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- Strategic Redistribution -
A cross-national investigation of swing voter effects

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Summary

What are the factors that shape the pattern and extent of income redistribution? This study investigates this question empirically. Building on the work of Milanovic (2000), we examine the effects of inequality on redistribution. In addition, we include variables to test the swing voter model. We expect that redistributive policy will favor groups with (1) low party identification and (2) a high proportion of indifferent voters.

While the relationship between inequality and redistribution has been extensively examined, no previous study has tested for swing voter effects using cross-national time-series data. A strength of this study is that it uses extensive microdata to construct the key variables. We integrate income data collected from the Luxembourg Income Study database with public opinion data from the World Value Survey, the Comparative Study of Electoral Systems, and the Eurobarometers. This provides a rigorous test.

We find that increasing income inequality is associated with increasing levels of redistribution, even in the presence of the political variables. This confirms the conclusion of Milanovic (2000).

We also find that swing voters influence income redistribution. This result is novel. Groups that have high levels of party identification tend to do less well than groups that have weak ideological preferences. This suggests that politicians bias policies towards those groups who can be cheaply "bought." This is evidence for politically motivated *strategic redistribution*.

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1. Introduction

A prominent prediction in political economy is that politicians will bias policy in favor of mobile "swing-voters." The swing voters are individuals who are willing to shift their vote as a consequence of particularistic benefits (Dixit and Londregan 1996; Persson and Tabellini 2000). Two distinct swing voter hypotheses are the *greed hypothesis* and the *cut-point density hypothesis*. The greed hypothesis predicts that politicians will favor groups that value consumption relative to ideology. The cut-point density hypothesis predicts that politicians will favor groups in which a marginal change in policy will lead to relatively many individuals switching their vote.

A core research program in political economy is the study of income redistribution[1]. It is surprising therefore that the impact of swing voters on income redistribution has not been examined. Income redistribution is the outcome of a political process and we may expect swing voters to impact the pattern and extent of benefits.

This study tests the proposition that swing voters shape redistribution. It examines the *greed hypothesis* and the *cut-point density hypothesis* using cross-national time-series data. We integrate income data from the Luxembourg Income Study (LIS) data with extensive cross-national political survey data. A strength of our test is its micropolitical foundation.

This study in addition examines the prevailing *inequality hypothesis*. The inequality hypothesis predicts that increasing income inequality will increase redistribution. This is the canonical median voter prediction of Meltzer and Richard.

We use an empirical approach similar to Milanovic (2000) to test the three hypotheses. We find compelling support for two of three:

[1] The convention used in this paper is to use the term "redistribution" to describe transfers of the resources that alter the level of income inequality. This is sometimes referred to as "vertical" redistribution because it involves a transfer of income from those with higher income to those with lower income. This contrasts with "horizontal" redistribution, which refers to situations in which government transfers resources between groups with a similar level of income.

Consistent with the *inequality hypothesis*, we find that an increase in pre-tax and pre-transfer income inequality is associated with an increased extent of redistribution. The size of the effect is large: We find, for instance, that the change associated with a standard deviation increase in the market inequality would make the United States in the period 2000 – 2004 almost as redistributive as Norway in the same period. However, this finding does not provide unambiguous support for the median voter hypothesis: The median voter holds a smaller share of income after redistribution in a meaningful fraction of the countries.

 Consistent with the *greed hypothesis*, we find that groups with a lower proportion of party identifiers tend to gain more via redistribution. Relative lack of party identification among the poor is associated with greater levels of redistribution while, conversely, relative lack of party identification among the wealthy is associated with lower levels of redistribution. This effect is smaller than the inequality effect, but still of meaningful size. A standard deviation change in the greed variable decreases the share of disposable income held by the bottom half of the income distribution by about 1% (0.368 percentage points). This finding corroborates the assertion that politicians are strategic actors who use redistributive policy strategically.

Inconsistent with the *cut-point density hypothesis*, we do not find that groups with more homogenous left-right preferences gain more.

These results are an important empirical contribution. In particular, there have been few rigorous tests of the swing voter model and none using cross-national data—this despite considerable popular and academic attention. The focus on redistribution is also unique. Almost all previous analyses have tested for swing voter effects in geographically defined spending.

The next section briefly presents models of electoral politics and considers their implications for redistribution. Thereafter, we review the empirical evidence. The fourth section focuses on our empirical strategy and our

expectations for the research hypotheses. Section five describes the data and variables in detail while section six is devoted to descriptive statistics. Section seven presents the regression results and section eight provides interpretation and discussion. The final section summarizes and concludes.

2. Theories of electoral competition

The Downsian model of political competition

The bulk of the theoretical and empirical studies of redistribution have been in the Downsian tradition. The essential features of the Downsian model are two-party competition in a unidimensional policy space (Downs 1957). All voters participate and have single-peaked preferences. To win the election, a party must win the median vote. Both parties will therefore propose the policy platform preferred by the median voter. This outcome is deterministic.

The classic prediction with regard to income redistribution was derived by Meltzer and Richard (1981): Modeling individuals as heterogeneous with respect to productivity, and policy as a lump-sum transfer based on a universal rate of taxation, application of the median voter theorem yielded a clear prediction: Redistribution will increase with the degree of market (pre-tax, pre-transfer) income inequality, formally the ratio of mean to median income[2]. The median voter sets the tax rate that maximizes his or her transfer, recognizing that taxation has a disincentive effect.

This logic is intuitive. As the market income distribution becomes more (positively) skewed, the median voter can gain more via taxes and transfers. Conversely, if the income distribution is less skewed, there is less opportunity to transfer income from the wealthy. We refer to this as the *inequality hypothesis*.

The inequality hypothesis

H1: redistribution increases with the market income inequality

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An important second implication is that the median voter will never be a net taxpayer: The median voter will always instead choose a zero tax rate.

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[2] We refer to income prior to taxes and transfers as "market" income because it reflects income earned in the market. Some authors refer to it as "factor" income because it derives directly from factors of production.

Although the *inequality hypothesis* has been the main focus of empirical tests of the median voter theorem, theorists have re-applied the median voter theorem under a variety of different assumptions and derived other predictions. For instance, redistribution is in reality intertemporal. Individuals' expectations about the future may thus shape their voting decisions. For example, Benabou and Ok (2001) and Alesina and La Ferrara (2005) suggest that the possibility of upward movement may be an important factor (the POUM hypothesis): They predict, under certain assumptions, less redistribution than Meltzer and Richard. Another issue is that redistribution often takes the form of social insurance. Moene and Wallerstein (2003) predict that the median voters' preferences for various types of social insurance will depend both on the pattern of benefits and the shape of the income distribution. A specific expectation in their model is that the median voter will favor larger unemployment benefits when society is more egalitarian—the opposite of the original inequality hypothesis.

The swing voter model of electoral competition

An alternative set of models of electoral politics may be described as "probabilistic models" (Persson and Tabellini 2000, 32). As in the Downsian model, two parties compete for voters by proposing economic policy[3]. In contrast to the Downsian model, however, parties and voters are also modeled as having fixed positions on an "ideological" dimension orthogonal to economic policy (Persson and Tabellini 2000, 52). Possible examples of non-economic ideological issues that may influence voting include stance on abortion or stance on gay and lesbian rights. This introduces an element of multidimensionality that is difficult to incorporate in median voter models.

The swing voter model is a particular specification of a probabilistic model (Dixit and Londregan 1996). The swing voter model depends on the additional assumptions of complete vote participation and an equal ability of the parties to redistribute resources. Although swing voter effects may still

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[3] The two party assumption may not, in practice, be a critical assumption: Even in political systems with multi-party competition there will be incentives to attract swing voters by biasing policy in their favor.

be important if these assumptions are violated, in the presence of these assumptions the swing voter outcome is unambiguous.

In the swing voter model, candidates compete for votes by proposing a vector of transfers among groups of voters, subject to a balanced budget constraint. The individual's voting decision is then a function of their ideological preference as well the "particularistic benefits" they receive (Dixit and Londregan, 1133). Individuals have preferences for the parties but may be willing to switch their vote in exchange for personal consumption.

The number of indifferent voters: Cut-point density

In each group of voters there is a distribution of preferences for the parties. Some individuals hold extreme views while others are indifferent between the parties. As Dixit and Londregan state: "*We find Millian conservatives among the poor and socialists among the rich*" (1996, 1137). However, the expected value and variance of the ideological preferences will differ across groups. Some groups tend to be more progressive, some more centrist, and some more conservative. The decisive feature in the political calculus is the number of indifferent voters. We define this formally as the "cut-point density" (CPD)—the density of the probability distribution at the point that divides the supporters of one party from the supporters of the other. Note that this cut-point need not lie at the median of the group or at the median position between the parties. The cut-point is determined by both the ideological position of the parties as well as the prevailing pattern of transfers. If the pattern of transfers is changed, then the cut-point will shift position.

Other factors constant, candidates will favor those groups in which there are many indifferent voters. They favor these groups because a marginal change in the cut-point yields the greatest return. If we consider two groups, one in which the CPD is high and one in which the CPD is low, it will be beneficial for the candidate to propose transfers from the low CPD group to the high CPD group, *ceteris paribus*, because this leads to a net gain in votes. In fact, the candidate should continue to do this up until the marginal transfer from the low to the high group yields not change in total vote share.

The cut-point density hypothesis

H2: A positive relationship between cut-point density and redistribution

Intensity of ideological preferences: The "greed" parameter

Besides the number of indifferent voters, a critical factor is the value that a group places on consumption relative to ideology. We may conceive of this as a measure of the intensity of preferences. Individuals may have a relatively extreme predisposition toward one candidate, yet still be sensitive to transfers. If the ideological intensity is low, voters may be characterized as "greedy" because they are willing to trade their vote for economic benefits. Conversely, some groups are intransigent and unwilling to shift their vote regardless of transfers. Consider voters whose primary electoral issue is abortion. For such voters, the promise of consumption benefits may matter very little.

Applying similar logic as above, candidates will favor greedier groups with benefits. Greedier individuals will be persuaded by smaller transfers and thus be cheaper to convince. Transfers from less greedy groups to more greedy groups will yield a net gain in votes, other factors constant.

The greed hypothesis

H3: A positive relationship between greed and redistribution

In summary, the swing voter model leads us to expect that policy will disproportionately benefit greedy groups with many indifferent voters. This prediction is consistent with our economic intuition. Politicians make the change to policy that gains the largest number of votes and the optimal policy vector satisfies the usual marginal condition.

Voter mobilization

The swing voter model focuses on voter *persuasion*. But another important feature of electoral competition is voter *mobilization* (Cox 2006). If a group is

easy to mobilize or has a greater inherent propensity to vote, this may make them a more attractive target for policy. For instance, in a recent model Jowei Chen (2011) suggests that because the relative costs of voting for poor voters may reduce their vote participation, it may be rational for candidates to prioritize middle-income voters—even though the middle-income voters are less sensitive to consumption benefits. *Mobilization* issues can lead to a "core-supporters" outcome if politicians are able to more effectively "turn out" their own constituents relative to swing voters. This effect may be especially prominent in elections with low turnout.

Swing voters and redistribution

This paper focuses on the impact of swing voters on redistribution. The difficulty of modeling redistribution using the swing voter model is that this model depends on the assumption of orthogonality between economic and ideological preferences. This assumption will rarely hold in practice. Progressive and left wing parties tend to support more redistribution while right wing and conservative parties tend to accept higher levels of inequality. Individuals may support a party precisely because of its stance on redistributive issues. This is why the formal models of swing voters focus on the tactical allocation of *distributive* goods such as infrastructure spending (Dixit and Londregan 1996).

Nevertheless, we argue on qualitative grounds that swing voters may systematically impact the overall level of redistribution. Income redistribution is the outcome of a political process and we see no reason why the incentives to bias policy to the advantage of swing voters should be less simply because this is a "general-interest" issue. If the distribution of swing voters is unequal across the income distribution, we expect swing-voter influence redistribution. For example, if the poor voters are consistently swing voters, then we expect this to manifest itself in more generous welfare policies.

Moreover, although redistribution is often conceived of as a lump sum high-profile issue, it is in reality the consequence of a large number of programs, exemptions, transfers, and taxes. These programs may have qualities of

"particularistic" benefits. These specific programs present an opportunity for politicians to tactically manipulate individual elements in order to gain support from swing voters. In such a case, even the formal swing voter model may be an appropriate characterization of the political process surrounding redistributive issues.

3. Empirical Studies

The inequality hypothesis and the median voter theorem

Milanovic's influential study *The median-voter hypothesis, income inequality, and income redistribution* (2000) is perhaps the best test of the relationship between inequality and redistribution, as well as the best test of the median voter theorem as conceived by Meltzer and Richard. Many earlier studies attempt to test the Meltzer and Richard hypothesis (Perotti 1996; Bassett et al. 1999). But these studies suffer from a variety of methodological problems . A common misspecification is the use of disposable income inequality as a regressor—that is, inequality after taxes and transfers. This is inconsistent with the logic of the Meltzer and Richard model. As Milanovic points out:

> *It is methodologically incorrect to explain people's decisions about their optimal level of taxes and transfers as depending on the distribution that emerges as a consequence of these decisions*" (2000, 370).

In addition, many of the early tests use government spending aggregates. This is an imperfect measure of redistribution because spending may be high but not redistributive. The rich may, for example, pay high taxes but also receive the majority of benefits[4].

Milanovic (2000) resolves these issues by taking advantage of microdata from the Luxembourg Income Study (LIS)[5]. He addresses the misspecification of the independent variables by constructing various measures of inequality based on the income distribution prior to transfers and taxes. These *market income inequality* measures include:

- The market income Gini coefficient (*Gini MI*)
- The share of market income held by the bottom half of the income distribution (*MI Share)*
- And the mean to median ration for the market income distribution (*Mean:Median*).

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[4] In some countries, in particular "corporatist" states like Germany, the magnitude of government benefits is linked to tax contributions. Thus, while spending is high, the degree of redistribution is rather low (Esping-Anderson 1990). In contrast, a state with lower levels of spending may be substantially more redistributive if the taxes collected on the wealthy go entirely to the poor.

[5] This LIS database collects and harmonizes income microdata for a large number of countries: www.lisdatacenter.org

Each of these captures a slightly different feature of inequality[6]. The Gini coefficient is based on the entire income distribution and increases when "overall" income inequality increases. The *MI Share* variable increases when the bottom half of the income distribution increases its share of the market income—but does not reveal changes that occur *within* the bottom or top half of the income distribution. The *Mean:Median* ratio focuses on the skewness in the income distribution but neglects the shape of pattern of inequality below the media.

Milanovic (2000) also proposes a novel measure of redistribution: *Sharegain*. This is a direct measure of redistribution that captures, in relative terms, how well a group does via redistribution.

The Sharegain measure of redistribution: Individuals are ranked according to market income and divided into market income group. *Sharegain* is then defined as the share of disposable income held by an income group *less* their share of market income. For income group *i*:

$$Sharegain_i = \frac{disposable\ income_i}{total\ disposable\ income} - \frac{market\ income_i}{total\ market\ income}$$

This measure of redistribution can be computed for an arbitrary income group. We aggregate our data on the decile level and then use these to construct measures of the *Sharegain* for the bottom half of the income distribution and the middle quintile. We use the convention of listing the decile range from which the measure is computed. For instance, we refer to the *Sharegain* of the bottom half as *Sharegain51* because it is composed of the *Sharegain* for deciles 5 through 1. Similarly, the *Sharegain* for the middle quintile is referred to as *Sharegain65*.

Sharegain captures the combined effect of all of the programs that affect income redistribution.

Regressing the *Sharegain* against a measure of market income inequality then yields a test of the inequality hypothesis. Support for the inequality hypothesis is provided by a ***i*)** positive relationship between *Sharegain* and *Gini MI*, ***ii*)** a positive relationship between *Sharegain* and *Mean:Median*, and ***iii*)** a negative relationship between *Sharegain* and *MI Share*.

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[6] The differences between the inequality measures are potentially important. Mohl and Pamp (2008) find that using the 90:50 ratio of inequality yields different results than use of the Gini coefficient.

Testing the inequality hypothesis: Milanovic estimates a fixed effects model of the following form:

Equation 1

$$Sharegain_{ik} = \alpha_0 + \alpha_1 \cdot Inequality_k + \alpha_2 \cdot Old_k + \sum_k \beta_k \cdot Country$$

Variables are defined for country-year *k* and income group *i* . A control variable *Old* is included to account for demographic effects on redistribution via pensions.

