverting resources from incarceration and directing the savings to successful social policy (like preschool interventions) would reduce crime without increasing spending in the US.

recognize this as an important issue, we do not consider demands for protection to be incompatible with preferences for redistribution. A number of issues make the comparative costs and benefits of these policies difficult to quantify. They include the implications of these policies in terms of investment in human capital, the encouragement of individual behaviors with positive externalities, the discouragement of behaviors with negative externalities, the spillover from one domain to another (such as education and health investments that affect human capital and work effort), the benefits of avoided crime (e.g., early childhood interventions that produce the primary intended impact, better cognitive development, but also later gains in schooling and employment that reduce criminal behavior), the effects of parental incarceration on children's prospects, etc.[16] For many rich individuals, uncertainty influences the assessment of the costs and effectiveness of redistribution and security as solutions to crime.[17] Considering this uncertainty, demands for protection should not be incompatible with preferences for redistribution. In Western Europe, where the empirical analysis below focuses on, we argue that the rich think of redistribution and protection as complementary policies to mitigate regional crime.[18]

III. DATA

To explore the theoretical claims explained above, we will first consider the effects of income distance at the individual level and of the macro level of inequality. Income distance is meant to capture the effects of individual current tax and transfer considerations and macro inequality those of externality-related factors. The first expectation is that income distance will be a significant determinant of redistribution preferences. We also expect, however, that increasing levels of regional inequality will make the rich more likely to support redistribution. We will then show that the very robust effects of macro inequality are in fact the product of fear of crime among the affluent.

[16] For a review of these assessment issues, see Vining and Weimer (2010).

[17] Moreover, if the poor as potential offenders value their future significantly less than their present welfare, as argued in this paper, the effectiveness of deterrence and punishment is put in question (see Lee and McCrary 2005).

[18] It is also reasonable to expect the level of privately financed security available in Western Europe to be lower than, for example, in the USA (where gated communities and private protection are more common). We will return to the American case in the Conclusion.

Source and coverage of survey data We use data from the European Social Survey, which includes consistent regional level identifiers allowing us to match individual and regional information while working with adequate sample sizes.[19] It also provides a consistent high quality measure of income. We limit our analyses to four surveys collected between September 2002 and January 2009, which was still a time of relative economic calm.[20] Our data set covers 129 regions in 14 countries: Austria, Belgium, Germany, Denmark, Spain, Finland, France, Great Britain, Ireland, Netherlands, Norway, Portugal, Sweden, and Switzerland surveyed between 2002 and early 2009. We treat missing data using multiple imputation (King et al. 2001) to obtain conservative standard errors (more details are given in online supplement S.1).

Redistribution preferences Our dependent variable, preferences for redistribution, 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. To ease interpretation we reverse this scale for the following analyses. Table 1 shows Western Europe to be characterized by a rather high level of popular support for redistribution. While almost 69% of respondents either agree or strongly agree with the statement that the government should take measures to reduce income differences, only 16% explicitly express opposition to redistribution. However, despite this apparent consensus, there exists substantial regional variation in redistribution preferences as well as between rich and poor, as we will show below.

Table 1: Redistribution preferences. Percentages.

Strongly disagree	Disagree	Neither	Agree	Strongly agree
2.7	13.7	15.3	44.7	23.7

Note: Based on five multiply imputed data sets.

The measure of relative income Our central measure of material self-interest is the distance between the income of respondents and the mean income in their country (at

[19] Regional level identifiers are provided by the NUTS system of territorial classification (Eurostat 2007). We selected countries who participated in at least two rounds (to obtain usable regional sample sizes) and which provided consistent regional identifiers over time.

[20] We also eliminated surveys after 2007 as a robustness check with no difference in results.

the time of the survey). In other words, we calculate income distance as a respondent's income minus the country-year income mean.[21]

The ESS captures income by asking respondents to place their total net household income into a number of income bands (12 in 2002-06, 10 in 2008) giving yearly, monthly, or weekly figures. 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).[22] We impute the top-coded income category by assuming that the upper tail of the income distribution follows a Pareto distribution (e.g., Kopczuk et al. 2010). 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.[23] Thus, for each country and each year we convert a country's currency into PPP-adjusted constant 2005 US dollars. Finally, for each respondent we calculate the distance between her income and the mean income of her country-year survey.[24]

Crime We measure individuals' crime concerns via a survey item that has become "the de facto standard for measuring fear of crime" (Warr 2000: 457). It prompts a respondent if he or she is afraid of walking alone in the dark with 4 category

[21]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.

[22]For example, this means that category band J (Less than Eur 1,800) becomes mid-point Eur 900 and category R (Eur 1,800 to under Eur 3,600) becomes Eur 2,700. We conducted a robustness test to show that alternative mid-points do not lead to substantively different results (see online supplement S.6).

[23]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.

[24]The distribution of income distances used in our analysis is summarized in Figure S.1 in online supplement S.2. To illustrate the nature of the measure, we aggregate data over all available waves within countries in this figure. The range of income distances reflects interesting national differences (for example, a more disperse distribution in Switzerland than in Spain) but the analysis to be developed below will emphasize the general effect of individual income distance on redistribution preferences. Note that we also carry out a number of income robustness tests, including one where we express the distance in percentages of the country-year average income (see appendix S.6). We also validated the distribution of income in the ESS against a high quality external reference source, the EU statistics on income and living conditions (see appendix S.9).

responses ranging from "very safe" to "very unsafe". As we discussed above, this captures subjective crime concerns instead of actual crime.[25]

Inequality A wide number of indices are available to measure inequality, of which the Gini index is the most popular one (e.g., Jenkins 1991). We perform a subgroup-decomposition of the Gini into its regional components (on the sub-group decomposability of inequality indices see Shorrocks 1980, 1984; Silber 1989; Cowell 1989).[26] We calculate our regional Gini measure from our full sample of imputed individual level data.[27] Following current 'best practice' in economics, we correct for non-random sampling and small-sample bias. Sample selection effects are taken into account by using an estimator that weights according to a household's sample inclusion probability (e.g., Cowell 2000). Since it is well known that Gini estimates are downward biased when calculated from small sample sizes, we employ the small-sample correction proposed by Deltas (2003). Gini values, so constructed, are estimated with error. In fact every measure of inequality is fraught with error – a fact that is often ignored in

[25] It could be argued that being afraid of walking alone in the dark is more related to fear of violent crime that to fear of property crime. To the extent that the connection between macro inequality and crime is considered to apply only to property crime, this would be an issue. There are, however, firm grounds to argue that violent crime would have similar effects to property crime in our model. Focusing on actual crime (rather than fear of crime as we do in our paper) in developing countries, Bourguignon argues that the relationship between inequality/poverty and crime in Becker's canonical model is mostly unaffected by the consideration that much of violent crime involves "conflicts that relate to the control of illicit activities like drug dealing, drug trafficking, gambling, and prostitution" rather than "more conventional property crimes like burglary or robbery" (2001: 180). This framework adds a new determinant of the general level of crime (namely the way the illegal sector is organized and the size of the sector), but it "remains true that an increase in urban poverty should, other things being equal, result in an increase in violence" (Bourguignon 2001: 181). Empirically, there is evidence in the criminology and sociology literatures supporting the existence of this link. For analyses of this relationship across countries and over time, see, for example, Fajnzylber et al. (2002a), which shows that income inequality leads to both higher robbery and higher homicide rates, and Fajnzylber et al. (2002b), showing the Gini index to be an important factor driving violent crime rates across countries and over time.

[26] Decomposability means that an index can be decomposed into three group-components: $B + W + k$, where W and B represent within and between group variance, respectively, while k is a residual component. An index is perfectly decomposable if $k = 0$. This is true, for example, for members of the family of Generalized Entropy measures; but it is not necessarily true for the Gini. We decided to use Gini in our main text since it is the most common measure. However, we replicated our results using the Theil index (obtained from a generalized entropy measure with parameter 1), which is perfectly decomposable. The correlation between it and our (small-N corrected) Gini measure is 0.98.

[27] We use the relative income measure explained above with an imputed top-coded income, which insures that our measure is not censored (and thus missing top-end inequality).

current research and which leads to classical errors-in-variables bias. In our analyses, we account for measurement error in our Gini estimates.

First, we use a jackknifing variance estimator to generate regional Gini standard errors (Karagiannis and Kovacevic 2000). Thus, for each Gini value, we have a point estimate $\hat{w}_j$ and a standard error $\sqrt{\text{Var}(\hat{w}_j)}$. Then, in all analyses described below, we account for measurement error following the methodology outlined by Blackwell et al. (2012), who propose to treat measurement error in the framework of multiple imputation by creating several "multiply overimputed" data sets, in which the variable measured with error is drawn from a suitably specified distribution representing the variable's measurement error.[28] To implement this idea, we generate 5 overimputed data sets with Gini values for each data set drawn from $w_j \sim N\left(\hat{w}_j, \text{Var}(\hat{w}_j)\right)$. To illustrate the 'penalty' incurred by this measurement error technique, we plot, in Figure 2, three regions with similar Gini estimates, but different standard errors. Région lémanique (in Switzerland), Niedersachsen (in Germany), and Noord-Friesland (in the Netherlands) share an estimated regional Gini between around 0.31 and 0.32. For each region we show the Gini estimate as black dot and five random multiple-overimputation draws as gray diamonds. Figure 2 clearly shows how larger Gini standard errors lead to a considerable increase in the variance of overimputed values. We use these overimputed values to estimate all our models five times; average our estimates and penalize standard errors as a function of the variance between overimputations, as suggested in Blackwell et al. (2012) or Rubin (1987). In essence, we account for the errors-in-variables problem caused by the uncertainty of our Gini estimates.[29]

Individual- and regional-level controls We control for a range of standard individual characteristics, namely a respondent's gender, age in years, years of schooling, currently being unemployed, not in the labor force, and the size of the household. We include a measure of social class. While social class is theoretically somewhat ambiguous, it allows us to capture a broad range of socio-economic outcomes which might be confounded with our income and inequality measures. Furthermore, we include a measure of specific skills, differentiating between high and low general

[28] In essence, the idea of multiple overimputation is to treat measurement error as a form of partly missing data. Since we already use multiple imputation to deal with missing individual level data, the multiple overimputation strategy can piggyback on these. For more details see Blackwell et al. (2012).

[29] Note that this is a quite *conservative* strategy. Our main results are stronger when ignoring measurement error.

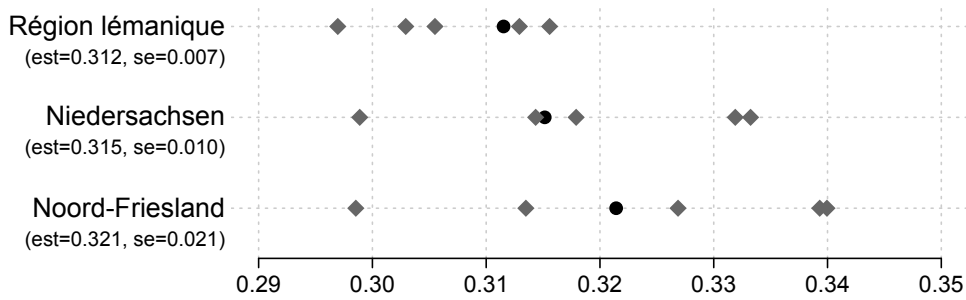

Figure 2: Illustration of multiple overimputation of Gini measurement error

skills, and specific skills. As controls for existing regional differences we include the harmonized regional unemployment rate, gross domestic product, the percentage of foreigners (see, e.g., Alesina and Glaeser 2004, Finseraas 2008) and a summary measure of a region's high-tech specialization.[30] Descriptive statistics for all variables can be found in online supplement S.3.