This set-up captures the correct "time-sequence" of voting: Voters earn income, then the political system acts on this income. Individuals base their vote on whether they are better or worse off after taxes and transfers[7].

Milanovic's study finds strong empirical support for the *inequality hypothesis*: Higher market inequality predicts higher redistribution. Our reproduction of Milanovic's study confirms his findings. Abbreviated results are presented below:

Milanovic Redux: We re-analyze Milanovic's findings for all available data (*all countries*); for "established democracies" only (*Estab. Democ.*); and for the country-year observations included in Milanovic's original study (*In Mil*). We include the direct re-estimate to account for the revisions in the datasets.

Table 1		All countries N=178		Estab. Democ. N=144		In Milanovic N =78	
		coeff.	sig.	coeff.	sig.	coeff.	sig.
A. dependent variable *Sharegain51*							
(1)	**MI Gini**	0.377	0.000	0.361	0.000	0.465	0.000
	Pct.Over 65%	0.042	0.369	0.068	0.321	0.126	0.228
(2)	**MI Share**	-0.463	0.000	-0.431	0.000	-0.646	0.000
	Pct.Over 65%	0.268	0.021	0.270	0.066	0.231	0.144
B. dependent variable *Sharegain65*							
(1)	**MI Gini**	0.058	0.000	0.050	0.000	0.055	0.000
	Pct.Over 65%	0.020	0.270	0.030	0.203	-0.001	0.489
(2)	**Mean:Median**	4.088	0.000	3.890	0.000	4.590	0.000
	Pct.Over 65%	0.048	0.040	0.047	0.056	0.045	0.154

We confirm the inequality hypothesis: *MI Gini* and *Mean:Median* exhibit a

[7] Although this logic may also misrepresent the "true" situation because of the incentives that redistribution has for the market inequality. We discuss this below.

positive relationship with *Sharegain,* while *MI Share* exhibits a negative relationship with *Sharegain.* All results are significant at the 1% level and hold across all samples. Our direct re-estimates of Milanovic's findings (the *In Mil* results) are consistently smaller than the original estimates. This reflects revisions to the datasets and probably also choices about data preparation (eg. different choices about top and bottom coding).

The support for the inequality hypothesis is superficially consistent with the Meltzer and Richard prediction. However, Milanovic also finds that the median voter is often a net loser via redistribution[8]: The share of income held by the middle quintile is often smaller after taxes and transfers. Moreover the ratio of mean to median income is positively related to redistribution only when social pensions are treated as redistribution—this relationship disappears when social pensions are treated as market income.

Criticism of the Milanovic (2000)

Despite the strengths of Milanovic's study, it has been criticized on a number of bases. A potential problem is endogeneity in the inequality variables. Although market income is considered exogenous, the market income distribution will itself be shaped by redistribution because taxes and transfers alter the incentives to work (Bergh 2005). Specifically, we expect more generous regimes to "overstate" redistribution because unproductive individuals will have fewer incentives to work thus exacerbating income skewness. This presents a thorny estimation problem that has not been resolved at this point (see Milanovic (2009) for an in-depth discussion of this issue).

Another criticism of Milanovic's study is that its results are driven by transitory effects rather than by actual changes in policy. For instance, a regime may appear more redistributive during an economic downturn even though policy has remained constant.

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[8] Milanovic presents results for the middle class *Share* and *Sharegain* that appear to be incorrect (2000, 407). However, we confirm his assertion, albeit somewhat more weakly: In 22% of cases the median groups are relatively worse off (18% for the established democracies only).

Other studies of the median voter hypothesis

The weak empirical support for the Meltzer and Richard prediction has motivated an enormous number of studies in which the median voter theorem is reapplied in different contexts, under different assumptions, or using different specifications. Some of these studies have found empirical support. Borge and Rattsø (2004) find evidence for the median voter hypothesis on the municipal level; Moene and Wallerstein (2003) find support for the impact of the median voter in social security spending; and Mohl and Pamp (2008) find support for "Director's law" when they examine the dynamics associated with the 90:50 measure of inequality.

Swing voter evidence

In light of the significant attention given to swing voters in the popular press and in the theoretical literature, it is surprising that there have been relatively few rigorous tests of the swing voter hypotheses. The studies that do exist offer mixed support. A primary impression that emerges from a review of this literature is that there are many difficulties involved with testing models of distributive politics.

A number of studies identify political variables as predictors of spending (Wright 1974; Wallis 1986; Schady 2000; Case 2001; Herron and Theodos 2004). Many of these studies are, however, only ambiguously related to the swing voter model. They also use voting behavior to construct the key political variables. Such an approach has a number of weaknesses. We discuss these below.

A handful of more recent studies are more tightly linked to theoretical models. Johansson and Dahlberg (2002), Johansson (2003), and Helland and Sørensen (2009) find support for the cut-point density hypothesis. Helland and Sørensen (2009) also find support for the greed hypothesis. In contrast, Larcinese, Rizzo, and Testa (2006), Larcinese, Snyder and Testa (2006) and Helland, Thorkildson, and Sørensen (2010) do not find support for swing voter effects.

The existing studies of swing voter effects utilize a geographic approach. This approach makes sense because it clearly links a policy outcome to a specific group of voters. The dependent variable typically takes the form of spending, for example in the form of infrastructure projects or grants[9]. The pattern of spending can then be compared against the swing voter characteristics across the various geographic areas.

Constructing appropriate measures of the swing voter variables is the major challenge in testing the swing voter model. Some studies use voting data to measure the percentage of swing voters. This approach "*is clearly problematic, however, since, by assumption, voting decisions are endogenous to the distribution of government funds*" (Larcinese, Snyder, and Testa 2006, 2). Furthermore, Larcinese, Snyder, and Testa suggest that these issues cannot be completely dealt with by use of lagged voting variables: "*Simulations show that the endogeneity of voting data can lead to severely biased estimates*" (2006, 5). Using observed voting behavior to construct swing voter variables is thus a deficient empirical strategy. This approach is also weakly connected to the swing voter theory because it is not linked to the micro level qualities of voters.

An alternative approach is therefore to use survey data to construct direct measures of voter characteristics. Political surveys often attempt to tap into the general characteristics of voters and thereby enable the estimation of the "true" underlying distribution of swing voters. We expect endogeneity issues to be less significant using this approach because the characteristics such as general party identification are independent of the idiosyncratic voting behavior in individual elections[10].

In practice, the use of direct measures of voter identification appears to improve the estimation of swing voter effects. Johansson (2003) employs

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[9] The only exception to the spending approach is Rattsø and Sørensen's study of the impact of public sector swing voters of reform.
[10] Research suggests that political ideology and party affiliation are generally slowly evolving over time. This measure thus also faces issues of endogeneity if the consequences of the voting decision end up shaping future preferences. This issue is discussed briefly in the variables and data section.

factor analysis of Swedish election data to estimate the number of swing

voters. He finds that:

> *When the closeness proxy is used, no statistical significant effects of tactics on the distribution of grants are found, although the effects have the predicted signs. If we instead estimate the number of swing voters directly using election survey data, it is found that municipalities with many swing voters are given larger grants than other municipalities* (Johansson 2001, 5).

Other examples of the studies that utilize survey data include Dahlberg and

Johansson (2002), Larcinese, Snyder, and Testa (2006) and Helland and

Sørensen (2009).

4. Empirical Approach

We investigate swing voter effects on redistribution in a cross-national context. To do so, we employ a potentially counterintuitive approach. Although we exploit cross-national data, we examine the effect on policy *within* not *across* the countries. Sørensen (2010) therefore proposes to examine how the *relative* characteristics of groups within a country affect policy outcomes. This has a natural interpretation in this study: We compare the distribution of swing voters across market income groups.

Relative swing voter variables

The primary results in this paper are based on a comparison of a "poor" and "rich" group. We define these categories as below and above the median income. We also estimate models that compare the median quintile with the other parts of the market income distribution.

We define *relative* swing voter variables as a ratio of the swing-voter characteristics between the income groups. The cut-point density variable takes the following form:

Relative cut-point density: We define our cut-point density variable as a ration of the cut point density in the poor group relative to the rich group:

$$Relative\ cut\ point\ density = \frac{cut\ point\ density_{poor}}{cut\ point\ density_{rich}}$$

We name this variable in terms of the numerator group. Thus, because the poor consist of the bottom five deciles, we refer to that variable as *Relative cut point density 51* (*RCPD51*).

We also construct the same measure for the median income group relative to the other parts of the income distribution (*RCPD65*).

If the *RCPD51* variable is larger than 1, then the cut-point density of the poor is larger than that of the wealthy group. Conversely, if the cut-point density of the poor group is smaller than that of the wealthy group then this variable will be less than 1.

Similarly, we define the "greed" variable as the ratio of the proportion of ideological voters:

Relative greed: We define our cut-point density variable as a ration of the cut point density in the poor group relative to the rich group:

$$Relative\ greed = \frac{greed_{poor}}{greed_{rich}}$$

We use the same naming convention as above: Here, *Relative greed 51*.

If this variable is larger than 1, then the poor group is less greedy (more ideological) than the rich group. If the variable is less than 1, then the poor group is more greedy (less ideological).

Estimation strategy

To test our three key hypotheses, we build on the ***Equation 1***. We add to this specification our relative swing voter variables, two additional controls, and, in some cases, time-period fixed effects[11] [12]:

Full Model Specification: We test our hypotheses using the following general model for income group *i,* country *k*, and time period *t.*

Equation 2

$$Sharegain_{ikt} = \alpha_0 + \alpha_1 \cdot Inequality_{kt} +$$

$$\alpha_2 \cdot Relative\ cut\ point\ density_{ikt} + \alpha_3 \cdot Relative\ greed_{ikt} +$$

$$\alpha_4 \cdot Controls_{kt} + \sum_k \beta_k \cdot Country + \sum_t \gamma_t \cdot Period + \varepsilon_k$$

In all cases we estimate the fixed effects model using dummy variable estimation.

This model enables us to test the theoretical predictions of the swing voter model. The operationalized hypotheses are reviewed below.

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[11] This approach is flexible. *Sharegain* reflects how well an income group does relative to other members of society. As long as the income groups considered in the study are dichotomous, *Sharegain* will reflect transfers of income from one group to the other. Defining *Sharegain* for the numerator group in the swing voter variables then yields a clear interpretation.

[12] The fixed effect model is a specific form of unobserved effects models. Alternatives estimation techniques include the random effects estimator and first differencing technique.

We test the inequality hypothesis using three different measure of inequality.

If inequality predicts redistribution we expect that:

H1' : *Sharegain increases with the Gini coefficient in market income*

H1" : *Sharegain increases with the ratio of mean to median income*

H1''': *Sharegain decreases with the Share of the market income*

If politicians favor groups with greater number of indifferent many voters,

we expect that:

H2: *Sharegain increases with the relative cut-point density.*

If politicians bias policies towards "greedy" groups that are willing to

compromise ideology for material benefits then we expect that:

H3: *Sharegain increases with the relative greed variable[13].*

Note that these hypotheses have quite specific interpretations that depend on

how the variables have been defined. If we switched the numerator and

denominator of the swing voter variables (defining the variables as the ratio

of rich/poor), we would predict the opposite relationship.

Fixed effects regression

In the fixed effects regression, we allow nations to vary with respect to their

intercept by including, either explicitly or implicitly, a set of country dummy

variables (Kennedy 2009, 283). In this study we use the dummy variable

approach, explicitly including dummies for the fixed effects. What we explain

are deviations from the expected level of redistribution within a country,

rather than the differences across countries. The magnitude of transfers is

important only insofar as it represents a deviation from the country (or

country and period) expectation.

This approach accommodates some of the difficult features of a cross-

national analysis. The nation fixed effects subsumes all the time invariant—

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[13] The actual construction of the greed variable is such that values above one reflect less
greedy voters—in our empirical analysis we expect to observe the opposite relationship.

observed and unobserved—characteristics of the country. This mitigates omitted variable bias associated with time constant factors and is particular strength in this context because it accounts for "unobserved" heterogeneity—features such as culture and history that are difficult to quantify but stable over time.

We choose to include time fixed effects in some specifications. These account for period-specific "shocks" that affect all countries—for example, the oil crises of the 1970s. They also account for trends that evolve in parallel across countries—for instance, declining size of family or growth in government spending consequent of increasing income ("Wagner's law"). We average across five year time periods in order to ensure that we have enough observations in each period to ensure variability[14]. Even so, we see in table 2 that in the 1970-74 and 1975-1979 periods, there are only a few observations:

Table 2		
Period	**Observations**	
	All	**Estb. Democ**
70-74	8	8
75-79	10	10
80-84	19	18
85-89	22	19
90-94	31	20
95-99	35	21
00-04	33	21
05-09	21	14

The fixed effects approach has a number of drawbacks. Because the constant factors cancel out, we cannot comment on potentially interesting factors, such as electoral system, that vary across but rarely within countries. Moreover, even for time varying variables, because we examine deviations from the mean within countries, we may neglect stable differences. Consider, for example, Germany and Denmark. In Germany, the poor voters are

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[14] Because there are only a few observation each year, including dummies for each year would "over-fit" the model.

consistently more greedy (this is indicated by a relative greed measure that is less than one; see the detailed description of this variable in the next section). In Denmark the reverse is true and rich are consistently more greedy:
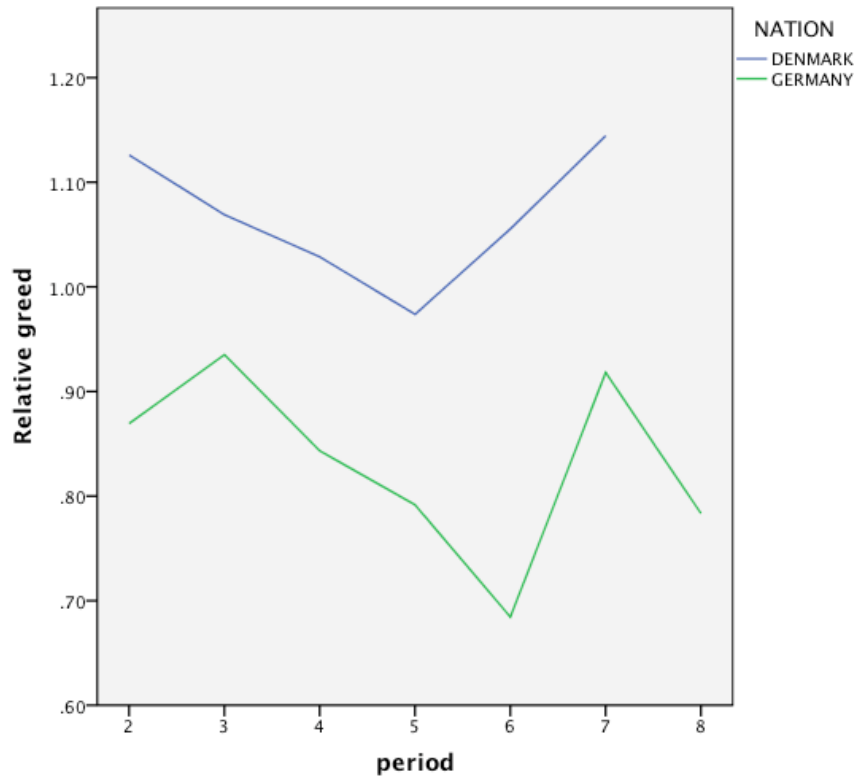
We therefore expect policy to be systematically biased toward the poor in Germany and toward the wealthy in Denmark. However, a weakness of fixed effects regression and our study is that it may not capture the effect of the stable differences because our estimation strategy focuses on variation around the mean (here, about 0.8 for Germany and 1.1 for Denmark). Our estimates are therefore conservative because they focus on marginal changes only.

Another drawback of the fixed effects approach is that it is inefficient. If we include both country and time fixed effects we use up over 40 degrees of freedom. This reduces the amount of variability available to estimate the effects, especially given the relative small sample size.

One way to both exploit the data more efficiently and to capture cross sectional variation is to utilize the random effects estimator. This is a

"feasible" or "estimated" generalized least squares technique. This technique treats the country specific error as a random variable (Peterson 2000, 340). The total error is then a composite of the random country term and an idiosyncratic error.