IV. METHODOLOGY

Models In the first stage of our analysis we study the link between inequality, relative income, and redistribution preferences R_i^*. Our model specification is

$$R_i^* = \alpha(v_i - \bar{v}) + \beta w_j + \gamma w_j(v_i - \bar{v}) + \boldsymbol{\delta}'\boldsymbol{x}_{ij} + \epsilon_{iR}. \tag{1}$$

This is an ordered probit regression of (latent) redistribution preferences R_i^* on our covariates of interest and controls.[31] Here α captures the effect of relative income, the difference between an individual's income v_i and country-year average income $\bar{v}$. The remaining (non-tax and transfer) effect of macro inequality w_j is captured by β.

[30] We used a factor model to generate a summary measure for regional high-tech specialization. We collected Eurostat data on regional information on the share of a region's total workforce employed in science and technology sectors, the share of the economically active population that hold higher degrees, a head count of personnel employed in R&D, and regional total R&D expenditure.

[31] Redistribution preferences R_i^* are a latent construct obtained from observed categorical survey responses R (with K_r categories) via a set of thresholds (e.g. McKelvey and Zavoina 1975; Greene and Hensher 2010) such that $R = r$ if $\tau_{r-1} < R^* < \tau_r$ $(r = 1, \ldots, K_r)$. Thresholds $\boldsymbol{\tau}$ are strictly monotonically ordered and the variance of the stochastic disturbances is fixed at $\epsilon_{iR} \sim N(0,1)$ yielding an ordered probit specification. For a more detailed discussed of this model setup see online supplement S.4.

Since we argue that inequality effects are more relevant among the rich than among the poor, our model includes an interaction between inequality and individual income with associated effect coefficient γ. Finally, we include a wide range of individual and regional level controls $\boldsymbol{x}_{ij}$ whose effects are represented by $\boldsymbol{\delta}$.

In the second stage of our analysis we jointly model preferences for redistribution R_i^* and fear of crime C_i^*.[32] A strict test for our argument that fear of crime is an important externality-related determinant of preferences is to estimate its direct effect in our redistribution equation.

$$C_i^* = \alpha_1(v_i - \bar{v}) + \beta_1 w_j + \boldsymbol{\delta}_1' \boldsymbol{x}_{1ij} + \epsilon_{iC} \tag{2}$$

$$R_i^* = \lambda_1 C_i + \lambda_2 C_i(v_i - \bar{v}) + \alpha_2(v_i - \bar{v}) + \beta_2 w_j + \gamma w_j(v_i - \bar{v}) + \boldsymbol{\delta}_2' \boldsymbol{x}_{2ij} + \epsilon_{iR}. \tag{3}$$

The direct effect of fear of crime on redistribution preferences is captured by λ_1 and λ_2 in our extended redistribution equation (3). It still includes the main effect of income distance α_2 as well as the remaining effect of inequality β_2 and its interaction with income distance, captured by γ. Estimates of individual and regional level controls $\boldsymbol{x}_{ij}$ are given by $\boldsymbol{\delta}$. Our fear of crime equation (2) contains relative income (captured by α_1), inequality (β_1), as well as further controls ($\boldsymbol{\delta}_1$).

In this second stage, our main interest lies on λ_1 and λ_2 which capture the effect of fear of crime (and its interaction with income) on redistribution preferences net of all other covariate effects. Ideally, if fear of crime plays a significant role in explaining redistribution preferences, we expect to see (i) a significant effect of inequality on fear of crime: $\beta_1 \neq 0$; (ii) a significant effect of fear on preferences: $\lambda_1 \neq 0$; and (iii) a reduction of the (remaining) effect of inequality on the rich γ vis-a-vis equation (1).

This is a simultaneous (recursive) ordered probit setup, sometimes called an endogenous treatment model (Greene and Hensher 2010: ch.10).[33] Errors from the redistribution and crime equations are correlated and thus specified as distributed bivariate normal (Greene 2002: 711f.): $[\epsilon_{iC}, \epsilon_{iR}] \sim BVN(0, 0, 1, 1, \rho)$. Here ρ captures the correlation of unobservables between both equations that are not due to the direct effect of fear. The model can be seen as a straightforward extension of the familiar bivariate probit model to ordered data (Butler and Chatterjee 1997).

[32] Our fear of crime variable C is also ordered categorical and we use the same ordered probit specification as for our redistribution measure, i.e., $C = c$ if $\tau_{c-1} < C^* < \tau_c$ $(c = 1, \ldots, K_c)$ with strictly ordered thresholds and errors $\epsilon_{iC} \sim N(0, 1)$ for identification.

[33] The system is recursive because C_i is allowed to influence R_i but not vice versa. The model employs the standard assumption that $E(\epsilon_{iC}|\boldsymbol{x}_{1ij}, \boldsymbol{x}_{2ij}) = E(\epsilon_{iR}|\boldsymbol{x}_{1ij}, \boldsymbol{x}_{2ij}) = 0$.

The effect of fear of crime is identified from the functional form assumption on the correlation structure between residuals (Wilde 2000; Heckman 1978).[34] However, to add one more level of robustness to the model (against distributional misspecification), we also use an exclusion restriction in our preference equation, i.e., $\boldsymbol{x}_{1ij}$ contains at least one covariate not in $\boldsymbol{x}_{2ij}$. We use actual victimization, i.e., if the respondent reports that he, or a member of his household, has been a victim of crime. Having been a victim of crime in the past is a strong determinant of fear of crime. We argue that it can plausibly be excluded from an equation describing preferences, in other words, that previous victimization affects preferences for redistribution via raising crime fears, and not via other channels. We have no knowledge of any literature that suggests a link between victimization and redistribution preferences, that is not channeled via increased fear of crime in the future.[35]

Estimation We estimate these two equations jointly by maximum likelihood (Butler and Chatterjee 1997).[36] In this setup, individuals within the same region and country will share unobserved characteristics, rendering the standard assumption of independent errors implausible (e.g., Moulton 1990; Pepper 2002). Thus, to account for arbitrary within region and country error correlations we estimate standard errors using nonparametric bootstrapping, resampling regions and countries, in order to yield conservative standard errors (e.g., Wooldridge 2003).[37]

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[34] More technically, a full rank condition of the covariate matrix is enough, as discussed earlier by Heckman (1978). This is achieved by the existence of at least one continuous, varying, exogenous regressor in each equation, "an assumption which is rather weak in economic applications" (Wilde 2000: 312).

[35] Nonetheless, one should keep in mind that even if this exclusion restriction should be violated, the model is still identified via the bivariate normal distribution.

[36] See Yatchew and Griliches (1985) for a discussion of the disadvantages of two-step estimation. We use CMP version 6.8.0 (Roodman 2011). Freedman and Sekhon (2010) caution against convergence to local maxima, which we check by (i) running our model several times from dispersed initial values, (ii) bootstrapping individual observations. In each case we get essentially the same results.

[37] Alternatively, one might employ heteroscedasticity-consistent standard errors, which are asymptotically equivalent to bootstrapped standard errors, or multilevel models. However, to correctly capture the correlation structure between units, their 'clustering' should be specified at the highest level. In our case this implies robust standard errors or random effects based on only 14 (country) units. Both methods can be severely biased with a small number of clusters (see, e.g., Angrist and Pischke 2008 and Stegmueller 2013a). Thus we opt for the nonparametric bootstrap, which is not adversely affected by sample size. Note that the decision to employ bootstrapping simply leads to conservative standard errors but does not in any way drive our results.

V. REGIONAL VARIATION IN INEQUALITY AND PREFERENCES

We have argued above that rich individuals who are concerned about crime because they live in unequal areas will be more likely to support redistribution. Panel (A) of Figure 3 represents a first illustration of the two things this paper's argument is about: the existence of regional variation in support for redistribution among the rich and the poor. It captures the average level of support (i.e., the mean of the 5-point scale) for redistribution in each of the regions in the sample. First among the rich (those with household incomes 30,000 PPP-adjusted 2005 US dollars above the mean, the 90th percentile in the sample's income distribution) and then among the poor (with household incomes 25,000 PPP-adjusted 2005 US dollars below the country-year mean, the 10th percentile).

Figure 3 (A) strongly suggest the existence of a general relative-income effect. By looking at the two panels side by side, we can see that the support for redistribution of the poor is almost always higher than that of the rich (there are some exceptions, but these are limited to very few regions where support for redistribution is generally very high for both groups). While the poors' average regional support for redistribution is close to 4 in the 5-point scale (the "Agree" choice), the average for the rich is closer to 3 (the "Neither agree nor Disagree" choice). The figure also shows a remarkable amount of regional variation. The lowest support for redistribution among the rich (2.2 on the 5-point scale, close to the "Disagree" choice) can be found in a Danish region (Vestsjællands Amt), while the highest support among the rich (4.6) is in a Spanish one (La Rioja). For the poor, the highest support for redistribution (4.5) is in France (Champagne-Ardenne, Picardie and Bourgogne) while the lowest support (2.6) is again to be found in Vestsjællands Amt.

More importantly for the arguments in this paper, the degree of regional variation within countries in Figure 3 (A) is remarkable. Looking at the redistribution preferences of the rich, this variation can be illustrated by comparing two regions in the United Kingdom. In the South East of England, the rich exhibit a low support for redistribution (2.8) while in Northern Ireland they are much more supportive (3.8, a whole point higher). The preferences of the poor can also be used as an illustration. In Denmark, the poor in Storstrøms Amt are much more supportive of redistribution (3.7) than in Vestsjællands Amt (2.6).