The random effects approach may not be appropriate here, however, because the validity of the random effects approach depends crucially on independence between country heterogeneity and the independent variables (Wooldridge 2009, 493)—a dubious assumption in our study. For instance, the degree of market income inequality is likely to be systematically related to the country specific error. In contrast to the random effects model, the fixed effects regression accommodates this relationship. So long as the idiosyncratic error is random, the fixed effect regression allows the country effects and the independent variables to be systematically related. This is a strong argument to prefer the fixed effects estimator.

Model specification

Although the fixed effects regression internalizes the time invariant variables, this does not obviate the need for appropriate controls: We must still adjust for factors that vary over time and exhibit relationships with the variables of interest. We must also identify a proper functional form[15].

We introduce controls for the following effects:

Proportion of population eligible for social pensions: The definition of *market income* employed in this study does not include state funded pensions that often have a redistributive character. The magnitude of these payments will therefore depend on the proportion of the population that receives these payments. We control for the eligible population because otherwise the *Sharegain* variable will be larger simply by virtue of a larger population of recipients.

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[15] We initially posit linear relationships across the level variables. However, we test for nonlinearities. We present comment on these results in Appendix C.

Unemployment: Unemployment leads to entry into social programs and the payment of various types of benefits. Changes in the unemployment rate will therefore affect the size of redistribution independent of actual changes in the generosity of the tax and transfer regime.

Preferences for redistribution: We expect that a shift in the left-right preferences in a society will affect the desired level of redistribution. If the median preferences evolve independently across countries, we want to explicitly control for these changing country preferences.

OLS estimation: Assumptions

The fixed effects model can be estimated via a number of methods. However, the OLS estimator applied to the fixed effects model has a number of desirable properties if its assumptions are satisfied. These assumptions include strict exogeneity, homoskedastic errors, and absence of serial correlation (Wooldridge 2009, 482).

Exogeneity: As mentioned above, validity of fixed effects regression depends on the independence of the error term across all periods. This assumption may not hold strictly. A possible issue is feedback from the dependent to the independent variables (i.e. the value of the dependent variable in the present period predicts the error in future periods): If the size of the *Sharegain* impacts the distribution of swing voters in the next election period, then the estimates will be biased[16].

Even so, fixed effects may still be a preferable estimation method. In the presence of violations of strict exogeneity, but in the absence of contemporaneous correlation, the bias in the fixed effects estimator tends to

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[16] Wooldridge (2002) proposes a test for strict exogeneity for one-way fixed effects with balanced panel data. However, we were unable to find a suitable test for *two-way* fixed effects with unbalanced panel data. We therefore do not have an estimate of the significance or magnitude of this problem.

zero at the rate 1/T (Arellano 2003, 85; Wooldridge 2009, 488)[17]. This is a primary reason to prefer the fixed effects estimator to the first difference approach, which otherwise would be a viable alternative.

A different method of addressing exogeneity problems is with instrumental variable estimation. However, justification of an appropriate set of instruments is beyond the scope of this study and will not be pursued here.

Homoskedasticity and Serial Correlation: Both homoskedasticity and serial correlation are possible and may impair the fixed effects estimation. With respect to homoskedasticity, we expect in particular that the Gini coefficient for market income inequality may yield increasing variances. At higher levels of inequality there is more opportunity for redistribution (a similar argument to increasing variance in spending at higher income). We test for these violations of homoskedasticity in our primary regressions by examining residual plots (the "eye-ball test") and using two forms of the Bresuch-Pagan test (see Appendix C).

With respect to serial correlation there is the possibility of both spatial and temporal autocorrelation. We test for autocorrelation using the Durbin Watson statistic under classical assumptions. We also use "Durbin's alternative" test for AR(1) without strictly exogenous regressors described by Wooldridge (2009, 416). Serial correlation does not appear to be a major concern in our primary regressions. It may however be a problem in some of our alternative specifications[18].

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[17] It is not evident how large a "T" is necessary. This result is furthermore derived for balanced panel data—the 1/T result may not hold exactly in these data.
[18] One war to mitigate the effects of heteroskedasticity and serial correlation is to estimate panel-corrected standard errors (Beck and Katz 1995). In the absence of these problems in our primary regressions, we do not, however, compute these statistics.

5. Variables and Data

This study differs from other investigations of the swing voter hypothesis: It does not employ a geographic approach, it examines redistribution rather than spending, and it utilizes cross-national data. In other respects, however, the study hews to the empirical work done by previous studies—albeit extending their approaches to a new context. In particular, we define the key swing voter variables in a similar fashion as Helland and Sørensen (2009). It thus adheres to the prevailing "best practice" in the field. The use of microdata to construct both the measures of redistribution and the political variables is a strength.

Measuring Redistribution: Constructing the Sharegain Variable[19]

The focus of this study is the extent of income redistribution from the wealthy to the poor. *Sharegain* is a natural way of operationalizing this concept. Even so, the researcher must make several decisions about the technical definition. Overlooking the challenges of measurement and comparability—issues that have been somewhat mitigated by the Luxembourg Income Study—two issues are of great importance: The definition of "market income" and the choice made about how to adjust for household economies of scale.

> *Income data preparation*: We prepare the LIS data by dropping observations with missing or zero disposable income. We do this because in some datasets missing values for disposable income were labeled with a zero rather than a missing value. We also drop observations that are missing values for household size.
>
> In contrast to standard practice in preparation of income statistics, we do make any adjustments for extreme values. We do not engage in any bottom- or top-coding, windsoring, or trimming. We thus compute our statistics based on all available data, including the most extreme outliers. This may explain some of the differences between our data and figures publicized in other places (including Milanovic's original figures). For example, we expect our Gini figures to be more extreme than those published else where because we include all outliers.

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[19] It may be useful in this section to refer to the SPSS syntax used to generate the underlying income aggregates. See appendix C.

The Market Income concept

An ideal measure of market income would include all income earned by an individual but no income that is redistributed. Wage, salary, self-employment, and property income clearly inhere in the concept of market income. Other elements are, however, problematic; the extent to which income is earned may not always be clear-cut. The most problematic example is the "social pension." These payments are normally financed via income or payroll taxes and available to all citizen[20]. Even in nations where pensions are indexed to income, old-age payments are rarely actuarial. These payments tend to have a significant redistributive dimension. Including these payments in market income will therefore systematically understate the extent of redistribution while excluding social pensions will overstate the extent of redistribution. The LIS data computed for this study reveals a significant difference: When social pensions are included, the *Sharegain* is 5.29%. When social pensions are excluded, the average *Sharegain* for the bottom half of the income distribution is 9.43%—a 78% larger.

The decision made by Milanovic (2000) and others, and the convention followed in this study, is to exclude social pensions ("old-age and survivor benefits") from the definition of market income, but to include occupational and private pensions. Although both occupational and private pensions may be redistributive (there are often tax incentives or subsidies to buy this coverage), we argue that these pensions are closer to being actuarial; excluding them would greatly overstate the extent of redistribution.

The market income definition therefore includes all wage, salary, and employment income as well as household transfers and non-redistributive transfers (alimony payments), in addition to occupational and private pensions:

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[20] These are referred to as "old-age and survivor" benefits in LIS terminology. In most OECD countries there is a three-tier pension system: The first tier consists of pension payments that are available to all citizens in some form ("social security" in the United States). The second tier consists of occupational pensions that are negotiated as part of the labor contract. The third tier consists of private insurance purchased by individuals. The "old-age and survivor" benefits belong to the first tier.

Market Income (MI): The market income concept measures household income prior to taxes and transfers. This study adopts the specific composition of the variable used by Milanovic (LIS codes in parentheses): Wage and salary income (*V1 or V1net*) + farm self-employment income (*V4*) + non-farm self-employment income (*V5*) + cash property income (*V8*) + private pensions (*V32*) *+* occupational public pensions (*V33*) *+* alimony received (*V34*) + other regular private income (*V35*) (household transfers) *+* other cash income (*V36*). See Appendix A for a technical description of the variables.

Precise descriptions of these variables are provided in Appendix A. Note that a subset of the data—the "net" datasets—use the variable *V1net* rather than *V1*. The *V1net* variable is net of mandatory payroll contributions. We therefore estimate the results for both the full sample and the "non-net" sample.

Disposable income is then defined as *Market income* plus transfers less taxes:

Disposable Income (DPI): Disposable income includes redistributive transfers and taxes (LIS codes in parentheses): *Market Income* + sickness benefits (*V16*) + occupational injury and disease benefits (*V17*) + disability benefits (*V18*) + cash social security benefits for old age or survivors (*V19*) + child and family benefits (*V20*) + unemployment compensation benefits (*V21*) + maternity and other family leave benefits (*V22*) + military/veterans/war benefits (*V23*) + social assistance cash benefits (*V26*) – mandatory contributions for the self-employed (*V7*) – mandatory employee contributions (*V13*) – income taxes (*V11*). See Appendix A for a technical description of the variables.

Equivalized income

We want to rank *individuals* by their market income in order to compute measures of redistribution (and our income inequality figures). This requires an adjustment of *household* market income for the number of household members[21]. However, simply dividing by the number of household members to achieve a per-capita income neglects household economies of scale.

Different institutions use different "household equivalence scales." A common adjustment is to divide the household by the square root of the

---

[21] Two households may have the same income, yet face very different economic realities. We do not, for example, want to rank a household with a single member at the same level as a household of four.

number of household members (this is the approach used by the LIS and, in some contexts, by the OECD). We adopt this approach. We refer to this income concept as *equivalized income*. After weighting each household's *equivalized income* by the household weight multiplied by the number of household members, we can then rank the households according to *equivalized per capita market income.*

Computing sharegain

We use the *equivalized per capita market income* to define income groups by deciles. Households under the 10th percentile of the equivalized market income distribution form the first decile; households with equivalized market income between the 10th and 20th percentile form the second decile, and so on.

After categorizing individuals into income groups the computation of *Sharegain* is straightforward: The share of disposable income held by the MI decile groups, less the market income held by the income group (i.e. the same individuals before and after redistribution). If the *Sharegain* measure is positive then this implies that an income group is better off after taxes and transfers, *relative* to other income groups. To find the *Sharegain* for the poor (below median) or the median groups, we simply aggregate the relevant deciles (5-1 for the poor and 6-5 for the median).

Inequality statistics

We also use the *equivalized per capita market income* to compute our inequality variables. For the *Share51* measure of inequality we compute the total share of the market income held by the bottom five deciles. For the *Mean:Median* ratio we produce the relevant descriptive statistics for each country and divide one by the other. The Gini coefficient is a somewhat more sophisticated calculation. See Appendix D.II for how Gini statistics were computed. As mentioned above, we do not perform any adjustments on the

datasets to account for extreme observations. This may account for some differences with data published elsewhere[22].

Measuring the swing voter characteristics

The key swing voter hypotheses are tested by two variables: A greed parameter and a measure of cut-point density. Data for these variables is gathered from three extensive cross-national surveys: The World Value Survey (WVS), the Comparative Study of Electoral Systems (CSES), and the Mannheim Eurobarometer Trend File, 1970-2002 (EB).

The greed parameter

The greed parameter measures the willingness of a group to compromise ideology for additional consumption. These groups are targets for benefits because they are "cheaper" to persuade, *ceteris paribus*[23]. We assert that an appropriate measure of the greed concept is party identification. This is an appropriate measure of greed because we expect individuals who don't identify with a party to place greater weight on particularistic benefits. In other words, the greedy voters do not have an intense preference for which party is in power.

There is relative unambiguous support for the contention that individuals with strong party attachment tend not to vote across party lines and that they make earlier voting decisions—in other words, individuals who are attached to parties seem to place less emphasis on policy relative to non-party identifiers (Stein 1998; Bartels 2000).

However, the question of preference stability has been a source of greater controversy. This is important because we treat party identification as exogenous. The classic reference is *The American Voter,* which suggests that partisan identification is a stable enduring trait—which, if it is the case—supports our strategy (Campbell, et al. 1960). More recent work complicates

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[22] A direct comparison with Milanovic is impossible because of dataset revisions that have occurred since the publication of his original paper.

this picture and the literature is quite convoluted (Franklin and Jackson 1983; Clarke and McCutcheon 2009).

We define individuals as either "close to a party" or "not close to a party." We utilize a similar, but not identical, party identification variable available across all three of the political surveys:

WVS: Question E178 was asked in the first wave of the World Value Survey:

> *Do you consider yourself to be close or not to any particular party?*

Answers of *close to a party* were assigned the value 1, while the answer of *not close to a party* received a value of 0. Other answers were coded as missing.

Eurobarometer: Eurobarometers (EBs) 4 – 9 posed the following question:

> *Do you consider yourself a supporter to any particular political party? If so do you feel yourself to be very involved in this party, fairly involved or merely a sympathizer?*

It offered the responses, *very involved, fairly involved, merely a sympathizer, no affinities with any party* along with codes for don't know and not applicable. Responses of *very involved* and *fairly involved* were coded as a 1 while responses of *merely a sympathizer* and *no affinity* were coded as a 0.

Beginning with EB10, the question has been posed in a slightly different form:

> *Do you consider yourself to be close to any particular party? <If yes> Do you feel yourself to be very close to this party, fairly close or merely a sympathizer?*

Besides the non-responses, answers included *very close, fairly close, merely a sympathizer* and *close to no particular party. Very close* and *fairly close* were coded as a 1 while *merely a sympathizer* and *close to no particular party* were assigned the value 0.

CSES: The party identification question is phrased identically for both waves, questions A3004 and B3028 respectively:

> *Do you usually think of yourself as close to any particular political party?*

Responses included: *Yes, no, refused, don't know, missing*, and in the first survey, *inconsistent response. Yes* was coded with a 1 while *No* was coded as a 0. All other responses were set as missing values. The inconsistent response was a consequence of individuals either listing themselves as close to a party without identifying the party or identifying a party but listing themselves as not close to a party.

Based on the number of individuals who identify as "close to a party," we can calculate a proportion of party identifiers in each income group. Helland and Sørensen (2009) use a similar approach to calculate the greed parameter in their study. After calculating the proportion of ideological voters in the top and bottom half of the income distribution, we divide the proportion of poor greedy voters by the proportion of rich greedy voters. We refer to this variable as relative close to party (*RCTP*), again defining the variable for the numerator group (that is, either *RCTP51* or *RCTP65*).

A criticism of this operationalization of the greed concept may be that it does not accurately tap into the ideology construct that we are interested in. This criticism may especially be the case if the ideological preference of the voters are closely related to their economic preferences (a violation of the orthogonality assumption). An ideal measure would capture how intensely the individual supports a party in terms of an explicit trade-off such as compensation or willingness to pay.

Another criticism reflects the wording used in these surveys. Closeness to a party may not reflect the "stickiness" concept in the same way as party attachment or as explicit identification as a party supporter might (Barnes et al. 1988). Closeness may also have different connotations in different countries (Sinnott 1998). At the same time, the general "closeness" question may be preferred to other versions of the identification measure because it may tap into the general concept rather than a specific party affiliation (Blais et al. 2001, 5).

Cut-point density: The marginal gain from a shift in policy

In almost all countries, political parties can be arranged along a left-right continuum. This continuum typically represents a ranking from progressive to conservative, with progressives arguing for more social programs and conservatives adopting more laissez-faire policy. However, this continuum also represents preferences for non-economic policy. Given that most voters characterize their preferences—economic and non-economic—on the left-right spectrum, we therefore argue that this self-placement captures the

ideological predisposition of voters. We collect data on self-right placement

based on the following questions:

WVS: Question E033, self-positioning on the political scale, is available in all
waves of the world value survey. Answers are on a ten-point scale, with 1
signifying "left" and 10 signifying "right":

> *In political matters, people talk of "the left" and "the right." How would
> you place your views on this scale, generally speaking?*

Eurobarometer: Every wave of the EB includes a ten-point left-right self-
placement question. Left is assigned the value 1 and right the value 10:

> *In political matter people talk of "the left" and "the right". How would
> you place your views on this scale?*

CSES: The first and second waves use an identical question to assess self-
placement on the left-right spectrum (question A3031 and question B3045):

> *In politics people sometimes talk of left and right. Where would you
> place yourself on a scale from 0 to 10 where 0 means the left and 10
> means the right*?

Whereas the WVS and the Eurobarometer use a ten-point scale (1-10), the
CSES uses an eleven point scale (0-10). To harmonize the data the WVS and
Eurobarometer were converted to the eleven point scale (1=0, 2=1.1,
3=2.2,…, 10=10).

Based on these data, we can construct a discrete distribution of left-right

placement for each country-year-income group.

We would like to use the left-right distribution of voters to estimate the cut-

point density. However, determining the cut-point density depends on

country specific data. In the absence of such data, we use instead the variance

of left-right distribution as a proxy for the cut-point density. We expect that

groups that are more spread out will tend to have a lower cut-point density

and therefore be less attractive for redistribution. We refer to this relative

cut-point density measure as *RCPD51* or *RCPD65*, depending on context.

Linking the income and political datasets

To create the swing voter variables we define income groups that align with
those used to create the *Sharegain* variable. That is, we rank households by
their equivalized per capita market income. To do this we return to the
country specific documentation when available and collect information based
on how the income groups in the original survey were defined. In almost all
cases individuals are asked the appropriate *household* question about *market
income*:

Given the large number of different country-years, we only present a few
examples below:

WVS: In the United States WVS for 2000:

> *Here is a scale of incomes. We would like to know in what group your
> household is, counting all wages, salaries, pensions and other incomes
> that come in. Just give the letter of the group your household falls into,
> before taxes and other deductions.*

Ten categories are available: *Up to 12,500, 12,501 to 20,000, 20,001 to
27,5000, … 75,001 to 100,000, 100,000 or more*.

Eurobarometer: Up to Eurobarometer 36, the following questions was used:

> *Here is an income scale; we would like to know in what group your
> family is counting all wages, salaries, child allowances, pensions and any
> other income that comes in. Just give me the letter of the group your
> household falls into before tax and other deductions.*

In France, for EB2 – EB6, respondents placed themselves into one of 9 income
groups: <250, 250-499, 500-799,…, 4000-6499, ≥6500.

CSES: For Australia in 1996, the following question was posed:

> *What is the gross annual income, before tax or other deductions, for you
> and your family living with you from all sources? Please include any
> pensions and allowances, and income from interest or dividends.*

Respondents were then offered a choice of 14 income categories: *Less than
3000 per year, 3001 to 5000 per year, 8001 to 12000 per year… 60001 to
70000 per year, More than 70000 per year*.

To each individual we assign the mean income associated with their self-
placement. Thus, if an individual from the 1996 Australia income group
identified their household income as belonging to the second income group

we would assign them the mean income in that group, calculated as the average of the sum of the income defining the upper and lower bound, or (5000+3001)/2, in this case. For groups with no lower bound (the lowest income group), we used 0.75 times the upper bound as the groups mean income. For groups with no upper bound (ie. the highest income group), we used 1.25 times the lower bound as the mean income.

Then, to rank households, we divided this income by the square root of the number of members of the household[24]. This is consistent with the equivalence scale used to define the income variables.

This procedure may "mis-rank" individuals near the upper or lower bound of their group. This seems an intractable problem. However, because the primary regression compares two large groups—above and below median income—we expect that only those individuals with the income near the median to be out of order in the market ranking (these ranking issues may be more serious with the median group).

Another issue relates to our definition of market income. In almost all cases individuals are asked to include pensions as market income. However, although our definition of market income includes occupational and private pensions, we excluded social pensions from our market income concept[25]. There will as a consequence be a slight mis-match between the LIS data and the political data. Again, we argue that this will only affect the marginal individuals and may therefore not be a significant effect when we compare the large "poor" and "rich" groups.

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[24] There were some instances where the number of household members was not directly available. In most cases it was still possible to compute a household size from other questions in the survey: For example, by computing "how many individuals under age 18" plus "how many individuals between 19 and 55" plus "how many individuals over age 55".
[25] Specifics information about how households have been interviewed is normally available in the country specific survey documentation. Often there is more information available in these documents than in the overall codebook. For instance, while the Eurobarometer harmonizes the income question to a ten point scale, the data may have been collected on a more extensive scale—up to 20 intervals are used in some cases. It is possible in the datasets to link the individual responses back to these original income categories.

Control variables: We use data on the percent of the population over 65 to control for demographic changes that increase redistribution. These statistics are available from the World Bank *World Development Indicators* (WDI) database. To control for unemployment shocks, we include the percentage of the unemployed, a statistics which we collect from *OECD.stat*.

To account for preferences for redistribution we compute the "true" median for each country using the political survey data on left-right self-placement. A way to compute this figure was proposed by Powell (2000): First we locate the median group (generally the 5th step—so between 4.5 and 5.5). Second, we calculate the percentage of respondents in this group (for example, 25% of the distribution). Third, we subtract the percentage of respondents below the median groups from 0.5 (If 30% of individuals are below the median group, we compute 0.5 – 0.3). Finally we divide the figure from step three by the figure from step two and add this to the lower bound for the range comprising the median group (in our example .2/.25 + 4.5 = 5.3). In the absence of this calculation almost all countries would appear to have identical left-right preferences. A alternative to using this procedure might be to use data on "electoral center of gravity," such as those produced by DeNeve.

6. Descriptive Statistics

Income data for this study are collected from the Luxembourg Income Study (LIS). This database provides remote access to extensive household surveys on income. The major contribution of the LIS project is to "harmonize" income surveys and thereby make them comparable across countries. During the past two decades, the LIS database has become the standard source for comparative research on income. Although the datasets are comparable in most respects, for about 30% of nations, wage income is reported net of payroll taxes.

Political data are collected from three extensive political surveys: The World Value Survey (WVS), the Comparative Study of Electoral Systems (CSES), and the Mannheim Trendfile file (1970-2002) for the Eurobarometer surveys (EB). Together these datasets include over 1.3 million individual unweighted observations.

After combining the income and political datasets and aggregating over five year periods—1970-1974, 1975-1979, 1980-1984, 1985-1989, 1990-1994, 1995-1999, and 2000-2004[26]—the final dataset includes 106 country-year observations (number of observations per country are listed in parentheses): Australia (3), Belgium (3), Brazil (1), Canada (5), Czech Republic (2), Denmark (5), Finland (2), France (8), Germany (8), Greece (2), Hungary (1), Ireland (4), Israel (3), Italy (5), Korea (1), Luxembourg (3), Mexico (5), Netherlands (6), Norway (4), Peru (1), Poland (3), Romania (2), Russia (2), Slovenia (3), Spain (5), Sweden (4), Switzerland(3), Taiwan (4), United Kingdom (6), and the United States (4). Note that because of the fixed effects estimation we exclude all countries for which there is only a single observation available. In addition, when the greed variable is dropped the total size of the dataset increases to 145 observations.

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[26] Use of an alternative set of five-year periods, beginning from 1967-1971, led to a slightly different dataset (for example, Estonia enters). However, this did not appear to meaningfully alter the results.

Bivariate correlations among the variables show high significance among a number of the variables, including between inequality and swing voter statistics and between the key variables and the controls (Appendix B.IV).

We present selected descriptive statistics in table 3. Additional descriptive statistics are included in *Appendix B.I.*

Table 3	Mean	Standard Deviation	Maximum (nation period)		Minimum (nation period)	
a. all nations						
Sharegain51	9.72	1.24	23.35	*Belgium 85-89*	0.34	*Taiwan 75-79*
RCPD51	0.96	0.05	1.11	*Germany 85-89*	0.70	*Canada 75-79*
RCTP51	0.92	0.08	1.19	*Norway 00-04*	0.57	*Slovenia 90-94*
Gini MI	43.96	2.61	58.96	*Brazil 00-04*	27.33	*Taiwan 75-79*
b. established democracies						
Sharegain51	11.14	3.48	23.35	*Belgium 85-89*	1.76	*Korea 00-04*
RCPD51	0.96	0.06	1.11	*Germany 85-89*	0.70	*Canada 75-79*
RCTP51	0.94	0.10	1.19	*Norway 00-04*	0.68	*Germany 90-94*
Gini MI	44.32	4.15	51.35	*Israel 95-99*	33.47	*Korea 00-04*

Variation over time

Since the focus is on variation within countries, we are primarily interested in how the key variables change over time. To illustrate the features of the data we plot the values for key variables for a subset of countries: Denmark, Germany, Italy, Netherlands, Norway, and the United States. We also present box plots over for all observations in Appendix B.II but with an unbalanced panel, time periods may not be directly comparable.

Redistribution

We see in table 3 that the *Sharegain* of the poorer half of the income distribution is on average about ten percentage points. This holds for both the full sample and the established democracies. Broken down by decile, we see the expected pattern. The gain from redistribution diminishes as we move up the market income distribution.

Table 4							
	1	**2**	**3**	**4**	**5**	**Bottom Half**	**Median**
Sharegain	3.47	2.45	1.8	1.16	0.55	9.43	0.53

In this table we also see that the average *Sharegain65* (***Median***) is rather small: About half a percentage point.

Plotting redistribution against time shows that redistribution varies considerably across countries, as well as somewhat within.
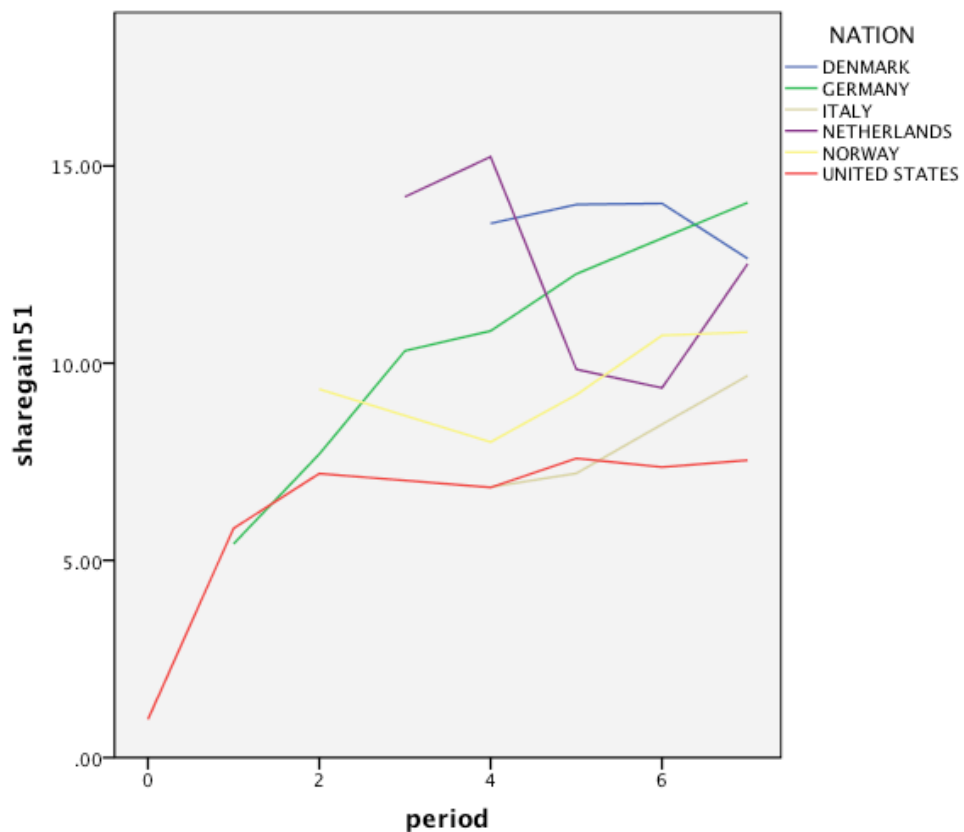
The redistribution captured by the *Sharegain51* variable appears to explain almost all the changes in the overall level of inequality after taxes and transfers: A plot of *Sharegain51* and a plot of the *market income Gini* minus *disposable income Gini* are almost exactly congruent (see Appendix B, plot III.A).

Inequality

The inequality statistics are presented in Table 5. These measures correlate strongly with each, but not perfectly (See Appendix B.IV). Each measure captures a slightly different feature of income inequality.

Table 5						
	mean	stdev.		minimum		maximum
Gini MI	43.96	6.34	27.23	Taiwan 80-84	58.96	Brazil 04-09
Gini DPI	30.08	6.81	18.94	Slovak Republic 85-89	51.60	Mexico 90-94
Mean:Median	1.19	0.16	0.99	Denmark 85-89	1.83	Brazil 04-09
Share51	22.79	3.92	13.19	Russia 00-04	32.10	Taiwan 80-84

The magnitude of redistribution that takes place is substantial. If we compare the average *Gini MI* with the average *Gini DPI* (that is the reduction in overall inequality), we see that slightly more than 30% of the Market income inequality is removed via taxes and transfers.

A clear time trend is the increasing inequality in market income (see also box plot C in Appendix B.II):
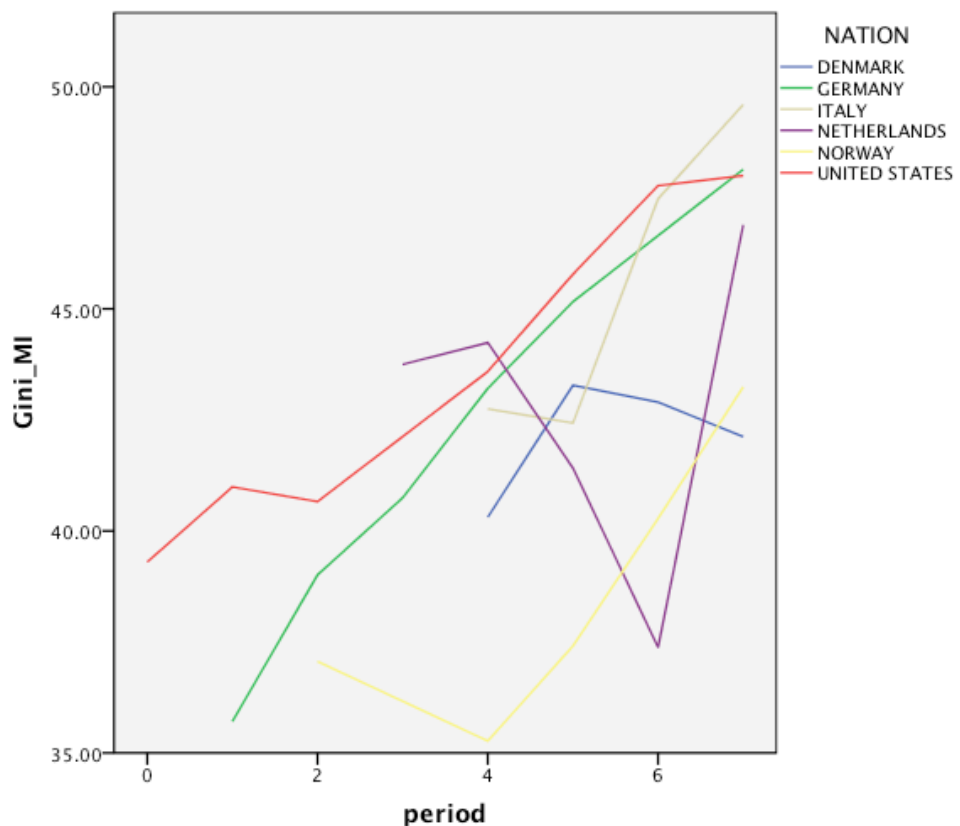
We can only speculate about the factors influencing this trend. One possibility is technological change that rewards skilled laborers. Another possibility might be the decline in the strength of unions. Both these effects might also impact the relative distribution of swing voters.