The more systematic analysis to be developed below will help explain the redistribution patterns shown in panel (A) of Figure 3, but an initial illustration of our main explanatory variables is offered in panels (B) and (C). Panel (B) captures

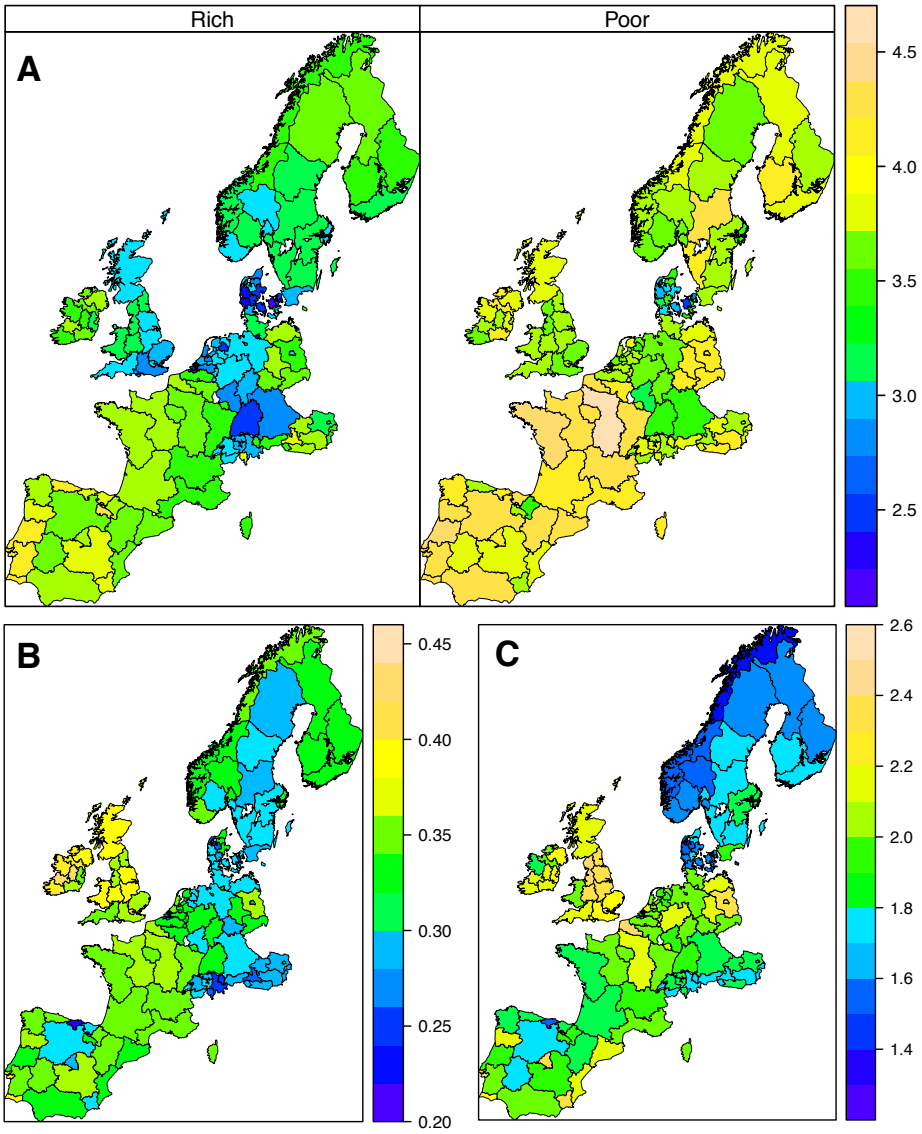

Figure 3: Support for redistribution among rich and poor (A), inequality (B), and fear of crime (C), by region

regional inequality (the Gini index calculated from the individual-level surveys as explained in the previous section) and panel (C) fear of crime (measured as the regional average of the 4-category responses to the survey question about respondents being afraid of walking alone in the dark). The figures show a general correlation between inequality and fear of crime and again a remarkable amount of regional

variation. The lowest levels of inequality and fear of crime can be found in regions of Denmark and Switzerland (and also in Cantabria, Spain). The highest levels of both variables are in some regions in the UK (like London, the North West or the East Midlands), in Ireland's Mid-East and in Portugal (Lisbon).

It is also the case that there is a significant degree of regional variation within countries. Looking at inequality in panel (B), there are stark differences between the South of England and Scotland or between Andalucia and Cantabria in Spain. Looking at fear of crime in panel (C), the regional differences in Spain are again significant (but so are they in Sweden).

VI. MODEL RESULTS

In order to save space, we do not present tables with coefficient estimates (full tables are available in online supplement S.5). Instead we focus on quantities of interest and calculate both predicted probabilities and marginal effects for the rich and poor conditional on different levels of macro inequality. Suffice it to say at this stage that parameter estimates for income, inequality, and their interaction are statistically significant. As expected, we find that income distance has a negative effect on redistribution preferences: the further above someone is from the mean income, the more she opposes income redistribution. We also find that increasing macro inequality goes hand in hand with higher preferences for redistribution, and that this relationship increases with an individual's income distance.

To gain a more intuitive understanding of the role of inequality, we calculate average predicted probabilities for supporting redistribution among rich and poor individuals living in high or low inequality regions, respectively.[38] In Figure 4, the only factors that change in the comparison of predicted probabilities, therefore, are income distance to the mean (in the x-axis) and the two levels of macro inequality (in the solid and dashed lines). High inequality refers to Gini values at the 90th percentile of the regional distribution (as in the East Midlands in the UK), while low inequality refers to the 10th (as in Oberösterreich in Austria). The results provide a

[38] In this section, we define support for redistribution as a response of "strongly agree" on our redistribution measure. Average predicted probabilities are calculated by setting the variables in question to the chosen values while holding all other variables at all their observed values. The final estimates are the average of these predictions. We do the same below when calculating average marginal effects. See Hanmer and Kalkan (2013) for a recent discussion of the advantages of this strategy (*vis-à-vis* simple predicted probabilities calculated at sample averages).

clear picture of the correspondence between our theoretical argument (in Figure 1) and the empirical findings.

Since, statistically, it is not strictly correct to infer the significance of the difference from our (non-)overlapping confidence intervals (see Afshartous and Preston 2010 for a detailed argument), we look at the differences between the poor and the rich more systematically in Table 2. As before, we define rich and poor as the 90th and 10th percentiles of the income distribution.[39] The results in panel (A) provide strong confirmation of our theoretical expectations. Among the poor the probability of strongly supporting redistribution remains at similar levels regardless of the level of inequality, changing only from 26 to 28 percent when moving from low to high inequality. In contrast, the effect of macro inequality is more pronounced among the rich: explicit support for redistribution rises from 17 percent in low inequality regions to over 22 percent in high inequality areas. In other words, the difference in predicted support for redistribution due to increased inequality is more than twice as large among the rich (and it is a statistically significant difference).

To put this conclusion to a stricter test we calculate the average marginal effects of macro inequality for rich and poor individuals, shown in panel (B) of Table 2 together with their respective standard errors and 95 percent confidence bounds. The results further support our argument. The marginal effect of inequality among rich individuals is large and statistically different from zero. In contrast we find a considerably smaller marginal effect among the poor, with a 95 percent confidence interval that includes zero. Higher levels of macro inequality increase the probability of support for redistribution among the rich, but make little difference to the poor.

It is important to point out that the estimates in Table 2 represent a significant amount of support for the relationship hypothesized in Figure 1. As we expected, redistribution preferences converge for the poor regardless of the macro-level of inequality. We also find the redistribution preferences of the rich to diverge as macro inequality grows. While we need to keep in mind that our results emphasize the effects of regional (and not national) inequality, some influential alternative hypotheses are contradicted by our evidence.

An prominent literature posits that, in high inequality contexts, the poor are diverted from the pursuit of their material self-interest. This effect would imply that, in contradiction to Figure 1, redistribution preferences would diverge for the poor and

[39] Using the 90th percentile defines "the rich" as being 30,000 (constant, ppp-adjusted) Euros above the national mean. Note that the same substantive pattern of results is obtained when we define rich as those only 20,000 Euros above the mean.

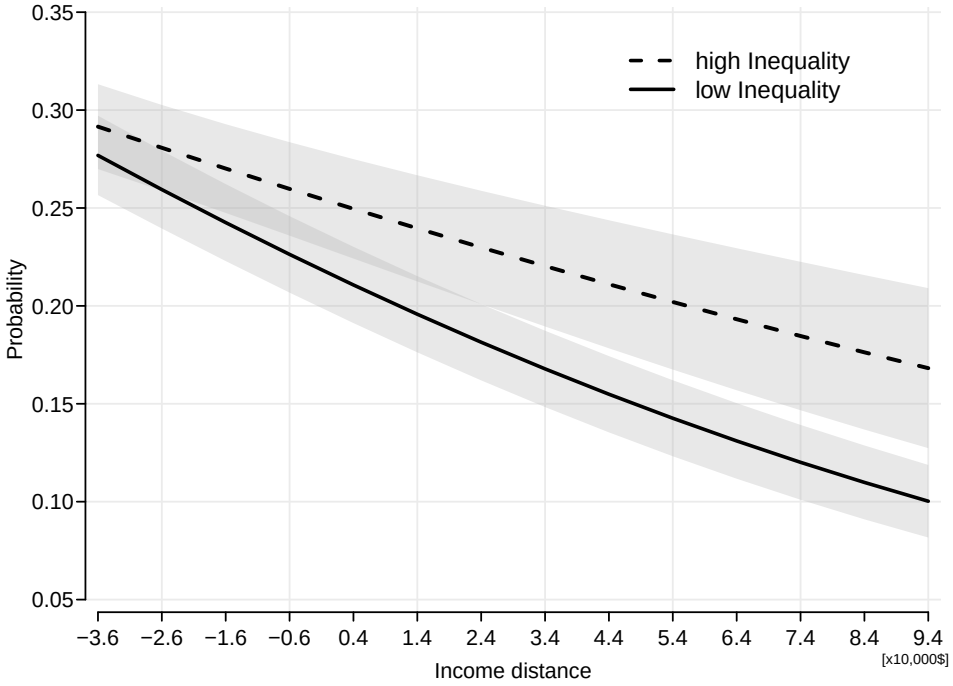

Figure 4: Support for redistribution. Predicted probability by income and inequality with 90% confidence intervals.

Table 2: Support for redistribution. Panel (A) shows predicted probabilities by income and inequality, panel (B) shows marginal effect of inequality among rich and poor

(a) Predicted probabilities

		Gini	
		low	high
Income	Poor	25.8	28.0
	Rich	17.4	22.5

(b) Marginal effect of inequality

	Marginal effect of Gini			
	est	s.e.	95 % CI	
Poor	0.246	0.202	−0.150	0.643
Rich	0.568	0.253	0.069	1.067

Note: Calculated from equation (1). All estimates available in Table S.3, online appendix S.5. Region-county bootstrapped, multiple overimputation standard errors.

converge for the affluent. Perhaps the most well-known example of these arguments is its application to the high inequality example of the US and the contention that second-dimension issues (particularly cultural and social) outweigh economic ones

for the American working class.[40] More comparatively, Shayo's (2009) important contribution to the political economy of identity formation follows a similar logic.[41] If these arguments were correct, we would expect the poor in unequal countries to be distracted from their material self-interested redistribution preferences, to the extent that these second-dimension concerns are correlated with macro-level inequality.[42] The results presented above suggest that the poor are not distracted from the pursuit of their present material self-interest in regions with higher levels of macro inequality, whether because of second-dimension concerns or prospects of upward mobility.

In another theoretical alternative, Lupu and Pontusson (2011) propose that macro-levels of equality are related to empathy. They argue that, because of social affinity, individuals will be inclined to have more similar redistribution preferences to those who are closer to them in terms of income distance. While Lupu and Pontusson emphasize skew (rather than Gini) and the position of the middle class, their argument implies that social affinity would make the rich have higher levels of support for redistribution as inequality decreases and their social distance with the middle class and the poor is reduced (the opposite of the predictions in Figure 1). A similar relationship would be expected by the approach that relates beliefs in a just world to redistribution preferences. To the extent that macro-levels of inequality are related to these beliefs (for example that inequality rewards the hard-working and punishes the lazy), we would observe lower levels of support for redistribution from the rich in countries with higher inequality and a higher normative tolerance for it (Benabou and Tirole 2006; Alesina and Glaeser 2004). Our evidence fails to support these arguments.

As we mentioned above, an influential literature in comparative political economy has argued that, if macro inequality means that the rich are more likely to become

[40] See Frank (2004), the critique in Bartels (2006), and the comparative analyses by De La O and Rodden (2008), Huber and Stanig (2011), and Stegmueller (2013b).

[41] 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.