By comparison to the *market income Gini*, the *disposable income Gini* coefficients have been stable:
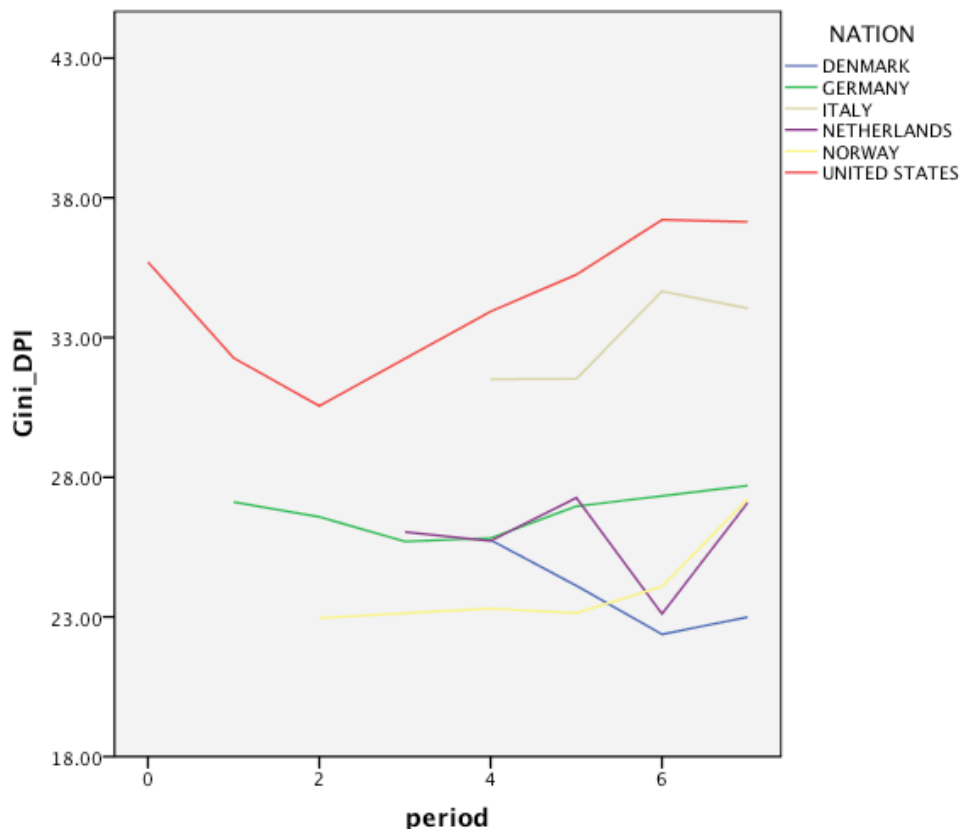
Swing voter variables

In general, the poor are the greedier group. This is indicated by a *relative close to party* variable less than one. This makes the poor more attractive for redistribution. At the same time, the poor are less homogenous, which makes them less attractive for redistribution. That is, they are more dispersed in their preferences as indicated by the *RCPD* variable.

In most countries the relative swing voter variables hover around unity. This indicates that the distribution of swing voters is relatively equal throughout the income distribution. In these countries, swing voter effects may be idiosyncratic. In a handful of countries, however, there appear to be stable

swing voter groups. The prominent example that we identified about is Germany—during the entire time period 1970-2004, the poor have substantially lower party identification. This makes them an attractive group to target with policy.

7. Results

We test the model specified in *Equation 2*. We predict: (1) A positive relationship between inequality and *Sharegain*; (2) a positive relationship between relative cut-point density and *Sharegain*; and (3) a negative relationship between party identification and *Sharegain*. Throughout, parameters have been estimated via dummy variable regressions—this explains why the R^2 are extremely high. We report the coefficient estimate (*coeff.*), the standard error (*se.*), and the significance for a one-tail significance test (*sig.*).

Primary Results

Table 6 presents the primary results, estimated on all available data. The dependent variable is the *Sharegain* for the bottom half of income distribution (the *Sharegain* of decile 5 to 1, hence *Sharegain51*). Both swing voter variables are included and we begin with the full set of controls.

Table 6									
Dependent Variable: *Sharegain51*									
Data: *All Nations*									
	(1)			**(2)**			**(3)**		
	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
RCPD51	0.014	(2.592)	0.50	-0.177	(2.529)	0.47	0.240	(2.395)	0.46
RCTP51	-3.428	(2.342)	0.07	-3.790	(2.250)	0.05	-4.332	(2.102)	0.02
Gini MI	0.431	(0.056)	0.00	0.440	(0.048)	0.00	0.433	(0.044)	0.00
Pct. over 65	0.245	(0.179)	0.09	0.254	(0.172)	0.07	-0.078	(0.130)	0.27
Unemployment	0.024	(0.064)	0.35						
Median	0.249	(0.482)	0.30						
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R^2 (*F*)	0.983 (46.160)			0.983 (49.740)			0.980 (54.026)		
SSERR	1880,767			1880,319			1868,091		
SSREG	66.21			66.65			78.88		
Durbin-Watson	2.21			2.18			2.12		
N	106			106			106		

Two of the three hypotheses find support in regression 6.1: Inequality, as measured by the market income Gini, and greed, as measured by the ideological affiliation of the poor relative to the wealthy, both have the predicted relationship with the magnitude of redistribution. In contrast, we do not find support for the cut-point density hypothesis. The relative cut-point density variable is neither significant nor of meaningful size: A change in the relative dispersion of left-right preferences does not appear to influence the *Sharegain* for the bottom five deciles.

We may give the coefficients from equation 6.1 the following concrete interpretations: When the Gini coefficient for the market income goes up by one point, then the bottom half of the income distribution increases their share of disposable income by 0.431 percentage points on average. In other words, they hold almost a half of a percent more of the disposable income. With respect to the greed variable, consider the situation where the party identification of the top and bottom half of the income distribution are initially identical. If the party identification of the bottom half subsequently increases by 10%, this yields a 0.1 change in the relative greed variable (that is a marginal change of 1.1 – 1.0 = 0.1). This change would be associated with a decrease of -0.343 percentage points of disposable income (0.1 *times* -3.428)[27].

The fit for model 6.1 is reasonable. The residuals follow an approximately normal distribution and tests for homoskedasticity do not lead us to believe that this is a problem in this regression (see Appendix C.I). The *Durbin-Watson* statistic is only slightly indicative of negative serial correlation and is not significant or in the Durbin-Watson "ambiguous region."

In regression 6.1, the control variables *unemployment* and *median* are not significant individually nor jointly; an *F-test* (0.25) suggests that they do not add explanatory power. Re-estimating the model without these control variables yields a simpler model and, we suspect, more precise estimates. In regression 6.2 both the greed variable, *relative close to party*, and the inequality variable, *Gini for market income*, increase in size. The greed variable also exhibits a higher level of significance.

Pursuing the logic that simpler is better, next we re-estimate regression 6.2 without the time effects. This may facilitate a more efficient exploitation of the data because we remove six time period dummies; the inclusion of time dummies may also be "over-fitting" the model, especially in the first two

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[27] Similar reasoning applies to all subsequent regression estimates.

periods[28]. The greed variable again increases in size relative to regression 6.2 and 6.3 and now is significant at the 1% level. The other variables remain approximately the same. The Durbin-Watson statistic drops further (2.12), lessening concerns about the effects of serial correlation.

The drawback of the "simple is better" strategy is, however, that overall model fit suffers. An *F-test* comparing regression 6.3 to 6.2 yields a statistic of 5.735. By comparison, the 1% critical value is 4.904. We confront a dilemma: On the one hand, inclusion of the time effects improves model fit. On the other hand, exclusion of the time effects allows a more efficient use of data and mitigates the serial correlation. A comfort here is that all three regressions are qualitatively similar[29].

Established Democracies

A precondition for observing median-voter or swing-voter effects is a functional democratic process[30]. Autocratic regimes and nations with vote manipulation do not create the same political incentives—in such cases, we may instead expect more patronage or core supporter effects. We therefore choose to re-estimate our regressions on a subset of the data: The established democracies. We include in the list of established democracies Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Israel, Italy, Korea, Luxembourg, Netherlands, Norway, Spain, Sweden, Switzerland, United Kingdom, United States. We only include countries that are established Democracies in all observed periods.

In table 7, we present the results for the established democracies. We follow a similar logic as that pursued for regression 6.1, 6.2, 6.3.

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[28] For instance, if we gave every country-year a dummy, we would have no variance to explain.

[29] We pursue a similar empirical approach to each set of regressions: First, we estimate the model with all controls. Then we examine the fit. If certain controls are insignificant, we remove them and re-estimate the model. Finally, we examine the effects of removing the time period effects. Because we follow the same tactic for all regressions, we do not report the results for each *F-test*, each test for homoskedasticity etc.

Table 7

	(1)			(2)			(3)		
Dependent Variable: *Sharegain51*									
Data: *Established Democracies*									
	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
RCPD51	-1.432	(3.159)	0.33	-1.064	(2.988)	0.36	-0.425	(2.722)	0.44
RCTP51	-5.662	(2.860)	0.03	-4.891	(2.609)	0.03	-5.702	(2.426)	0.01
Gini MI	0.46	(0.070)	0.00	0.466	(0.056)	0.00	0.456	(0.051)	0.00
Pct. over 65	0.207	(0.204)	0.16	0.22	(0.199)	0.14	-1.51	(0.149)	0.16
Unemployment	0.023	(0.080)	0.39						
Median	-0.724	(0.770)	0.18						
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R^2 (*F*)	0.969 (25.847)			0.968 (28.238)			0.962 (33.131)		
SSRESID	903,606			902,539			890,922		
SSREG	59.433			60.499			72.117		
Durbin-Watson	2,209			2.230			2,219		
N	82			82			82		

A remarkable feature of the regressions on the established democracy sample is that the size of the greed effect gets substantially larger while remaining significant at the 5% or 1% levels (comparing regression 7.1 with regression 6.1, the greed variable is nearly 67% larger). Perhaps equally remarkable is the stability of the inequality parameter, which also increases relative to the first set of regressions. Consistent with our expectations, both the inequality and swing voter effects appear to manifest themselves more strongly in democratic states. These estimates are based on a substantially smaller sample, however: Only 82 country-year observations for established democracies compared with 106 in the full sample. The Durbin-Watson statistics also are in the borderline region in which serial correlation may be an issue.

Qualitatively, these regressions support our initial conclusions: Support for the greed and inequality hypotheses but not for the cut-point density hypothesis.

Focus on cut-point density

In order to further examine the effects of the cut-point density hypothesis—which did not find support in regressions 6.1 through 7.3—we estimate a set of regressions excluding the greed variable. This increases the effective sample size to 145. We expect this to lead to a superior estimate of the cut-point density hypothesis because we exploit more of the variance in this variable—although we must be careful to interpret any results in light of the moderate relationship between the two swing voter variables (-0.113 bivariate correlation, significant at the 5% level):

Table 8						
Dependent variable: *Sharegain51*						
Dataset	**All nations**			**Estab. Democracies**		
	(1)			**(2)**		
	coeff.	**se.**	**sig.**	**coeff.**	**se**	**sig.**
RCPD51	-1.044	(2.964)	0.31	-0.714	(3.357)	0.42
RCPD51						
Gini MI	0.337	(0.054)	0.00	0.279	(0.066)	0.00
Pct. over 65	0.131	(0.194)	0.25	0.158	(0.214)	0.23
Unemployment	0.158	(0.067)	0.01	0.179	(0.081)	0.02
Median	0.736	(0.456)	0.06	0.3	(0.709)	0.34
Country effects	yes			yes		
Period effects	yes			yes		
R^2 (*F*)	0.965 (58.717)			0.935 (17.069)		
SSRESID	2524,85			1085,861		
SSREG	188,227			157,049		
Durbin-Watson	1.803			1.567		
N	145			112		

As it turns out, in neither the full dataset or in the established democracy subset does the relative cut-point density variable take on significance; it also displays the incorrect sign. The interesting feature of these regressions may be identified in the control variables. In the absence of the relative greed variable, unemployment is significant and displays the correct sign in all regressions. We speculate that there may be a relationship between unemployment rate and pattern of party identification[31].

Non-net data

An issue in the LIS data is that in a subset of the data the wage income is measured net of payroll taxes. In other words, some taxes have already been removed from our measure of market income. This will overstate the extent of redistribution. We therefore re-estimate our regressions on the subset of data that is correctly defined[32]:

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[31] For instance, could higher unemployment rates lead to lower party identification amongst the poor?

[32] Compared with the primary regressions (table 6), exclude 2 observations from Belgium, 2 from France, 1 from Hungary, 2 from Ireland, 5 from Italy, 3 from Luxembourg, 5 from Mexico, 1 from Poland, 2 from Russia, 3 from Slovenia, and 1 from Spain.

Table 9

	(1)			(2)			(3)		
Dependent Variable: *Sharegain51*									
Data: *non-net country observations*									
	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
RCPD51	-1.505	(3.021)	0.31	-0.523	(.2.818)	0.43	-0.049	(2.766)	0.49
RCTP51	-3.757	(2.572)	0.08	-2.909	(2.471)	0.12	-3.957	(2.421)	0.05
Gini MI	0.415	(0.072)	0.00	0.418	(.072)	0.00	0.381	(0.059)	0.00
Pct. over 65	0.052	(0.264)	0.42						
Unemployment	0.083	(0.081)	0.16	0.079	(.079)	0.16			
Median	-0.775	(0.673)	0.13				0.134	(0.071)	0.03
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R^2 (*F*)	0.983 (35.958)			0.982 (38.594)			0.975 (54.026)		
SSRESID	1071,39			1069,834			1055,234		
SSREG	38.176			39.732			54.332		
Durbin-Watson	2.260			2.233			2.158		
N	74			74			74		

The results for the "non-net" datasets are similar to the previous regressions, with the slight exception of regression 9.2 in which the greed variable is not significant, even at the 10% level. The size and significance of the coefficients fluctuates somewhat compared with other regressions, but this may be a consequence of the small sample size. These estimates do not lead us to question the results obtained in the full dataset. There may be systematic differences for the net datasets, but these do not appear to dramatically alter the effects.

Alternative measures of inequality

Although the results above are convincing, are the results contingent on a specific operationalization of the inequality measure? To test this, we re-estimate the model with two alternative measures of inequality: *Share of market income* and the *ratio of mean to median income* for the market income distribution.

The *share of market income* is defined as the share of the market income held by the first five deciles (*Share51)*. If the poor hold more of the market income, we expect less redistribution and a negative sign for the *Share51* coefficient (i.e. opposite of our expectation for the Gini inequality measure):

Table 10

Dataset	All nations			Established democracies					
	Dependent Variable: *Sharegain51*								
	(1)			(2)			(3)		
	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
RCPD51	-0.321	(3.317)	0.46	-1.816	(4.038)	0.33	-0.786	(3.731)	0.42
RCTP51	-2.938	(2.972)	0.16	-5.06	(3.594)	0.08	-4.788	(3.077)	0.06
Share51	-0.514	(0.137)	0.00	-0.547	(0.158)	0.00	-0.532	(0.124)	0.00
Pct. over 65	0.163	(0.223)	0.23	0.195	(0.250)	0.22			
Unemployment	0.132	(0.078)	0.05	0.152	(0.095)	0.06	0.175	(0.075)	0.01
Median	-0.233	(0.599)	0.35	-1.227	(0.958)	0.10	-0.834	(0.699)	0.12
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R² (*F*)	0.973 (29.002)			0.953 (54.026)			0.949 (49.740)		
SSRESID	1843,67			874,121			866,526		
SSREG	103,302			88,917			96,512		
Durbin-Watson	2.492			2.410			2.365		
N	106			82			82		

In regression 10.1 for the full sample, only *Share51* and *unemployment* are significant. When we limit the sample to the democracies only, however, the importance of the greed effect re-emerges. In regressions 10.2 and 10.3, the *relative close to party* measure is significant and similar in size to the high-end estimates using the Gini measure. For both *Share51* and *RCTP51*, we confirm our expectations.

The *share of market income* variable has a meaningful interpretation. When the poor hold 1 percentage point more of the market income, the poor gain about a half percentage point less via redistribution. Conversely, if the poor hold a percentage point less in market income, they only gain about a half point of the disposable income. That the coefficient is less than unity implies that the poor in more unequal countries remain more poor than those in more equal countries, even after redistribution.

A weakness of the estimates from regressions 10.1, 10.2, and 10.3 are the high Durbin-Watson statistics. This is evidence of meaningful negative autocorrelation. We therefore are critical to the exact predictions in these regressions.

The second alternative measure of inequality is the *ratio of mean to median income*. It is primarily of interest because it is the specific parameter that Meltzer and Richard predict will determine the extent of redistribution if the median voter has determinant preferences. Notably, this measure of inequality does not reveal much about the pattern of inequality in the bottom

half of the population. We predict a positive relationship whereby an increase in the mean market income relative to the median market income increases the extent of redistribution.