[42] A similar expectation emerges from the "prospect of upward mobility" (POUM) hypothesis. Benabou and Ok (2001) argue that the poor do not support high levels of redistribution because of the hope that they, or their offspring, may make it up the income ladder. To the extent that mobility is correlated with macro-level inequality (something often argued in relation to the US but that is empirically not clear), we would expect a different relationship between income and preferences from that depicted in Figure 1.

poor, current generosity may not reflect externality concerns but the demand for insurance against an uncertain future (Moene and Wallerstein 2001; Iversen and Soskice 2009; Rehm 2009). To address this, we introduced an explicit measure of risk into the analysis. An important component of the demand for insurance and redistribution has to do with the risk of becoming unemployed. We operationalize risk as specific skills. Iversen and Soskice (2001) argue that individuals who have made risky investments in specific skills will demand insurance against the possible future loss of income from those investments. Our measure of skills (taken from Fleckenstein et al. 2011) distinguishes among specific, high and low general skills and it is meant to capture this individual risk directly. The effects of risk are not an issue of primary importance to our analysis, we are only interested in showing that our findings are robust to the inclusion of these explicit measures of risk. And this is indeed the case in Figure 4 and Table 2.[43]

In the previous sections, we went on to argue that the main mechanism linking inequality and redistribution preferences is fear of crime. In the second stage of our analysis, we thus estimate our simultaneous ordered probit model linking inequality to fear of crime, which then is expected to shape preferences for redistribution (all estimates for both equations are available in Table S.4, online appendix S.5). In our fear of crime equation, we include a number of factors identified in the literature (e.g., Hale 1996). We find, not surprisingly, that having previously been a victim of crime increases a person's fear of crime and that other variables affect fear of crime in the expected directions. More importantly, our results show that, in agreement with our argument, in regions with higher levels of inequality, respondents – whether rich or poor – are more afraid of crime. We also find clear evidence that fear of crime matters for redistribution preferences. Individuals who are more afraid of crime show higher levels of support for redistribution, a relationship that is slightly stronger among those with higher incomes. A test for independence of fear of crime and redistribution equations is rejected (F=14.9 at 2df.). We also find that the direct effect of macro inequality becomes statistically insignificant once we explicitly estimate the effect of fear of crime.

Again, a stricter test of our hypotheses can be obtained by calculating average marginal effects. We expect to find (i) a significant (both in the statistical and substantive sense) marginal effect of fear of crime on redistribution preferences, and

[43]For the estimates of the skills variables, see online appendix S.5. Furthermore, using Iversen and Soskice's (2001) alternative measure of skill specificity leads to the same results, as shown in online appendix S.6.

Table 3: Effects of fear of crime and inequality among the rich. Average marginal effects for predicted strong support of redistribution.

	Marginal effect among rich			
	est	se	95% CI	
Fear of crime	0.099	0.033	0.035	0.163
Gini	0.317	0.261	−0.198	0.833

Note: Calculated from eqs. (2) and (3). All estimates available in Table S.4, online appendix S.5. Region-county bootstrapped, multiple overimputation standard errors.

(ii) the size of the remaining effect of macro inequality (operating through other channels) to be reduced. Table 3 shows average marginal effects of fear of crime and inequality among the rich. As already indicated by our coefficient estimates, the marginal effect of fear of crime is strong and clearly different from zero. More importantly, we find the remaining marginal effect of inequality to be greatly limited. In fact, it is reduced to such an extent that its confidence interval includes zero. This result does of course not negate the existence of other relevant channels linking inequality and preferences, but it at least signifies that externalities go a long way in explaining the effect of inequality on redistribution preferences.

Robustness tests We conducted a large number of robustness tests studying alternative model specifications (too many to include here). They are described in detail in supplement S.6. To capture alternative macro explanations, we included existing levels of redistribution, regional transfers, measures of urbanization and population density. To capture alternative individual-level explanations, we included religion, ideology, an alternative measure of skill specificity, and a measure of altruism. We also carried out a number of tests for our measurements of income and inequality. Our estimates (in Table S.5, appendix S.6) show that our core conclusions remain valid under these alternatives. Furthermore, appendix S.7 includes specifications using country and year fixed effects, again, confirming our main results.

VII. CONCLUSION

It is appropriate to conclude this paper by re-emphasizing the importance of our main results and exploring some of their implications for further research. The evidence demonstrates that for the poor externality concerns are trumped by immediate disposable income incentives and that redistribution preferences converge regardless of the macro-level of inequality as income declines. By contrast, macro inequality promotes concerns about negative externalities for the rich. We showed that the redistribution preferences of a rich individual in a low-inequality region differ starkly from those of a similarly rich individual in a high-inequality region and, more importantly, that this difference is motivated by fear of crime.

In some ways, this is a profoundly unintuitive result (the rich are more supportive of redistribution in those regions where inequality is highest). We do provide an intuitive solution for this puzzle (the concern for crime by the rich) but it is germane to ask whether our results emerge from the idiosyncrasies of our particular sample. We have mentioned before that the rich, if concerned about the externalities of inequality, could do (at least) two things: reduce inequality through redistribution, or reduce its potential consequences by demanding more protection. We have argued that demands for redistribution and security can be complementary, but it is tempting to think that the rich in Western Europe may be more likely than the rich in other regions to think of redistribution as an attractive option. In related (but preliminary) work reproducing the analysis presented in this paper, however, the effect of macro inequality in the US is remarkably similar to what we find in Western Europe. The American data allow us to directly address the possibility of a security versus redistribution trade-off. Looking at inequality at the state level in the US, the evidence we find supports the idea that these preferences are complementary, as individuals more likely to support redistribution are also more likely to support increasing the resources dedicated to public security provision. While this is a topic we hope to do further research on, we will mention that our findings connect with a significant literature of the consequences of inequality in the US. Using American data and focusing on voting behavior, Gelman et al. (2008) find, like us, that the poor (whether in Connecticut or Mississippi) are quite similar. It seems to be the case that it is the rich who are responsible for some of the political differences we see (in Western Europe as well as the US). And this is perhaps the most important take-home message in our paper.

Our research, moreover, runs counter to a set of findings in the psychology literature about the influence of income on charitable giving and pro-social behavior. Using

surveys conducted in the US, some authors find that lower income individuals give proportionally more to charitable causes than higher income ones (see for example, James and Sharpe 2007).[44] Other authors using experimental data find that subjective perceptions of one's social class promote generosity and charitable donations (see Piff et al. 2010). This paper does not address the role of altruism in determining voluntary donations. But our results do indicate that, irrespective of charity and controlling for altruism,[45] the rich in Western Europe are more likely to support government-based redistribution when regional inequality makes them more concerned about crime.

Going back to the unintuitive nature of our findings, one might finally ask why we do find less redistributive systems in precisely the places where the rich are more supportive of redistribution. We think this is an important question in need of a significant amount of further research. As McCarty and Pontusson (2009) note, models of the political economy of redistribution involve two separate propositions: there is a "demand" side, concerning the redistribution preferences of voters, and a "supply" side, concerning the aggregation of these preferences and the provision of policy. In this paper we have focused on the first proposition and ignored the second. We hope that the arguments in this paper clarify the role of preferences as an essential first step for an accurate understanding of the supply of redistribution.

[44]This research has found wide resonance in the popular press. See Greve (2009) or Johnston (2005).
[45]See the altruism analysis in Appendix S.6.

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Supplement to "The Externalities of Inequality: Fear of Crime and Preferences for Redistribution in Western Europe".

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S.1 MULTIPLE IMPUTATION

We use multiple imputation to address missing values. It is well known that listwise deletion or various 'value substitution' methods are likely to produce biased estimates and standard errors that are too small (Allison 2001; King et al. 2001; Little and Rubin 2002). Using multiple imputation we not only obtain complete data sets, but (more importantly) generate conservative standard errors reflecting uncertainty due to missing data (Rubin 1987, 1996). Multiple imputations are created by random draws from a multivariate normal posterior distribution for the missing data conditional on the observed data (King et al. 2001). These draws are used to generate five complete (i.e., imputed) data sets. All our analyses are performed on each of these five data sets and then averaged with standard errors adjusted to reflect the uncertainty of the imputed values (Rubin 1987).

Imputations are carried out using the missing-at-random assumption, which entails that the process generating missing values is either purely stochastic, or depends on other observed covariates (Rubin 1976). This assumption is violated if missing values depend on "themselves" (e.g., when higher income individuals are more likely to refuse to answer an income question). One way to deal with this issue is to use additional covariates, which are not part of the analysis model but can be used in the imputation model to better predict missing income and thus move us closer to the missing-at-random ideal (Rubin 1996). As additional predictors we include a set of variables which represent the economic situation of a household – the number of dependent children, if it is located in an urban or rural area, as well as questions on satisfaction with one's current income. Furthermore, we include other variables which are known from previous research to be significantly related to income: an assessment of one's subjective health, general life satisfaction, and political ideology.[3] However, since we cannot test if missing values are in fact missing at random, we also perform an additional check. If respondents with particularly high or low incomes were less likely to answer our income questions, or even participate in the survey at all, we would find a rather skewed distribution of income when comparing it to external reference sources. In Section S.9 we compare the income distribution of the ESS to two high-quality household surveys, which specialize in measuring income (the EU Statistics on Income and Living Conditions and the German Socio Economic Panel). We do not find any stark systematic differences.

Table S.2 on page 5 in the descriptive statistics section shows which variables are imputed and what percentage of cases are imputed. It shows that raw and imputed data

[3]Note that their relationship does not need to be 'causal,' only predictive of income.

are very similar. Note that we also carry out a test in the model robustness section in this appendix (see Section S.6), which shows that our results are not driven by our imputation procedure (i.e., they are obtained via listwise deletion as well).

S.2 DISTRIBUTION OF INCOME DISTANCE

Figure S.1 plots the distribution of income distance by country. For simplicity, we average over multiple years (and use density estimation to "smooth" over categories). Distributions are calculated via kernel density estimation with a fixed bandwidth of 1.4. The blue line shows the density of the raw income data (i.e., before multiple imputation), the red line shows the density after imputation of missing income.

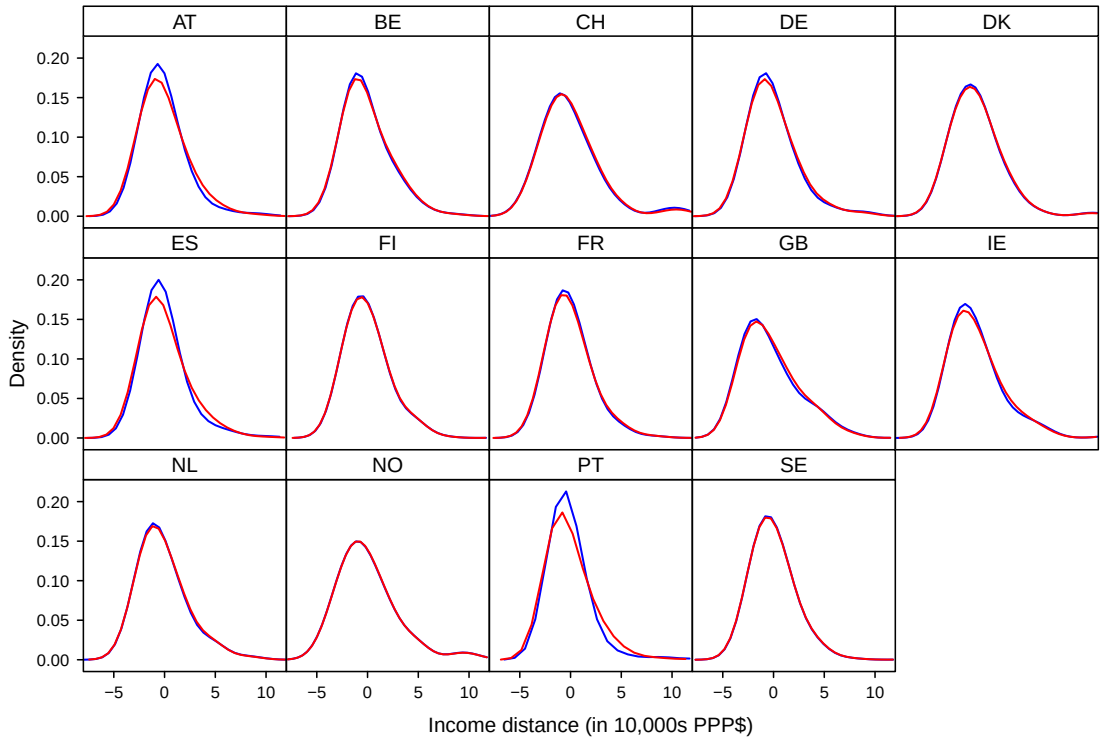

Figure S.1: Distribution of income distance. Kernel density estimates (Gaussian kernel, bandwidth 1.4). Blue line shows raw data, red line imputed data.

S.3 DESCRIPTIVE STATISTICS

Table S.1 shows the countries and respective European Social Survey rounds included in our analysis.

Table S.1: Overview of countries and ESS rounds included in the analysis

Country	ESS rounds
AT	2002, 2004, 2006
BE	2002, 2004, 2006, 2008
CH	2002, 2004, 2006, 2008
DE	2002, 2004, 2006, 2008
DK	2002, 2004, 2006
ES	2002, 2004, 2006, 2008
FI	2006, 2008
FR	2002, 2004, 2006, 2008
GB	2002, 2004, 2006, 2008
IE	2002, 2004
NL	2002, 2004, 2006, 2008
NO	2002, 2004, 2006, 2008
PT	2002, 2004, 2006, 2008
SE	2002, 2004, 2006, 2008

Table S.2 contains descriptives statistics of the variables included in our analysis. We show statistics for both the 'raw' data using listwise deletion of missing cases, and the data after our multiple imputation procedure. Most variables have statistically indistinguishable first and second moments under both listwise deletion and imputation. Only mean imputed income is slightly higher than under listwise deletion (probably due to underreporting of high income earners). Nonetheless, this difference is minor and does not, as we show in a robustness check (see Table S.5 below), influence our substantive conclusions.

Table S.2: Descriptive statistics (using listwise deletion and multiple imputation). Means, standard deviations and percentages.

	Listwise		Imputed[a]	
Continuous	Mean	SD	Mean	SD
Income [10,000 USD][†]	3.48	2.76	3.53	2.67
Income distance	0.00	2.68	0.00	2.61
Age (years)	48.45	17.68	48.45	17.68
Education (years)[†]	12.16	4.29	12.15	4.29
Household member[†]	2.59	1.35	2.59	1.35
Regional high-tech spec.	0.14	1.01	0.14	1.01
Regional GDP [10,000 USD]	2.69	0.69	2.69	0.69
Regional Gini	0.33	0.03	0.33	0.03
Foreigners [% in region]	6.58	5.77	6.58	5.77
Unemployment [% in region]	6.51	3.44	6.51	3.44
Dichotomous	%		%	
Unemployed[†]	4.4		4.5	
Not in labor force[†]	17.0		17.1	
Female	53.0		53.0	
Crime victim[†]	20.1		20.1	
Lower supervisor	20.2		20.2	
Self-employed	8.1		8.1	
Skilled worker	10.2		10.2	
Unskilled worker	22.7		22.7	
High general skills	37.2		37.2	
Low general skills	34.6		34.6	
Specific skills	20.3		20.3	

[a] Based on five multiply imputed data sets

[†] Variable has imputed missing values. Income: 19.1% imputed, Education: 1.1%, Unemployed, Not in LF: 0.4%, HH members: 0.1%, Crime victim: 0.2%.

S.4 MODEL DETAILS

As described in the main text, our model is a latent variable model, where redistribution preferences R_i^* are a latent construct obtained from observed categorical survey responses R (with K_r categories) via a set of thresholds (e.g. McKelvey and Zavoina 1975; Greene and Hensher 2010) such that $R = r$ if $\tau_{r-1} < R^* < \tau_r$ $(r = 1, \ldots, K_r)$.[4] Thresholds $\boldsymbol{\tau}$ are strictly monotonically ordered and the variance of the stochastic disturbances is fixed at $\epsilon_{iR} \sim N(0, 1)$ yielding an ordered probit specification. This model thus possesses what is known as the single-crossing property: as one moves along the values of x, the predicted probability $Pr(y = r)$ changes only once (Greene and Hensher 2010; Boes and Winkelmann 2006). As Greene and Hensher (2010) argue at length, models that do not enforce this restriction (such as multinomial or generalized ordered logit modes) are not appropriate for strictly ordered preference data. An argument that is sometimes made (especially in the sociology literature) is that one should conduct a Brant test, which compares an ordered specification with an 'unordered' one. However, since an unordered specification is clearly an inappropriate *behavioral* model for the data used here, we do not pursue this further. For arguments against this kind of test, see Greene and Hensher (2010).

[4] An ordered probit model needs two identifying restrictions. Besides setting the scale by fixing the error variance, we fix the location by not including a constant term (but estimate all thresholds).

S.5 COMPLETE TABLES OF MODEL ESTIMATES

Table S.3: Income inequality and redistribution preferences. Maximum likelihood estimates, bootstrapped, multiple overimputation standard errors, and 95% confidence intervals.

Eq.: Redistribution	est.	s.e.	95% CI	
Income distance	−0.045	0.003	−0.050	−0.039
Inequality (Gini)	1.392	0.741	−0.069	2.853
Distance×Gini	0.247	0.105	0.041	0.453
Age	0.010	0.005	0.000	0.019
Female	0.142	0.013	0.118	0.167
Education	−0.023	0.002	−0.028	−0.018
Unemployed	0.150	0.022	0.107	0.193
Not in labor force	−0.064	0.012	−0.088	−0.040
Household size	0.020	0.004	0.012	0.029
Self-employed	−0.079	0.018	−0.113	−0.044
Lower supervisor	0.042	0.013	0.017	0.067
Skilled worker	0.066	0.019	0.029	0.102
Unskilled worker	0.123	0.018	0.089	0.158
High general skills	−0.092	0.015	−0.121	−0.063
Specific skills	0.002	0.014	−0.026	0.030
Percent foreigners	−0.228	0.350	−0.914	0.458
Unemployment rate	0.035	0.008	0.019	0.051
High-tech specialization	−0.084	0.036	−0.156	−0.013
Gross-domestic product	−0.059	0.040	−0.137	0.020
Test vs. M0	F=92.05, p=0.000			
Likelihood	−120,206			

Note: Estimates from equation (1) in main text. Multiple overimputation, bootstrapped standard errors based on 100 replicates from 129 regions and 14 countries. N=96,682. Wald test M0 is against null model without predictors. Estimated cut-points not shown. Distribution of test is based on Barnard and Rubin (1999). Likelihood value is averaged across imputations.

Table S.4: Fear of crime, relative income, inequality, and redistribution preferences.

Eq.: Fear of crime	est.	s.e.	95% CI	
Crime victim	0.279	0.021	0.238	0.321
Income distance	−0.017	0.003	−0.024	−0.011
Inequality (Gini)	4.807	0.700	3.410	6.205
Age	0.063	0.005	0.054	0.073
Female	0.587	0.019	0.549	0.625
Education	−0.014	0.003	−0.020	−0.007
Unemployed	0.096	0.026	0.044	0.147
Not in labor force	0.070	0.014	0.043	0.097
HH size	−0.024	0.006	−0.036	−0.013
Self-employed	−0.062	0.023	−0.108	−0.017
Lower supervisor	0.100	0.013	0.074	0.126
White collar	0.185	0.019	0.148	0.222
Blue collar	0.128	0.018	0.093	0.163
Percent foreigners	1.270	0.714	−0.130	2.669
Gross-domestic product	−0.120	0.048	−0.214	−0.026
Eq.: Redistribution				
Income distance	−0.063	0.004	−0.071	−0.054
Inequality (Gini)	0.553	0.798	−1.020	2.127
Income distance*Gini	0.209	0.097	0.018	0.400
Fear of crime	0.246	0.071	0.107	0.386
Income distance*Fear	0.011	0.002	0.007	0.015
Age	0.000	0.006	−0.012	0.013
Female	0.043	0.033	−0.022	0.108
Education	−0.020	0.002	−0.025	−0.016
Unemployed	0.131	0.024	0.084	0.178
Not in labor force	−0.076	0.014	−0.103	−0.049
HH size	0.024	0.004	0.015	0.033
Self-employed	−0.068	0.017	−0.101	−0.035
Lower supervisor	0.026	0.014	−0.002	0.054
Skilled worker	0.037	0.023	−0.007	0.082
Unskilled worker	0.104	0.020	0.065	0.143
High general skills	−0.088	0.015	−0.118	−0.058
Specific skills	0.002	0.015	−0.026	0.031
Unemployment rate	0.034	0.008	0.019	0.050
High-tech specialization	−0.084	0.040	−0.162	−0.006
Percent foreigners	−0.423	0.432	−1.270	0.424
Gross-domestic product	−0.040	0.043	−0.124	0.043
Error corr.	$\hat{\rho}$=−0.186, p=0.004			
Test vs M1	F=216.3, p=0.000			
Test $(\lambda, \rho) = 0$	F=14.9, p=0.000			
Likelihood	−224,607			

Note: System of equations (2) and (3) in main text. Robust, multiple imputation standard errors clustered by 129 regions and penalized for 5 imputations (Rubin 1987). N=96,682. Test M1 is against model without fear equation. Distribution of tests is based on Barnard and Rubin (1999). Likelihood value is averaged across imputation. Error corr. is Fisher's z-transformed correlation between equation residuals.

S.6 ROBUSTNESS AND PLACEBO TESTS

Existing levels of redistribution. Previous research indicates that average support for redistribution tends to fall when the existing levels of redistribution are high. The idea that there is some threshold at which the disincentive effects of redistribution become more severe (see for example Tanzi and Schuhknecht 2000) provides a possible explanation for this relationship. Arguably, people who live in countries with large redistributive welfare states are more concerned about, and more aware of, the disincentive effects of redistribution. It also seems likely that some respondents take actual levels of redistribution into account when expressing their preferences, i.e., that they are expressing agreement or disagreement with the proposition that the government should do more to reduce income differences. In an alternative, but related, explanation, high levels of redistribution are argued to be connected with encompassing welfare and labor market institutions which provide the poor and the rich with more information about redistributive issues (see Kumlin and Svallfors 2007). This would imply more extreme redistribution preferences by poor and rich in high welfare state countries.