Table 11									
Dependent Variable: *Sharegain51*									
Dataset	**All nations**			**Established democracies**					
	(1)			**(2)**			**(3)**		
	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
RCPD51	2.588	(3.510)	0.23	1.429	(4.261)	0.37	5.404	(3.065)	0.04
RCTP51	-0.615	(3.170)	0.42	-2.624	(3.856)	0.25	-1.035	(2.833)	0.36
Mean/Median	8.535	(7.349)	0.13	14.105	(9.200)	0.07	11.99	(5.832)	0.02
Pct. over 65	0.115	(0.244)	0.32	0.162	(0.273)	0.28			
Unemployment	0.206	(0.095)	0.02	0.214	(0.114	0.03	0.232	(0.075)	0.00
Median	-0.022	(0.663)	0.49	-0.870	(1.036)	0.20			
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R2 (F)	0.968 (24.056)			0.944 (13.885)			0.964 (30.281)		
SSRESID	1823,773			857,992			1810.572		
SSREG	123,199			105,047			136.400		
Durbin-Watson	2.413			2.458			2.304		
N	106			82			82		

The mean-median ratio is not as predictive an inequality ratio as the *Share51* or *Gini MI* measures of inequality. In fact, in the regression with the full set of controls and nations, there are no significant findings apart from *unemployment*.

After restricting the estimates to the established democracies, the mean to median ratio turns significant with the correct sign. Then, excluding time fixed effects, in regression 11.3 the cut-point density variable also turns significant with the right sign—but not the greed variable. This is the opposite finding from all previous regressions. Given the idiosyncratic nature of this cut-point density finding, we speculate that this is an incidental finding rather than meaningful support for the cut-point hypothesis.

Of the results presented so far, regressions 11.1, 11.2, and 11.3 are the most difficult to reconcile with the conclusions from the other specifications, especially with respect to the *relative close to party* variable. These regressions imply that after controlling for mean to median income, greed does not impact redistribution.

Alternative dependent variable: Sharegain65

Up to this point, we have focused on redistribution from the wealthy to the poor. However, the model is quite flexible. We therefore examine how inequality and swing voters affect redistribution to other, narrower, segments of the population.

In light of the median-voter theorem, we focus particular attention on the median voter. We test how the redistribution to the middle quintile—the *Sharegain* for the fifth and sixth deciles (hence, *Sharegain65*)—depends on inequality and the swing characteristics defined for the median quintile relative to the other parts of the income distribution (quintile 1,2,4, and 5).

Table 12									
Dependent Variable: *Sharegain65*									
Dataset: *All nations*									
	(1)			**(2)**			**(3)**		
	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
RCPD65	-0.559	(1.278)	0.31	-0.635	(1.097)	0.28	-1.61	(1.083)	0.07
RCTP65	-0.654	(0.765)	0.18	-0.645	(0.682)	0.17	-1.141	(0.633)	0.04
Gini MI	0.049	(0.019)	0.00	0.050	(0.015)	0.00	0.072	(0.013)	0.00
Pct. over 65	0.019	(0.055)	0.35						0.00
Unemployment	0.027	(0.022)	0.07	0.027	(0.017)	0.06	0.001	(0.016)	0.49
Median	-0.082	(0.132)	0.27						
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R2 (F)	0.942 (12.846)			0.942 (13.800)			0.925 (13.575)		
SSRESID	40.299			40.253			38.865		
SSREG	5.098			5.143			6.531		
Durbin-Watson	2.196			2.192			2.275		
N	106			106			106		

In regression 12.1, with the full set of controls, we only find support for the inequality hypothesis. However, this effect is rather small. The effect of inequality on redistribution to the median group is about an order of magnitude smaller than that observed for the both half (for *Sharegain51*). In regression 12.3 we re-estimate the parameters with country-effects only. Both swing variables turn significant. However, the relative cut-point variable has in this case the wrong sign.

Regressions on the established democracies yield a similar set of results:

Table 13

	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
	Dependent Variable: *Sharegain65*								
	Dataset: *Established democracies*								
	(1)			**(2)**			**(3)**		
RCPD65	-0.025	(1.278)	0.31	-0.118	(1.240)	0.46	-1.375	(1.083)	0.13
RCTP65	-0.479	(0.765)	0.18	-0.505	(0.746)	0.25	-1.173	(0.633)	0.04
Gini MI	0.053	(0.016)	0.00	0.052	(0.018)	0.00	0.078	(0.013)	0.00
Pct. over 65	0.002	(0.050)	0.35						
Unemployment	0.017	(0.018)	0.07	0.02	(0.020)	0.17	-0.009	(0.016)	0.30
Median	0.104	(0.132)	0.27						
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R2 (F)	0.922 (9.687)			0.922 (10.719)			0.902 (11.655)		
SSRESID	25.229			25.205			24.109		
SSREG	4.427			4.451			5.547		
Durbin-Watson	2.559			2.540			2.451		
N	82			82			82		

Finally, we turn to perhaps the most direct test of the median voter hypothesis: The redistribution to the median voter dependent on the mean to median measure of inequality.

Table 14

Dataset	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
	Dependent Variable: *Sharegain65*								
	All nations						**Estab. Democracies**		
	(1)			**(2)**			**(3)**		
RCPD65	-0.09	(1.186)	0.47	-0.760	-1.205	0.27	-1.383	(1.353)	0.16
RCTP65	-0.309	(0.740)	0.34	-1.185	-0.698	0.05	-1.207	(0.773)	0.06
Mean/Median	0.558	(1.592)	0.36	3.639	-1.458	0.08	4.597	(1.553)	0.00
Pct. over 65	0.005	(0.053)	0.46	0.080	-0.042	0.03			
Unemployment	0.05	(0.021)	0.01	0.014	-0.018	0.22	0.007	(0.019)	0.36
Median	-0.136	(0.143)	0.17	-0.059	-0.13	0.33			
Country effects	yes			yes			yes		
Period effects	yes			no			no		
R2 (F)	0.933 (10.955)			0.913 (10.442)			0.869 (8.246)		
SSRESID	39.533			37.831			22.378		
SSREG	5.864			7.566			7.278		
Durbin-Watson	2.342			2.335			2.471		
N	106			106			82		

Examining regression 14.1, we find that for the full sample with both country and time-period effects none of the hypotheses find support. However, in the absence of time fixed effects, we find support for both the inequality hypothesis and the greed hypothesis across a variety of specifications and subsets of data. Here, again, we recognize the possible deleterious effects of serial correlation.

Results Summary

The results are summarized in table 15. Coefficients significant at the 10%

level or lower are indicated in bold.

Table 15: Regression Summary								
Regression	**Swing voter variables**				**Inequality measures**			
	RCTP51	**RCPD51**	**RCTP65**	**RCPD65**	**Gini MI**	**Share51**	**Mean/ Median**	
6.1	**-3.428**	0.014			**0.431**			
6.2	**-3.790**	-0.177			**0.440**			
6.3	**-4.332**	0.240			**0.433**			
7.1	**-5.662**	-1.432			**0.460**			
7.2	**-4.891**	-1.064			**0.466**			
7.3	**-5.702**	-0.425			**0.456**			
8.1		-1.044			**0.337**			
8.2		-0.714			**0.279**			
9.1	**-3.757**	-1.505			**0.415**			
9.2	-2.909	-0.503			**0.418**			
9.3	**-3.957**	-0.049			**0.381**			
10.1	-2.938	-0.321				**-0.514**		
10.2	**-5.060**	-1.816				**-0.547**		
10.3	**-4.788**	-0.786				**-0.532**		
11.1	-0.615	2.588					8.535	
11.2	-2.624	1.429					**14.105**	
11.3	-1.035	**5.404**					**11.990**	
12.1			-0.654	-0.559	**0.049**			
12.2			-0.645	-0.635	**0.050**			
12.3			**0.633**	**-1.610**	0.072			
13.1			-0.479	-0.025	**0.053**			
13.2			-0.505	-0.118	**0.052**			
13.3			**-1.173**	-1.375	**0.078**			
14.1			-0.309	-0.090			0.558	
14.2			**-1.185**	-0.760			**3.639**	
14.3			**-1.207**	-1.383			**4.597**	

The summary of the results tells a clear story. Foremost, the inequality effect

is strong across all specifications. It is statistically significant for different

dependent variables, for different operationalizations (*Share51*, *Gini MI*, and

mean to median MI), and across different sets of the data (full, established

democracies, and non-net). Nearly as strong a finding is the evidence for the

greed effect. In the majority of specifications, the relative close to party

measure is significant and of meaningful size. A result worth commenting on

is that the greed effect appears to manifest itself most strongly in the

established democracy subsample. Not only are the largest greed effects

observed for democracies (regressions 7.1, 7.3) but in several instances the

greed variable goes from being insignificant in the full sample to significant in the democracy sub-sample (e.g. regressions 10.2 and 10.3)—and this increase in significance is despite a reduction in sample size from 106 to 82.

In contrast, the cut-point density variable leaves us "scratching our head." This is an area for future research. Despite our theoretical prediction and the empirical findings of other researchers, we find scant evidence for this effect. The RCPD variable is significant only twice, and only once with the correct sign (regression 11.3 has the correct sign, while regression 12.3 has the incorrect sign). These observations may be statistical aberrations. This is not surprising when examining many regressions. With respect to the coefficients for RCPD, they also fluctuate more than for the other key variables. In fact, a simple average of the RCDP coefficients yields an expected effect close to zero. We therefore conclude that our operationalization of the cut-point density concept does not provide any support for this hypothesis.

8. Discussion

This study reveals two unambiguous empirical relationships. The first is a positive relationship between inequality and redistribution. This supports the "inequality hypothesis" and confirms Milanovic's (2000) findings. The second empirical finding is a negative relationship between party identification and redistribution. This supports the greed hypothesis.

Effect magnitude

Both of the empirical relationships are substantial in magnitude. We exemplify this using the estimates from regression 6.1 and regression 7.1. These are estimates from identical models except that for 7.1 the sample was limited to established democracies:

Table 16					
		effect magnitude "between" nations (1 std. dev. change)			
		coeff.	std. Dev.	effect	% of SG
Reg 6.1	**RCTP51**	-3.428	0.046	-0.158	1.638
	Gini MI	0.431	2.610	1.125	11.686
Reg 7.1	**RCTP51**	-5.662	0.065	-0.368	-3.607
	Gini MI	0.460	4.150	1.909	18.710

In the first column we present the estimated coefficients. In column two, we present the standard deviations. We multiply the estimated coefficients with the standard deviation to find the effect on *Sharegain51* of a one standard deviation change in the key variables[33]. A standard deviation increase in the *market income Gini* yields a relative gain of 1.125 percentage points in disposable income. A standard deviation increase in the ideological identification of the poor relative to the wealthy decreases *Sharegain51* by about 0.158 percentage point.

While both of these effects are small in absolute terms, if we compare them to the extent of redistribution (i.e. *Sharegain51*) or to the share of income held by the poor (*Share51*), we see that both inequality and greed explain a significant portion of the overall level of redistribution. The average

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[33] The estimated cofficient (coeff.) multiplied by the standard deviation (std dev.) equals *effect*: For example, -3.428 *times* 0.046 = -0.158).

Sharegain51 in the entire sample is 9.63. If we compare the effect of a standard deviation change in RCTP51 with this number (0.158 divided by 9.63), we see that the magnitude of RCTP51 is equal to 1.638% of the total *Sharegain51;* likewise, the magnitude of the Gini MI effect is equal to 11.686% of the total size of redistribution.

If we focus on the established democracy subsample, these effects are even larger (Table 16, Reg. 7.1). The average size of *Sharegain51* for the established democracies is 10.20 and the greed and inequality variables appear to account for—both in absolute terms and percent-wise—a greater portion of this redistribution than in the full sample: 3.607% and 18.710% for greed and inequality respectively.

However, the comparison across countries neglects an important feature of this analysis: We focus on how changes *within* countries impact redistribution—not changes across countries. We therefore repeat the analysis in table 17, but instead of using the standard deviation across countries, we consider the effects of a change equal to the average *range within* the countries. We define the "range within" in the following fashion: First we calculate the maximum and minimum value for each country. Then we subtract the minimum value of the variable from the maximum to find the range for each country. For example, in Australia the maximum Gini coefficient is 48.09 (observed in 2001) while the minimum Gini coefficient is 40.24 (observed in 1981). The *range within* Australia is then equal to 48.09 *less* 40.24, that is 7.85. Finally, we average the range across all countries to get an average measure of the total variation of the variables during the time periods considered. Conceptually, *range within* captures the effect of moving from the average minimum value of a variable within a country to the average maximum value within a country.

Table 17					
		effects"within" nations (avg. min to max change)			
		coeff.	range	effect	% of SG
Reg 6.1	RCTP51	-3.428	0.180	-0.617	-6.410
	Gini MI	0.431	8.679	3.741	38.858
Reg 7.1	RCTP51	-5.662	0.167	-0.946	-9.267
	Gini MI	0.460	7.486	3.444	33.749

For the full set of nations, the average *range within* for *RCTP51* is 0.180[34]. An increase of this size is associated with a 0.617 point reduction in redistribution (or 6.415% of *Sharegain51*). For the *MI Gini*, the effect of going from the average minimum value to the average maximum—an increase of 8.679 points—is associated with an increase of 3.741 percentage point in disposable income. Relative to *Sharegain51*, this would predict 39% of total redistribution.

The effects of swing voters and inequality: Comparing the US and Norway

Comparing two countries provides an instructive illustration of these findings. Although the redistribution effect is in general larger than the swing voter effect, the relative magnitude of these effects depends on how the key inequality and swing variables change over time within a country.

We present the values for Gini MI for Norway and the United States in two consecutive periods, 1995 – 1999 and 2000 - 2004:

Table 18					
Gini MI					
Country	95-99	00-04	Change	Effect	
Norway	40.280	43.240	2.960	1.3616	
United States	47.775	48.000	0.225	0.1035	

In both countries the market inequality increased slightly during this time period. In Norway the MI inequality increased by 2.96 while in the United States it increased by 0.225. Using the estimates from regression 7.1 (for the democratic subsample), we compute the expected impact of these changes: A 1.3616 increase in *Sharegain51* for Norway and a 0.1035 increase for the United States. Thus we expect the extent of redistribution in Norway to

[34] The average range *across* countries is by comparison 0,613.

increase relative to the United States as a consequence of the increase in market income inequality[35].

Next we consider the greed variable, again looking at the periods 1995-1999 and 2000 – 2004:

Table 19				
RCTP51				
Country	95-99	00-04	Change	Effect
Norway	0.905	1.006	0.101	-0.572
United States	0.807	0.738	-0.068	0.387

In this case, the variables evolve in different directions. In Norway the wealthy become relatively more greedy—a change of 0.101 means that the proportion of party identifiers in the poor group has increased relative to the wealthy. Conversely, in the United States the poor voters are now expected to be the swing constituency because their relative party identification falls by 0.068. As a consequence of the change in the pattern of greed, we expect Norway to redistribute *less* while the United States to redistribute *more.* The effect of swing voters is to decrease the differences between these two countries.

Considering the "net" consequence of both the *Gini MI* and the *RCTP51*, we see that they are countervailing. In fact, if both of these effects were to hold, the *Sharegain51* for the USA and Norway would stay approximately the same for this time period: Although market inequality increases the *Sharegain51* in Norway relative to the United States (by 1.2 percentage points), because the poor are more attractive targets for redistribution in the United States than in Norway, almost all of these gains are reduced (the swing voter effect is about 0.96 in favor of the poor in the United States relative to Norway).