To test these alternatives, we include existing levels of social spending in our estimation.[5] The results in model (1) in Table S.5 show that inclusion of pre-existing redistribution reduces the direct effect of inequality in the model with endogenous fear even further, but leaves our core result the role of fear of crime virtually unchanged.

National versus regional redistribution. We consider the measure of redistribution preferences in the paper's analysis to (mostly) capture national redistribution. It is possible that in federal countries (where regional government have more of a role in providing redistribution), respondents integrate a combination of national and regional policy into their assessment of redistribution. We address this issue in two ways. First, we create a measure of regional social spending. The regional measure weights national expenditure by the regional share of the recipient population (since region-specific spending data are only available for a very limited number of countries in our sample).[6] Second, we use a measure of federalism by creating an indicator variable that equals one if, within a country, regions are an autonomous source of government spending (based on Eurostat's Government revenue and expenditure database). Specifications (2a) and (2b) in Table S.5 show that our main conclusions are robust to these considerations.

[5] Spending data are total public social spending (in cash and in kind), per head, in constant 2000 prices and PPP US dollars from OECD's SOCX database. The main social policy areas covered are: Old age, Survivors, Incapacity-related benefits, Health, Family, Active labor market programs, Unemployment, and Housing.

[6] We calculate the regional share of the recipient population from our available survey data as the population share of unemployed, the disabled, and those in retirement.

Table S.5: Overview of robustness checks. Average marginal effects among the rich from simple model and model including fear of crime. Estimates whose 95% confidence interval includes zero are marked with †.

	Simple model		Model with endogenous fear			
	Gini		Fear		Gini	
Robustness tests	Est	s.e.	Est	s.e.	Est	s.e.
(1) Social spending	0.706	0.304	0.088	0.028	0.481	0.308†
(2a) Regional social spending	0.552	0.256	0.101	0.029	0.297	0.255†
(2b) Federalism	0.605	0.280	0.104	0.026	0.353	0.290†
(3a) Population density	0.610	0.267	0.095	0.029	0.401	0.272†
(3b) Urban area	0.562	0.257	0.096	0.029	0.318	0.261†
(3c) Urban region	0.552	0.245	0.102	0.029	0.294	0.250†
(4) Religion	0.494	0.235	0.102	0.027	0.237	0.242†
(5a) Ideology (redist. eq)	0.587	0.245	0.097	0.025	0.338	0.242†
(5b) Ideology (both eq.)	0.587	0.245	0.099	0.025	0.334	0.242†
(6) Altruism	0.580	0.258	0.071	0.025	0.392	0.265†
(7) Skill specificity	0.570	0.262	0.097	0.028	0.324	0.262†
(8) Pre-crisis years	0.570	0.249	0.091	0.031	0.342	0.257†
(9) Listwise deletion	0.588	0.256	0.110	0.032	0.317	0.259†
(10) Income trimmed	0.670	0.278	0.113	0.032	0.411	0.288†
(11) Wage earner sample	0.591	0.271	0.115	0.031	0.299	0.270†
(12) Perturbed midpoints	0.578	0.254	0.109	0.032	0.309	0.258†
(13) Income, percent distance	0.671	0.283	0.111	0.032	0.385	0.282†
(14) Small N regions dropped	0.560	0.273	0.102	0.030	0.297	0.281†
(15) Grouped Gini	0.565	0.260	0.099	0.029	0.317	0.262†
(16) UK and Portugal dropped	0.557	0.274	0.121	0.037	0.329	0.271†
(17) Subsample validation	0.624	0.274	0.085	0.068	0.414	0.319†
Placebo test			Ideology		Gini	
(18) Ideology instead of fear			−0.076	0.004	0.591	0.241

Note: Robust, multiple imputation standard errors clustered by 129 regions and penalized for 5 imputations (Rubin 1987).

Population density/urbanization. Although our analyses emphasize the regional level, one may argue that we ignore political geography, i.e., the distinct preferences of individuals living in high-density, urban areas (see, for example, Cho et al. 2006). As argued by Rodden (2010: 322), it is clear that individuals sort themselves into neighborhoods with similar demographic, occupational, income, and ultimately political preferences. We address this concern in two ways. First, we simply include an individual-level survey variable, which indicates if the respondent lives in an urban region. Second, we construct variables measuring the degree of urbanization of a region (this is simply the regional mean of our individual level variable) and population density (data from Eurostat). Table S.5, specifications (3a), (3b), and (3c) show that both individual and contextual measures do not change our core results.

Religion. Previous research has stressed the role of religion for redistribution preferences (Scheve and Stasavage 2006; Stegmueller et al. 2012; Stegmueller 2013). We expect religion to have an additional effect, largely unconnected to the inequality-preferences nexus. Including religion (indicator variables for Catholic and Protestant, as well as a measure of church attendance) in model (4), we find a somewhat reduced effect of inequality among the rich, but it is still significant (its confidence interval excludes zero).[7] Similar to our previous checks, results in Table S.5 confirm that including fear of crime substantially reduces the remaining effects of inequality.

Ideology. Our main analyses exclude a measure of ideology or left-right self-placement, since we believe that explaining economic preferences helps us understand a key constituent of ideology and therefore it should not be an 'explanatory' variable in our model. Nonetheless, it has been argued that ideological positions are an independent source of redistribution preferences (see Margalit 2013) and we can show that the inequality-fear link is robust to the inclusion of this variable. In Table S.5, specification (5), we account for respondents' ideology in two ways. First, we simply include ideology in our redistribution equation and find the results unchanged. Second, we allow for the fact that conservative respondents might be more likely to indicate fear of crime, by including ideology in our fear of crime equation. Again, we find our results confirmed.

Altruism. A most significant approach to non-economic motivations for redistribution preferences has focused on other-regarding concerns (for reviews, see Fehr and Schmidt 2006; DellaVigna 2009). Other-regarding concerns are particularly relevant to our arguments because of the possibility that the rich could be more willing to support redistribu-

[7]Note that the difference in marginal effects between the rich and poor (0.32) is still highly relevant (s.e.=0.14).

tion in highly unequal societies because they can afford to be more compassionate. This would be the opposite of the relationship between empathy and inequality proposed by Lupu and Pontusson (2011) and analyzed in more detail in the main text. To address this issue directly, we introduce a control for other-regarding preferences. Due to the sparsity of data on altruism, we rely on a proxy measure. The ESS surveys ask respondents to listen to a description of different kinds of persons and to declare whether these persons are (or are not) like them. One of the descriptions is as follows: "She/he thinks it is important that every person in the world should be treated equally. She/he believes everyone should have equal opportunities in life." Respondents can then decide whether this person is 'Very much like me,' 'Like me,' 'Somewhat like me,' 'A little like me,' 'Not like me,' or 'Not like me at all.' We create an indicator variable equal to one for respondents who indicate full agreement with this statement of equality (by responding 'Very much like me'). Our results in specification (6) show that including other regarding preferences does not alter our basic findings.

Alternative measure of skill specificity. In specification (7) we use the "original" measure of skill specificity of Iversen and Soskice (2001). Skill specificity is calculated following Cusack et al. (2006).[8] Using this one-dimensional, continuous measure of skill specificity as opposed to the categorical one based on Fleckenstein et al. (2011), we find our core results unchanged.

Pre-crisis years One might argue that survey interviews conducted in late 2008 are affected by the onset of the global economic downturn. To check for this possibility we drop this entire wave from the analysis in specification (8) and use only interviews conducted before 2008. Results in Table S.5 on page 10 show that this does not affect our results.

Listwise deletion In specification (9) we check if our results also hold when using listwise deletion of missing cases instead of multiple imputation.

Income Owing to the usual constraints of large-scale comparative survey projects, income measurement in the ESS is based on a single item with discrete categories. In the following specifications we test the robustness of our results to changes in the distribution of income. Specification (10) checks the influence of extreme incomes by trimming the bottom and top 5% of incomes from the sample. Our results are not driven by either extremely rich or poor individuals. In fact the effect of richer individuals being more supportive of redistribution in high inequality regions is somewhat more marked in this specification. Adding our explanation in terms of crime to the model reduces this consid-

[8]We are indebted to Philip Rehm for providing us with skill specificity measures on the ISCO 1d level.

Table S.6: Illustration of midpoint perturbation in specification test (12)

			Perturbed Midpoint	
Category	Range	Midpoint	Draws 1	Draws 2
1	0 − 1800	900	814	960
2	1800 − 3600	2700	2928	3083
3	3600 − 6000	4800	5214	5554
4	6000 − 12000	9000	7903	9020
5	12000 − 18000	15000	16419	16171
6	18000 − 24000	21000	18579	20915
7	24000 − 30000	27000	33794	23030
9	30000 − 36000	33000	33908	29851
10	36000 − 60000	48000	48069	45012
11	60000 − 90000	75000	72429	62949
12	90000 − 120000	105000	106446	101101

erably. Specification (11) only includes respondents who currently obtain labor income. Again, our core results are confirmed.

Specification (12) is more demanding. As one of our reviewers has pointed out, the widely used procedure of assigning midpoints (cf. Hout 2004) can have some shortcomings. In essence, if the distribution of income within bands is asymmetric, using conventional midpoints (mean values of each band) would systematically distort imputed income values. This would not be an issue if the direction of this bias was uniform (i.e., all midpoints are too low or too high), since our final income variable is expressed as distance to the mean. However, there is no guarantee that this will be the case. To check the robustness of our results, we thus simulate what would happen if the "true" midpoints of each band were different from the ones we used. To do so, we use a perturbation approach, where the true midpoint μ^* is given by $\mu^* = \mu\delta$, i.e., our standard midpoint, μ, scaled by some bias factor δ. We set δ to be normally distributed with mean 1 and a standard deviation of 0.1. This standard deviation is large enough to substantially alter mid-point values – but not so large as to "switch" categories. The consequences of this approach are illustrated in Table S.6 below. It presents categories, standard mid-points and two sets of draws from our perturbation model. It illustrates how our mid-points are significantly moved both closer and further apart by our perturbation. We use this perturbed data to re-estimate our model in specification (12). Even with such perturbed midpoints we find our central results confirmed.

Finally, in specification (13) we add an alternative measure of relative income, which expresses income distance as percentage of the national mean (and thus takes into account income-scale differences between countries). More precisely, our income measure in specification (13) is calculated as the distance between an individual's income and the mean in her country-year expressed as a percentage of the mean in her country-year. This is measured in constant (base year 2005) local currency. We find that our substantive results are the same as the main results reported in the paper, namely that richer individuals are more sensitive to inequality, and that this effect diminishes (or vanishes) when accounting for fear of crime.

Gini from limited sample size The next two robustness tests deal with the fact that we have to calculate our Gini index from limited survey data, in order to have full coverage of all regions. While we already adjust for the fact that our measure of inequality is calculated from relatively small samples (following Deltas 2003), we perform an additional robustness test by dropping the 20% of regions with the smallest sample size in specification (14). We find that our main results are confirmed.

Gini from grouped income data Further problems might arise from the grouped nature of income measurement in the ESS. As we describe in the main text, incomes are asked in brackets, leading to coarse income measurements, which we treated as continuous in our calculation of the Gini coefficient. However, Gini calculations based on grouped data are known to exhibit (some) downward bias (Lerman and Yitzhaki 1989; Davies et al. 2011). To check if this influences our results, specification (15) uses a first-order correction term proposed by Van Ourti and Clarke (2011) to adjust for the fact that we use group data. With this adjustment our main pattern of results is confirmed as well.

Country-specific income concentration It could be argued that the bulk of rich or poor people are concentrated in the wealthiest (or most unequal) countries. Although the transformation of local currencies into PPP-adjusted constant 2005 US dollars mitigates this potential problem (see Figure S.1 on page 3), we verify that the households with the greatest and lowest income differences are in fact not concentrated in a handful of countries.

Table S.7 contains the number and percentages of individuals classified as rich and poor in our analysis (with standard errors in parentheses). It shows most countries to have significant amounts of individuals included in both the poor and rich groups. Confirming Figure S.1, no country seems to concentrate either an overwhelming number of poor or of rich people. Nevertheless, the two countries with the more unusual percentages of rich and poor are Portugal and the UK. The reasons for this are slightly different. In Portugal there is an unusually low number of poor people (we must keep in mind that incomes

Table S.7: Individuals classified as rich and poor in our analysis. Number of cases and percentages (standard errors in parentheses)

	Poor		Rich	
	Percent	N	Percent	N
Austria	7.65	3000	13.65	5348
	(0.13)		(0.17)	
Belgium	6.74	2742	14.76	6009
	(0.12)		(0.18)	
Switzerland	14.39	6551	13.14	5979
	(0.16)		(0.16)	
Germany	10.21	6651	11.49	7481
	(0.12)		(0.12)	
Denmark	7.30	1908	10.14	2651
	(0.16)		(0.19)	
Spain	5.31	2256	14.72	6254
	(0.11)		(0.17)	
Finland	5.90	1387	10.23	2404
	(0.15)		(0.20)	
France	5.76	2473	10.05	4315
	(0.11)		(0.15)	
Great Britain	20.34	10265	18.74	9459
	(0.18)		(0.17)	
Ireland	7.10	1777	14.99	3750
	(0.16)		(0.23)	
Netherlands	11.24	5212	12.00	5563
	(0.15)		(0.15)	
Norway	16.08	6610	12.53	5153
	(0.18)		(0.16)	
Portugal	0.87	411	14.73	6978
	(0.04)		(0.16)	
Sweden	4.01	1771	9.38	4143
	(0.09)		(0.14)	

Note: Calculated based on five multiply imputed data sets.

are measured as PPP-adjusted constant 2005 US dollars). In the UK (a highly unequal country), we have a high number of rich people and a high number of poor people. We therefore conduct an additional robustness test in Table S.5 by dropping simultaneously both countries from our analysis. Specification (16) shows our substantive results remain the same.

We also address this concern in an alternative way by using country specific definitions of "rich" and "poor" and calculating marginal effects of inequality by country. See Section S.7 below for more details.

Sub-sample validation As a final, extensive, robustness check against the presence of unobserved heterogeneity (or omitted variables), specification (17) creates 5 datasets with 20% of all observations deleted at random and re-estimates the model 5 times on these random subsets. The final estimate is the average of these 5 models with standard errors penalized proportional to the variance between each set of estimates. Even under this rather strict permutation-test (which also entails a smaller sample size per subsample), we find our core results confirmed.

Placebo test. We also conducted a placebo analysis. One may argue that the inclusion of any variable measuring political perceptions or beliefs could render the macro effect of inequality insignificant. To check for this possibility we replace our theoretically important variable, fear of crime, with the 'catch-all' ideology variable. We find in specification (18) that, as expected, ideology does shape redistribution preferences. However, unlike in our main models, the effect of inequality is significant and not reduced in magnitude at all, indicating that this alternative political variable does not contribute to explaining the effect of inequality.

S.7 CLASSIFICATION OF POOR AND RICH

For predicted probability and marginal effects calculations in the main text we define Poor and Rich in terms of 10th and 90th percentiles of the overall distribution of incomes in our sample. A high concentration of rich and poor people in particular countries may emerge from this global definition of "poor" and "rich". In Figure S.2, we report results using both a common definition of poor and rich and a country-specific one. Panel (A) shows marginal effects of inequality among the poor (first plot), the rich (second plot), as well as a the difference in marginal effects of poor and rich. The definition of poor and rich is the same as we use in the main text (the 10th and 90th percentile of the overall income distribution). Marginal effects are calculated for each country separately. Panel (B) repeats the same plots (poor, rich, difference) but uses the country-specific income distribution to define the 10th and 90th percentile.

Figure S.2 shows both that the results are substantively the same when using these alternative definitions and that the country-specific marginal effects of inequality on preferences are remarkably similar. The marginal effect of inequality is always lower for the poor than for the rich. In a more systematic test, the difference in marginal effects between rich and poor is always significant (as indicated by the confidence intervals not including zero).

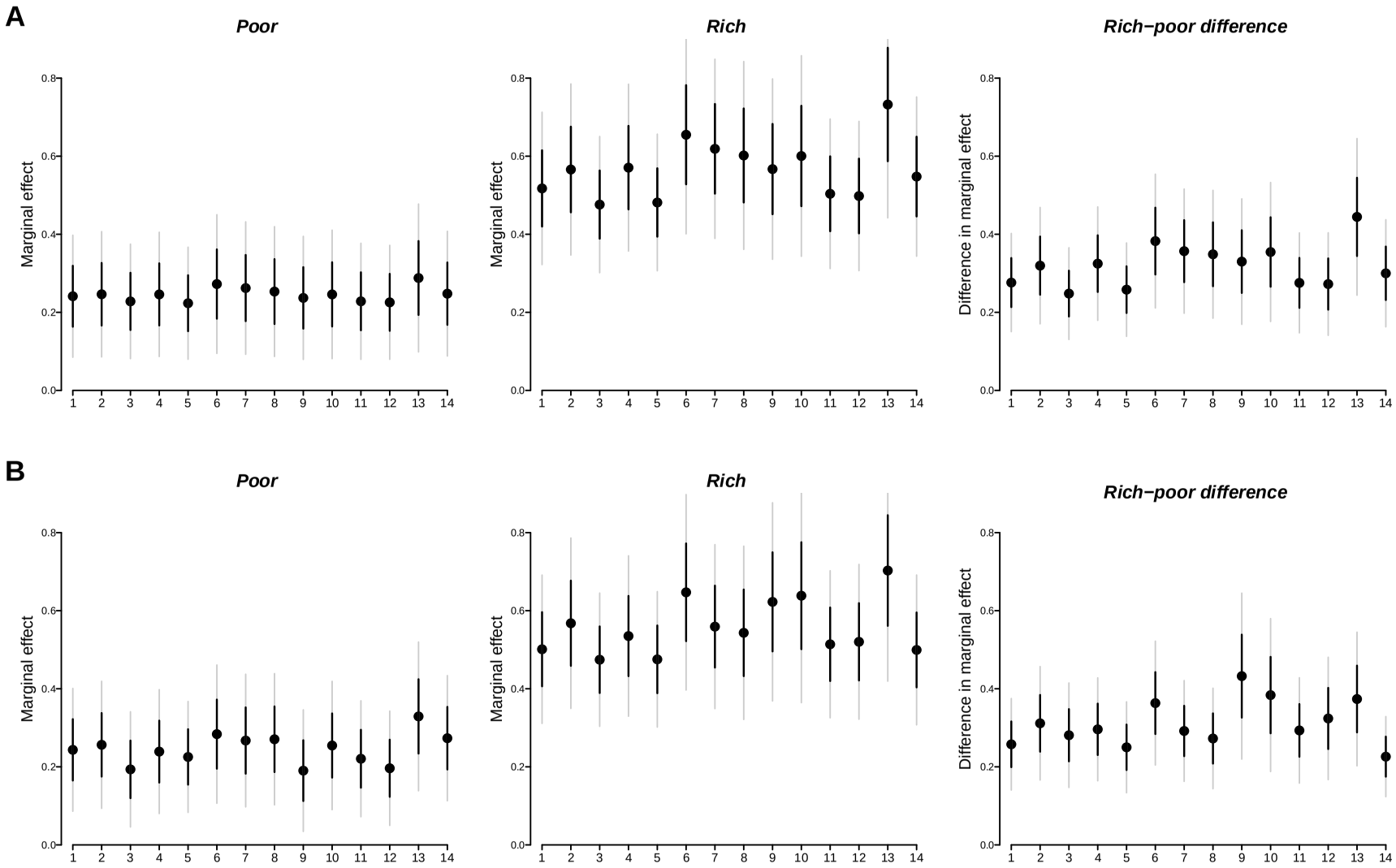

Figure S.2: Effects of inequality on preferences by country. Shown are marginal effects among the Poor, the Rich, as well as the difference between Rich and Poor. Panel (A) uses pooled sample percentiles, Panel (B) uses country specific percentiles.

S.8 FIXED-EFFECTS SPECIFICATIONS

Our main concern when setting up the model in the main text was to account for possible unmodelled intra-country correlation, which is a prime concern when pooling observations. This is fully taken into account by our nonparametric bootstrap procedure, which provides very conservative standard errors and confidence intervals. We also originally opted against using a fixed-effects specification in the main text, since we are using a non-linear, two-equation model, and a "fixed-effects estimator" is not well defined for this model. In addition to estimation complications, it is hard to properly establish population quantities of interest, such as our predicted probabilities or marginal effects (see, for example, the discussion of conditional and unconditional FE estimators in Greene 2004).

However, including fixed effects does provide an additional strong test case for our results since these allow us to control for *time-invariant* confounders (such as second dimension concerns). Table S.8 on the next page shows estimates from the model used in the main text of the paper, followed by three fixed-effects specifications, including year, country, and country and year. Note that, due to the fact that this is a nonlinear model, this is an unconditional fixed-effects model.[9] Including country-specific constants captures unobserved, time-constant country effects, and provides an additional, strict robustness test of our main results.

Starting with panel (A) of Table S.8, which displays estimates for the main variables from our first model including only redistribution preferences, we find our main results confirmed. Our main parameter of interest, the interaction of inequality and income shows that a unit change in inequality still has a marginally higher effect among the rich than among the poor. This effect is statistically significant. We find a slightly stronger effect of income, while the main effect of inequality is still indistinguishable from zero (but now measures deviations from country specific averages). Turning to panel (B), which present our model including fear of crime, we find similar results. Even using a within-country design, we find that in regions with higher levels of inequality, individuals are more afraid of crime. The effect of fear of crime on redistribution preferences is somewhat smaller when accounting for country-specific effects, but it is still statistically and substantially significant.

[9] It is an unconditional fixed effects model, since there is no way to integrate state-specific constants out of the likelihood (Greene 2004), and a "true" fixed effects estimator (such as within estimator, or first-difference estimators in the continuous outcome case) does not exist.

Table S.8: Comparison of model in main text with specifications including country and time fixed effects

	Main Model	Year FE	Country FE	Country year FE
(A) M1: redistribution preferences				
Redistribution: Income	−0.045	−0.449	−0.502	−0.050
	(0.003)	(0.002)	(0.002)	(0.002)
Redistribution: Inequality	1.392	1.415	−0.172	−0.171
	(0.741)	(0.319)	(0.304)	(0.304)
Redistribution: Income×inequality	0.247	0.247	0.234	0.235
	(0.105)	(0.053)	(0.046)	(0.046)
(B) M2: redistribution preferences & fear of crime				
Redistribtion: Income	−0.063	−0.063	−0.062	−0.062
	(0.004)	(0.004)	(0.004)	(0.004)
Redistribtion: Inequality	0.553	0.538	−0.327	−0.334
	(0.798)	(0.361)	(0.320)	(0.320)
Redistribtion: Income×inequality	0.209	0.208	0.210	0.210
	(0.097)	(0.052)	(0.045)	(0.044)
Redistribution: Fear of crime	0.246	0.257	0.173	0.182
	(0.071)	(0.038)	(0.034)	(0.034)
Fear of crime: inequality	4.807	4.809	1.583	1.586
	(0.700)	(0.361)	(0.394)	(0.392)

Note: Control variables not shown to save space. Standard errors are multiple overimputation adjusted to correct for measurement error in Gini.