Impact on overall level of inequality

An alternative way to measure the strength of the redistribution and greed effects is to estimate an equation in which the dependent variable is the

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[35] We cannot say anything about the overall or net effect on income inequality from these figures. We take up this question—the extent of redistribution—in the next section.

disposable income Gini coefficient. This provides insight into how much a given change in the swing voters or the inequality variable will impact on overall income inequality[36]:

Table 20									
Dependent Variable: *Disposable income Gini*									
Dataset		*All Nations*					*Estab. Democ*		
	(1)			**(2)**			**(3)**		
	coeff.	se.	sig.	coeff.	se	sig.	coeff.	se.	sig.
RCPD51	3.526	(2.189)	0.14	2.602	(2.916)	0.19	5.874	(3.526)	0.05
RCTP51	3.886	(2.881)	0.09	4.203	(2.548)	0.05	5.377	(2.973)	0.04
Gini MI	0.210	(0.069)	0.00	0.238	(0.059)	0.00	0.259	(0.069)	0.00
Pct. over 65	-0.176	(0.220)	0.21	0.012	(0.153)	0.47	-0.038	(0.174)	0.41
Unemployment	0.062	(0.078)	0.22	0.065	(.066)	0.16	0.038	(0.075)	0.31
Median	-0.314	(0.593)	0.30	0.016	(0.496)	0.49	1.059	0.685)	0.06
Country effects	yes			yes			yes		
Period effects	yes			yes			no		
R2 (F)	0.98 (79.516)			.979 (96.443)			0.939 (36.2570		
SSRESID	4903.163			4897.325			1404.614		
SSREG	100.201			106.039			92.009		
Durbin-Watson	1.743			1.788			1.650		
N	106			106			82		

Our expectations for this regression differ somewhat from the earlier models.

- If the poor become relatively more ideological, we expect that redistribution will decrease and *disposable income Gini* will increase, i.e. a positive coefficient for *RCTP51*.
- However, for the inequality measure we expect a positive number, somewhere between 0 and 1. This indicates that higher market inequality leads to higher disposable income inequality but not by a 1 to 1 ratio. We expect some of the inequality to be reduced via redistribution.

The coefficients can be interpreted directly as the effect of the independent variables on post-tax and post-transfer income inequality. Greed has the expected sign: Based on regression 20.1, when the proportion of poor party identifiers increases by 10% relative to the rich (a 0.1 change), the disposable income inequality increases 0.353 percentage points.

As for the Gini MI variable, a point increase in the market income inequality translates to only a 0.2 point increase in the disposable income. This means that, for the values observed in this study, nearly 80% of the market inequality is removed via taxes and transfers.

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[36] A similar approach would be to estimate the effect of

Relation to the inequality hypothesis and the median voter theorem

This study builds directly on the framework of Milanovic (2000). We confirm almost all of his findings. The *inequality hypothesis* finds strong support, even in the presence of the swing voter variables.

This study does not, however, provide support for the median voter theorem. Although it is superficially consistent with the Meltzer and Richard prediction, the other implications of the median voter model are not borne out. For instance, we expect the median voter to choose an "optimal" level of taxation. We therefore never expect the median voter to be a net payer: The median voter would rather choose zero taxation. We observe, however, that the median quintile gains in only 78.9% of cases (95.5% of the fifth deciles gain, while only 39.33% of the sixth deciles gain[37]). The average magnitude of the *Sharegain65* is also miniscule: Only a 0.0053 increase in the disposable income share held by the median quintile. Furthermore—in perhaps the most direct test of the median voter theorem—the ratio of mean to median income only weakly predicts the extent of redistribution to the median group, gaining significance only in the absence of time-period effects (regressions 9.1, 9.2, and 9,3). In contrast, measures that address the income inequality in the bottom of the income distribution (*Share51* and the market income Gini) are highly significant throughout. It may be the case that the extent of inequality in the bottom half of the income distribution is more important that the shape of the top half of the distribution (such as the mean to median or 90th:50th percentile measures of inequality—although Mohl and Pamp make the opposite observation). While recent applications of the median voter theorem have made different predictions than Meltzer and Richard, it is difficult to comment on these models. An ideal test would also incorporate dynamic features and take into account individual mobility in the income distribution over time.

The only real difference between our findings and Milanovic's results are the consistently smaller estimates of the coefficient on inequality. We do not

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[37] And, because of lower turnout amongst the poor, we may expect the sixth decile to hold the median among people who actually vote.

believe that this is a result of the presence of the swing voter variables. Rather, we suggest three factors that may be important. (1) New nations have entered the dataset. Some of these, such as Korea redistribute very. (2) In the period of time since Milanovic published his study, many of the LIS datasets have been revised. And (3) we may have made different choices about data preparation. In contrast to many researchers we did not make any adjustments for outliers.

The strong confirmation of the inequality finding draws into question studies that suggest that more egalitarian countries redistribute more, not less (Moffitt, Ribar, and Wilhelm 1998; Moene and Wallerstein 2003). However, in fairness, these studies emphasize specific types of welfare spending rather than overall redistribution *per se*. Their conclusions are therefore not directly comparable with the aggregated approach employed in this study. A valuable future contribution would therefore be to refocus on specific spending programs, testing for swing and median voter effects in unemployment benefits and pensions.

Relation to the swing voter literature

We find that groups with relatively high party identification receive less favorable redistributive policy. This supports the greed hypothesis and corroborates the findings of Helland and Sørensen (2009) in a new context. It appears that political incentives may play a role in shaping even general interest politics. Furthermore, the results that we derive do not exploit cross-national variation. Conceivably, swing voter effects could be even larger than those found in this study.

Examining the variation in the relative party identification, we see that there is considerable variation within and across countries. Even so, the poor are a stable swing constituency in some countries (when RCTP51 is less than 1) such as Germany, while the wealthy are the stable swing constituency in

other countries (when RCTP51 is greater than 1) such as Denmark. Although we do not propose an explanation for these differences, this is remarkable[38]:
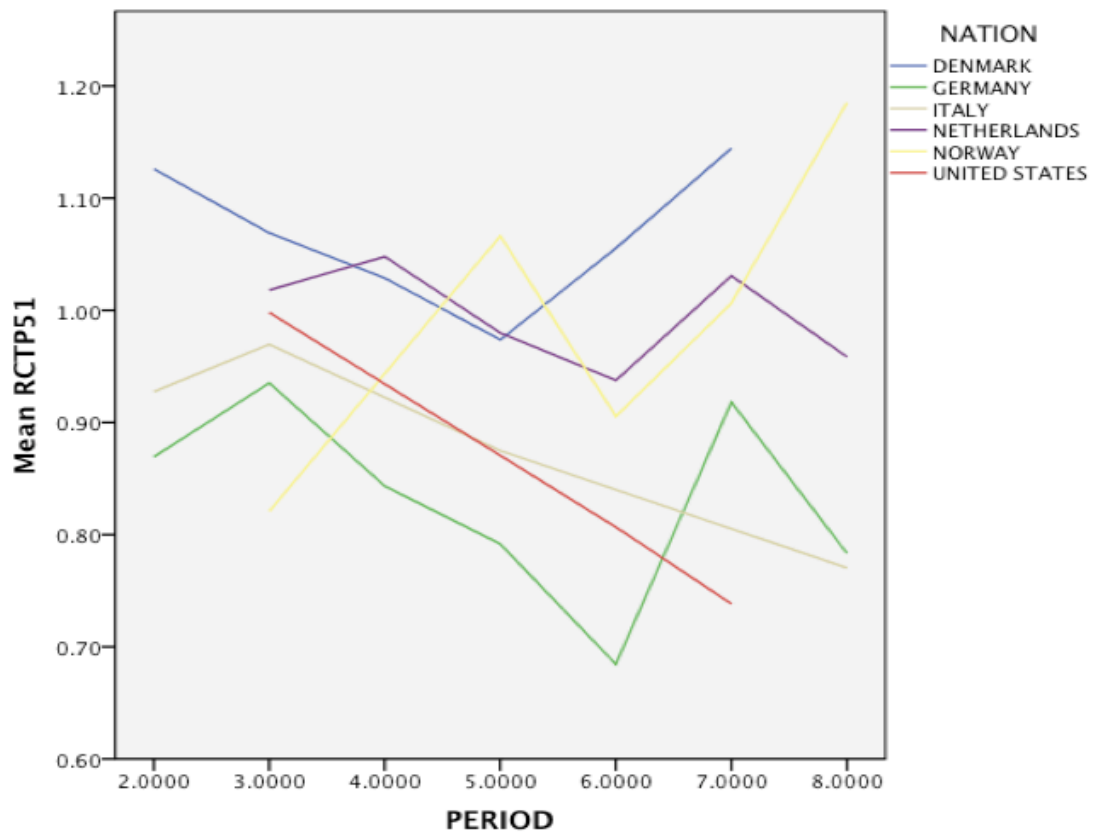
We suspect that in countries like Germany and Denmark that the effects of swing voters will be larger than those found in this study.

Another feature of the data that warrants further explanation is the consistent decline in party identification amongst the poor relative to the wealthy. Beginning around period 3 (1980-1984) and continuing until about period 6 (1995-1999) the poor become more greedy relative to the rich in a large number of countries[39]. Other factors equal, the practical impact of this is that we expect more redistribution to the poor.

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[38] For instance, did Germany unification introduce a large number of poor, unaffiliated East Germans into the political system that then acted as swing voters?

[39] During this period of time we also observe an upward trend in market income inequality. This trend may explain some of the falling party identification for the poor. Lower party identification of the poor may be a consequence of the poor economic performance of the Western countries in 1970s relative to the years of economic expansion that followed World War II.

In contrast to the greed hypothesis, we find no support for the cut-point density hypothesis. This is contrary to the findings of a number of earlier studies (Dahlberg and Johansson 2002; Johansson 2003; Helland and Sørensen 2009). We suspect that the lack of a finding may reflect our operationalization of this concept—not necessarily an absence of the effect.

Future Research

We neglect *mobilization* in our study. Voter turnout is, however, an important feature of electoral politics. For instance, Bartels (2000) finds that while the number of independent voters has increased in the United States, the partisanship of the individuals *who actually vote* has not changed. Accounting for mobilization would improve our estimates and shed light on another electoral incentive, namely vote propensity. We speculate that it may be possible to link respondents by type—perhaps most easily, by age—with a propensity to vote variable derived from voting surveys. Alternatively it may be possible to use variables in the dataset to build an index that predicts voting propensity.

Another area in which it may be possible to improve our study is the cut-point density variable. What we are missing is a measure of election specific cut-point for the individual income groups. This probably introduces substantial measurement error into our estimation because the variances are a crude approximation. This is problematic for our specification because the fixed effects estimator focuses on marginal changes. Finding a valid source of cut-point density estimates would undoubtedly improve our results.

9. Summary

We find support for the swing voter model in the form of the *greed hypothesis*: Groups with lower party identification receive more favorable redistributive policies. This is fascinating. We have uncovered a swing voter effect in the absolute largest aggregate—the overall level of redistribution. This lends support to the existing swing voter studies. It also is testament to the strategic nature of politics at all levels of policy-making. This finding manifests itself most strongly in the established democracy subsample. This seems to suggest that the swing voter hypothesis is an electoral phenomenon.

Besides the greed hypothesis, we also find support for the *inequality hypothesis*. Whether or not this is support for the median voter theorem is ambiguous. The evidence does not appear to support the Meltzer and Richard prediction given that the median voter is often a net payer. However, the results may be consistent with the median voter models as promulgated by others. Expectations and imperfect information are just some of the factors that might lead to lower levels of redistribution than what we would otherwise expect.

The strength of our study is its use of extensive micro data. Even so, there are a number of issues that could be improved upon. In particular, we would like replace the current cut-point density variable with a direct measure of the cut-point concept. Measurement error may have undermined the estimation of the effects of cut-point. It also is worthwhile to re-examine these results using a different estimation strategy. Alternatives to the fixed effects estimator should be considered including instrumental variable estimation.

After considerable technical material, we conclude with a question: Is the support for the *greed hypothesis* consistent with our expectations from "real" life? We argue that it is. Swing voters feature prominently in campaign strategy and an opinion piece titled "*Swing is Still King at the Polls*" from the March 21st, 2006 edition of the *Washington Post* is revealing:

These voters are untethered to either political party. While it's become conventional wisdom to say that voters' minds are firmly made up, and that certain candidates can or cannot win, it's just not true. The growing bloc of swing voters takes a hard look at candidates much later in the process, and they adjust and shift as they gather information. They may seem like wallflowers in the political process right now, but they are the ones a successful campaign eventually needs to cross the finish line.

The author of the piece? None other than Mark Penn, CEO of Burson-Marsteller and former advisor to Hilary Clinton and Bill Clinton, as well as to Tony Blair. We have every reason to expect that politicians will adjust policy to favor swing groups. The evidence of strategic redistribution found in this study should come as no surprise.

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II. The Luxembourg Income Study (LIS)

Luxembourg Income Study Database (LIS), www.lisdatacenter.org (Australia, Austria, Belgium, Brazil, Canada, Colombia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Guatemala, Hungary, Ireland, Israel, Italy, Korea, Luxembourg, Mexico, Netherlands, Norway, Peru, Poland, Romania, Russia, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Taiwan, United Kingdom, United States, Uruguay; {April – June 2011}). Luxembourg: LIS.

III. The Mannheim Eurobarometer Trend File, 1970-2002 (EB)

Schmitt, Hermann, and Evi Scholz. THE MANNHEIM EUROBAROMETER TREND FILE, 1970-2002 [Computer file]. Prepared by Zentralarchiv fur Empirische Sozialforschung. ICPSR04357-v1. Mannheim, Germany: Mannheimer Zentrum fur Europaische Sozialforschung and Zentrum fur Umfragen, Methoden und Analysen [producers], 2005. Cologne, Germany: Zentralarchiv fur Empirische Sozialforschung/Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributors], 2005-12-06. doi:10.3886/ICPSR04357

IV. OECD.Stat: Unemployment rate (stats.oecd.org).

V. World Development Indicators: Persons aged 65 and above, % total populations (http://data.worldbank.org/data-catalog/world-development-indicators)

VI. The World Value Survey (WVS)

WORLD VALUES SURVEY 1981-2008 OFFICIAL AGGREGATE v.20090901, 2009. World Values Survey Association (www.worldvaluessurvey.org). Aggregate File Producer: ASEP/JDS, Madrid.

Appendix A: Variables

The Luxembourg income study provides extensive harmonized income microdata. Data documentation can be found on-line:

http://www.lisdatacenter.org/our-data/lis-database/documentation/

We re-produce the LIS definitions below. All definitions have been collected from the variable definition list:

http://www.lisdatacenter.org/wp-content/uploads/2011/02/variable-definition-list-2011-03.xlsx

(V1) *Cash wage and salary income*: Cash wage and salary income (including employer bonuses, 13th month bonus, etc.), gross of employee social insurance contributions/taxes but net of employer social insurance contributions/taxes. Conscript's pay is also included here.

(V1net) *Net cash wage and salary income*: Cash wage and salary income (including employer bonuses, 13th month bonus, etc.), net of employer and employee social insurance contributions and taxes. Conscript's pay is also included here.

(V4) *Farm self-employment income*: Profit/loss from unincorporated enterprises in the agricultural sector. The income is recorded gross of social insurance contributions (but net of expenses).

(V5) *Non-farm self-employment income*: Profit/loss from unincorporated enterprises not in the agricultural sector. The income is recorded gross of social insurance contributions (but net of expenses).

(V7) *Mandatory contributions for the self-employed*: Mandatory social insurance contributions paid by the self-employed: social security, medical insurance, unemployment, etc.

(V8) *Cash property income*: Cash interest, rent, dividends, annuities, private individual pensions, royalties, etc. Excludes capital gains, lottery winnings, inheritances, insurance settlements, and all other forms of one-off lump sum payments (these are included in V37 and therefore excluded from the DPI).

(V11) *Income taxes*: Taxes on income.

(V13) *Mandatory employee contributions*: Payroll taxes from wage and salary workers for all forms of mandatory social insurance: social security, health plans, unemployment insurance, etc.

(V16) *Sickness benefits*: Income maintenance or support in cash in connection with short-term physical or mental illness of non-occupational

origin, excluding disability. Benefits provided by employers in the form of continued payment of wages and salaries during sickness should be taken into account. Includes:
- paid sick leave (flat-rate or earnings related payments intended to compensate the protected person in full or in part for the loss of earnings caused by temporary inability to work due to non-work related sickness or injury (provided either by autonomous social protection schemes or by the employer in the form of continued payment of wages and salaries during the period of sickness; paid leave in case of sickness or injury of a dependent child or other family dependent is also reported here;
- other sickness cash benefits: miscellaneous payments made to the protected people in connection with sickness or injury (e.g. allowances for intensive care, special bonuses or allowances for tubercolosis patients, rehabilitation benefits, etc.).

(V17) *Occupational injury and disease benefits*: Benefits in the event of incapacity or death caused by an employment-related work injury or occupational illness. Includes short-term sickness cash benefits (that replace in whole or in part loss of earnings during temporary inability to work), and long-term benefits (pensions) in the events of permament disability or death.