S.9 INCOME VALIDATION AGAINST EXTERNAL DATA

Like anyone else in American and Comparative Politics, we are constrained by the limited nature of income data in large-scale surveys. For example, the American National Election Studies Time Series file only identifies 5 broad income categories, forcing some researchers (e.g., McCarty et al. 2008) to impute values based on external sources. In contrast, the European Social Survey provides higher quality income measurement (Stoop et al. 2010), following the implementation of the U.S. General Social Survey (GSS). It is this similarity that drove our decision to treat our income variable following the standards established by the GSS (for an extended discussion see Hout 2004).

The quality of the ESS is also acknowledged in the comparative politics literature. Svallfors (2007) concludes that the two "most valuable" cross-national comparative datasets to understand income inequality are the Luxembourg Income Study (LIS) and the ESS. While the LIS depends on post-harmonization of data from national surveys, the European Social Survey maximizes comparability in the original indicators before the surveys are fielded. It does so through an "elaborate procedure for securing cross-national validity of samples, indicators and translations" [p.3]. In other words, within the confines of survey research the ESS provides high quality income measures and combines these with subjective data on preferences.

Nevertheless, in this section we provide the results of two external validation exercises: an aggregate comparison with EU-SILC data maximizing the number of countries and years used in the ESS, and a high-quality income data comparison with German SOEP data. The key difference between the ESS and our two reference sources is that (due to the constraints of general social surveys) the ESS relies on a single income question, whereas specialized household studies use a detailed battery of questions. They separately measure each income component (such as labor income, income from assets, social insurance transfers, etc.). From these income components one can compute total gross household income and subtract all taxes applicable to the household to arrive at an accurate measure of total disposable household income. In the following we describe our two data sources, details of the samples selected and how we construct a net household measure comparable to the one in the ESS. We then present the result of our validation.

German Socio-Economic Panel To validate the distribution of incomes we use data from the German Socio-Economic Panel (SOEP), a longitudinal representative survey of German households. It provides high quality data on individuals' labor market activities, such as income, work experience, and unemployment spells. Information on households and every adult individual living in that household is obtained through face-to-face interviews

held annually (mainly between January and May).[10] Since its inception in 1984, SOEP has been extended by a number of "refreshment" samples, in order to maximize its cross-sectional representativeness. Most notable are the addition of an East German sample and a sample of immigrants. In fact, the SOEP is the largest regular survey of foreigners and immigrants in the Federal Republic of Germany, including households whose head is Turkish, Spanish, Italian, Greek or former Yugoslavian, as well as ethnic Germans from Eastern Europe.[11] Especially attractive for our purpose is an oversample of high income households (SOEP sample G), added in 2002, which provides high-quality data on household at the upper end of the income distribution. These features make SOEP the prime source of accurate information on household income in Germany, providing us with a baseline against which we can judge the adequacy of German income information in the European Social Survey.

We use data on 79,938 households from years 2002 to 2008 in all SOEP samples (A to H) to maximize comparability with our ESS sample. We employ weights to adjust for (i) sample inclusion probability, and (ii) inverse staying probability of households in the sample. This maximizes cross-sectional representativeness of the sample at each point in time.

Our measure of net household income is calculated in three steps. (1) Annual net household income is calculated as the sum of gross total family income from labor earnings, asset flows, private retirement income, private transfers, public transfers, and social security pensions *minus* total family taxes. Table S.9 shows income components used in the SOEP and their exact definitions. (2) Then, we deflate annual net household income to constant 2005 prices using the consumer price index. (3) In the final step, we convert constant annual net household income into purchasing power parity adjusted US Dollars. Using this measure we calculate mean income as well as selected quantiles of the income distribution and compare them to the ESS.

EU Statistics on Income and Living Conditions For our comparison of Western Europe we use the European Union Statistics on Income and Living Conditions (EU-SILC), an annual, EU-wide, household survey with special focus on income, inequality, and poverty (Atkinson and Marlier 2010). EU-SILC is a central source of indicators used by the European Union in its social and economic policy making.

[10] For more information on the SOEP contents and structure see Haisken-DeNew and Frick (2005) and Wagner et al. (2007).

[11] See `http://www.diw.de/en/diw_02.c.299726.en/soep_overview.html`.

Table S.9: Income components in the SOEP

Income component	Definition
Labor Earnings	Wages and salary from all employment including training, self-employment income, bonuses, overtime, and profit-sharing
Asset flows	Income from interest, dividends, and rent
Private transfers	Payments from individuals outside of the household including alimony and child support payments
Public transfers	Housing allowances, child benefits, subsistence assistance from Social Welfare Authority, special circumstances benefits from the Social Welfare Authority, government student assistance, maternity benefits, unemployment benefits, unemployment assistance, and unemployment subsistence allowance
Social security pensions	Payments from old age, disability, and widowhood pension schemes
Family taxes	Income taxes, payroll taxes (health, unemployment, retirement insurance, and nursing home insurance taxes)

Source: Grabka 2012

EU-SILC contains both longitudinal and cross-sectional components. Since our aim is to obtain population statistics for Western Europe, we use the latter.[12] We use information on household incomes for years 2003 to 2008, in order to match the time period of the ESS. This provides us with a sample of over 600,000 households in Western Europe.

Our measure of net household income is calculated in four steps. (1) Annual net household income is calculated as (a) the sum of all household members' income components: employee cash or near cash income; non-cash employee income; cash benefits or losses from self-employment (including royalties); unemployment old-age, survivor, sickness, and disability benefits; education-related allowances; plus (b) income components at the household level: income from rental of a property or land; family/children related allowances; housing allowances; received regular inter-household cash transfers; interests, dividends, profit from capital investments in unincorporated business; income received by persons aged under 16; *minus* regular taxes on wealth; paid regular inter-

[12]The major advantage of using EU-SILC over other databases (such as the Luxembourg Income Study), is that it covers the same countries as in our ESS analysis save for Switzerland. In fact, for wave five of LIS many countries have deposited data based on the European Community Household Panel, a predecessor to EU-SILC, which however suffers from the usual problems of longitudinal panels (mostly selective panel attrition). EU-SILC is superior for our purpose in that it uses both panel and cross-section components.

household cash transfer; taxes on income and social insurance contributions. (2) For countries, which do not use the Euro, EU-SILC data contains values already converted to Euros. We convert them back into national currencies (using annual exchange rates from Eurostat) in order to use national specific price indices and PPP adjustment. (3) Then, we deflate annual net household income to constant 2005 prices using consumer price indices. (4) In the final step, we convert constant annual net household income into purchasing power parities adjusted US Dollars. Using this measure we calculate mean income as well as selected quantiles of the income distribution in Western Europe and compare them to the ESS.

Validation results Table S.10 on the following page contains the results of our calculations. It shows mean income in the ESS, EU-SILC and SOEP together with 95% confidence intervals and selected quantiles of the income distribution.[13]

Focusing on our European comparison first, we find that the means of both income distributions are surprisingly similar. Average income in the ESS is circa 33,900 dollars per year while in the EU-SILC reference data it is 33,400 dollars (both units are in constant 2005 prices and adjusted for purchasing power). The exact difference is 453 dollars, which comes down to a difference in monthly income of 38 dollars. The closeness of the central moment of our two income distributions is further underlined by the fact that their 95% confidence intervals overlap. Even more, mean income in the ESS is in fact within the 95% confidence interval of EU-SILC.

[13] We use Taylor linearization to obtain variance estimates (to create confidence intervals) taking survey-sampling design into account. In case of the SOEP this includes clustering of households within primary sampling units, as well as stratified sampling. In EU-SILC this includes clustering of households within primary sampling units (in countries that do not provide PSUs, households were assigned to a 'catch-all' sampling unit).

Table S.10: Validation of net household income in the European Social Survey against external data. Panel (A) compares our Western European countries with EU Statistics on Income and Living Conditions (EU-SILC). Panel (B) validates our household income in Germany against the German Socio-Economic Panel (SOEP). Table entries are means with 95% confidence intervals, and empirical quantiles. All units are PPP-adjusted US dollars in constant 2005 prices.

(A) Western European data		
	ESS[a]	EU-SILC[b]
Means	33847	33394
	[33648, 34046]	[32711, 34077]
Empirical quantiles		
0.1	9652	10979
0.25	16611	17047
0.5	27384	27633
0.75	42654	42646
0.9	63162	60595
(B) German data		
	ESS	SOEP[c]
Means	34549	33533
	[33997, 35100]	[32984, 34082]
Empirical quantiles		
0.1	10540	11636
0.25	18043	17885
0.5	30656	28081
0.75	39695	42188
0.9	57739	58815

[a] Excludes Switzerland since no corresponding EU-SILC data exists

[b] Household income corrected for within-household nonresponse. Weighted adjusting for sample inclusion probabilities of households. Taylor linearized standard errors correcting for cluster primary sampling units.

[c] Includes oversample of high income earners; data are weighted adjusting for sample inclusion probabilities of households and panel attrition. Taylor linearized standard errors correcting for cluster primary sampling units and stratification.

Looking at the quantiles of the two income distributions we find similar results. The quartiles of both distributions are close. The lower quartiles (0.25) are less than 500 dollars apart, while the upper quartiles (0.75) are almost identical. The modes of both distributions differ by slightly less than 250 dollars (which comes down to 20 dollars a month). Since many of our calculations involve the 10th and 90th percentile, we report them as well. As they are in the tails of the income distribution, we see somewhat larger discrepancies. For the 0.1 quantile the difference is roughly 110 dollars per month, while for the 0.9 quantile it is somewhat over 200 dollars (which of course needs to be put in context of roughly 60,000 dollars of disposable income).

In sum, we find that both distributions – one derived from a general social survey and the other derived from a high-quality, specialized household survey – are remarkably close.

Focusing on Germany, where we have available high-quality panel data with a special emphasis of high-income earners, we find similar results. The means of both distributions are close, with a monthly difference of 85 dollars. While the ESS population mean does not lie within the SOEP confidence intervals, both confidence intervals do overlap (indicating that both samples indicate a comparable population moment). Inspecting the quantiles again yields similar conclusions. The differences in monthly income are comparable in magnitude to the ones we found for Western Europe as a whole, with the largest difference being the 2nd quartile, which is 215 dollars a month higher in the ESS. Of particular interest here is a comparison of the 90th percentiles, since the SOEP takes special care to sample high income earners. The 0.9 quantile in the ESS is at around 57,700 dollars, while in the SOEP it is at 58,800. This amounts to a difference in monthly disposable income between both samples of only 90 dollars. Again, these results confirm Svallfors' (2007) observation that the ESS is a "most valuable" cross-national comparative dataset to conduct analyses of income.

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