(V18) *Disability benefits*: Income maintenance and support in cash in connection with the inability of physically or mentally disabled people to engage in economic and social inactivities, excluding the inabilty due to work-related sickness or injury (see V17).
Includes disability pension, care allowance, benefits for the economic integration of the handicapped and other cash benefits.

(V19) *Cash social security benefits for old age or survivors*: Cash income maintenance and support in connection with old age or death of a family member; first-pillar pension schemes.

In a few cases, the original data made it impossible to separate pension types (e.g., State from non-State, old-age from disability or survivors pensions). In these cases the overall pension amount was recorded here. Where separable, the amounts are recorded in V19S* below.

V19 is one component of PPENSTL in the person-level file (along with V8S3, V17S2, V17SR, V18S1, V18SR, V23, V25S2, V32, V33). It exists as PSOCRET in the person-level file.

(V20) *Child/family benefits*: Cash payments for child or family allowances not relating to maternity/paternity/child care leave.

(V21) *Unemployment compensation benefits*: Full or partial unemployment insurance benefits, vocational training benefits, relocation benefits, and other benefits from unemployment insurance. Excluded are unemployment assistance benefits, redundancy compensation (capital sums paid to employees who have been dismissed through no fault of their own by an enterprise that is ceasing or cutting down its activities), and early retirement due to long-term unemployment into old-age (even if paid by the

unemployment insurance).

(V22) *Maternity and other family leave benefits*: Cash payments for maternity, paternity or child-care.

(V23) *Military/veterans/war benefits*: Cash veteran's benefit or military benefits for old age, military disability, war separations, etc., including cash benefits provided to dependents of the military, as long as they are not means-tested.

(V26) *Social assistance cash benefits*: All forms of transfers that are, IN A STRICT SENSE, in-kind payments (i.e. they are tied to a specific requirement such as school attendance) but have a cash equivalent value equal or nearly equal to the market value, including near-cash housing benefits.

(V32) *Private pensions*: Second pillar in a 3-tiered pension system. These are usually employer payments for retirement that supplement social security transfers. Self-employment pension plans (or, more generally, personal pension plans) are included if they are designed to supplement social security (e.g. individual retirement accounts (IRAs)).

(V33) *Occupational public pensions*: Old age, survivors' and disability pensions for public sector employees, supplementary to the social retirement pension.

(V34) *Alimony received*: Alimony and/or child support received from non-household members.

(V35) *Regular cash*: Regular cash private transfers

(V36) *Other cash income*: All cash income that could not be classified in one of the previous cash income variables. Often referred to in the original survey as other income.

Appendix B: Data and descriptive statistics

I. Descriptive statistics

A.1 Overall descriptive statistics

	Sharegain51	Sharegain65	RCPD51	RCPD65	RCTP51	RCTP65	Gini MI	Pct Over 65	Unemploy	Median
min.	0.34	-1.34	0.70	0.86	0.57	0.83	27.23	4.03	0.40	3.13
max.	23.35	3.96	1.11	1.24	3.00	1.18	58.96	19.82	19.92	7.27
mean	9.72	0.54	0.96	1.02	0.94	0.99	43.96	12.81	7.15	4.38
stdev.	4.44	0.77	0.07	0.06	0.23	0.08	6.34	3.41	3.86	0.52

A.2 Established democracies descriptive statistics

	Sharegain51	Sharegain65	RCPD51	RCPD65	RCTP51	RCTP65	Gini MI	Pct Over 65	Unemploy	Median
min.	1.76	-0.90	0.70	0.89	0.68	0.85	33.47	7.99	2.18	3.13
max.	23.35	2.08	1.11	1.24	1.19	1.18	51.35	19.82	19.92	5.23
mean	11.14	0.43	0.96	1.02	0.94	1.00	44.32	14.11	7.68	4.32
stdev.	3.46	0.59	0.06	0.05	0.10	0.07	4.15	2.21	3.81	0.45

II. Box plots for key variables

A. Cut point density of poor relative to rich
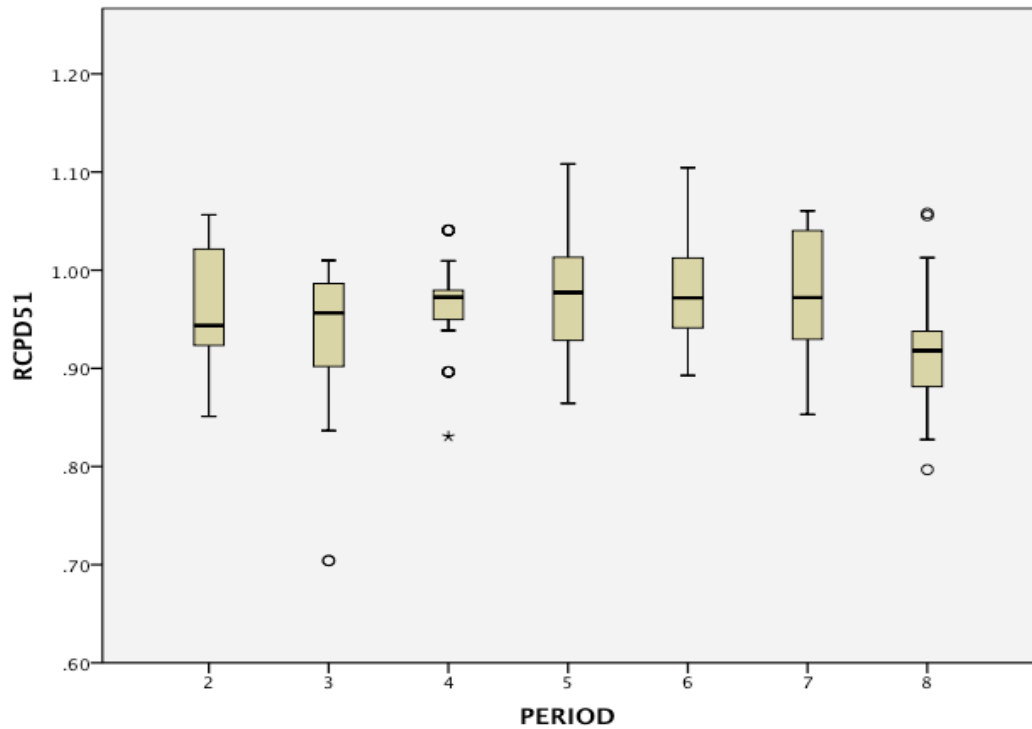
B. Relative close to party for poor relative to rich
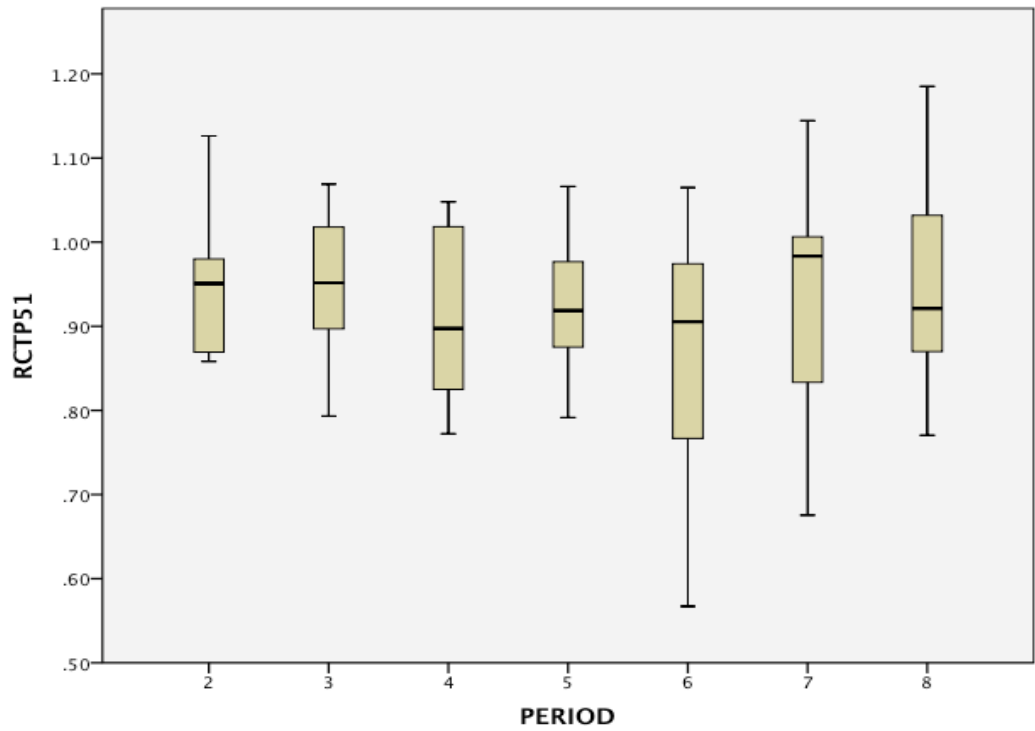
C. Market income Gini
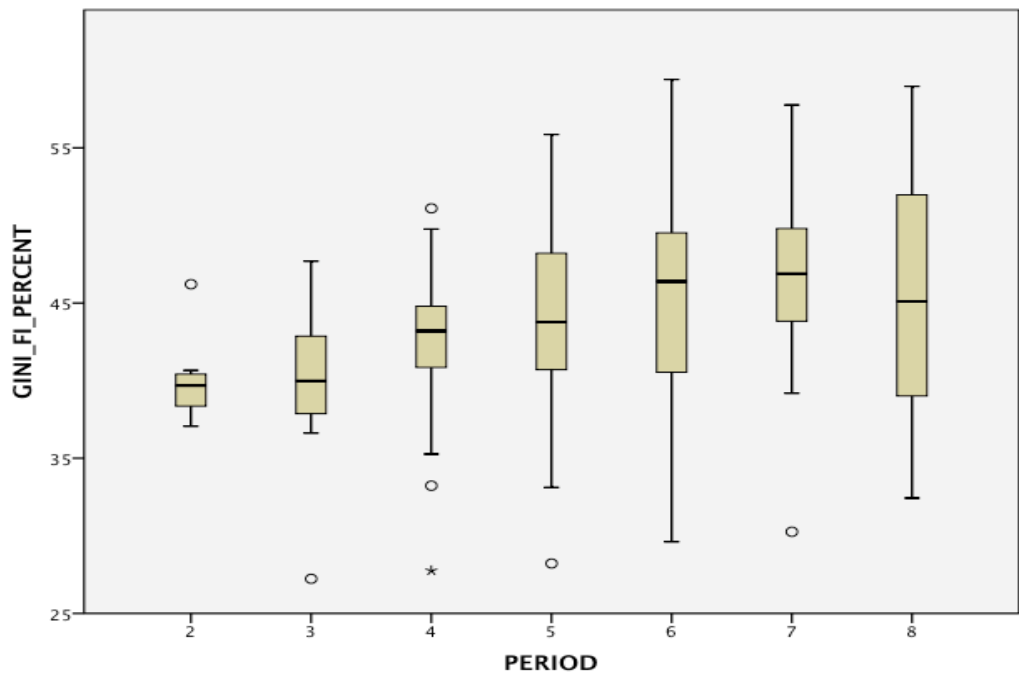
D. Disposable income Gini
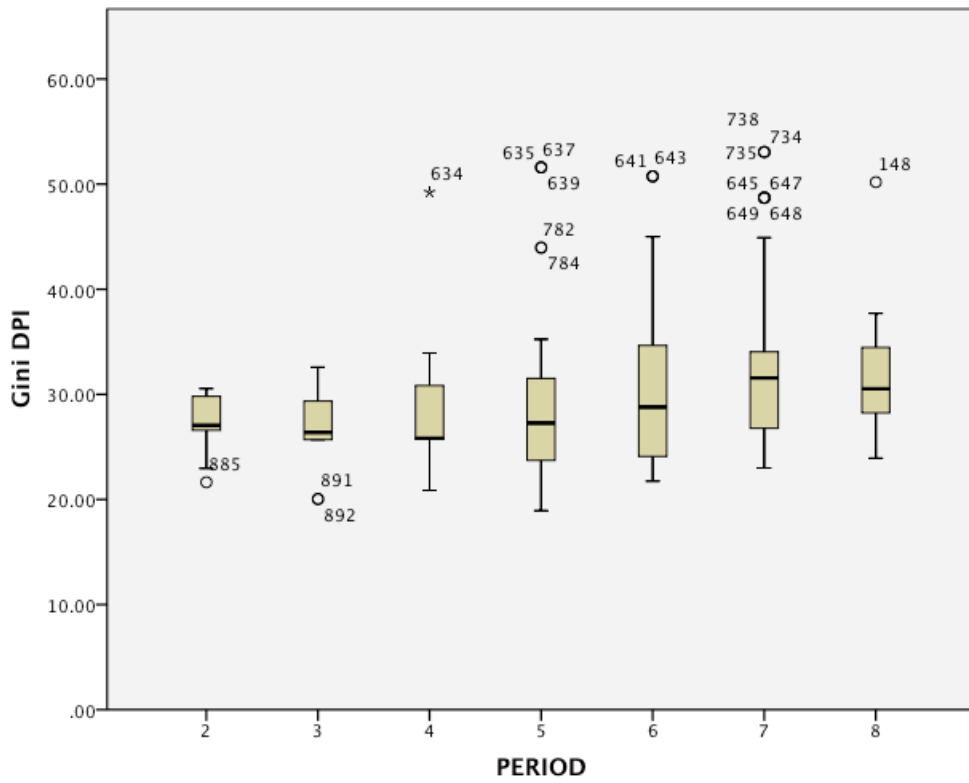
III. Selected plots

A. Reduction in inequality: Market income Gini *less* Disposable income Gini:
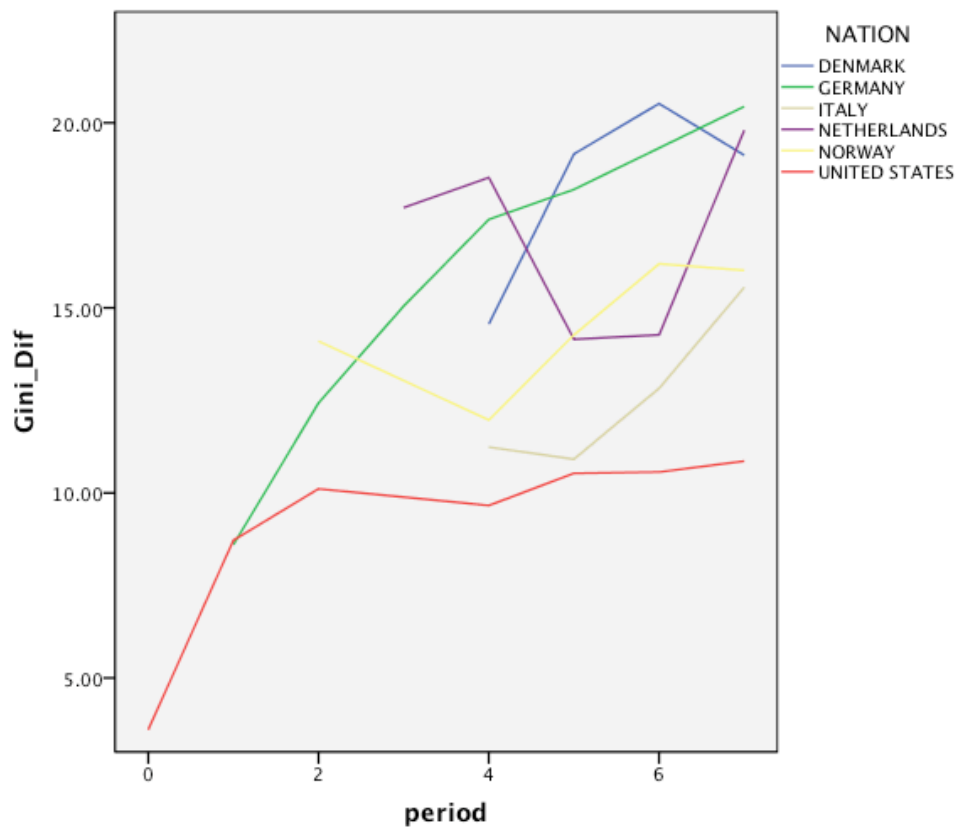
B. Relative cut point density for bottom half (all nations)
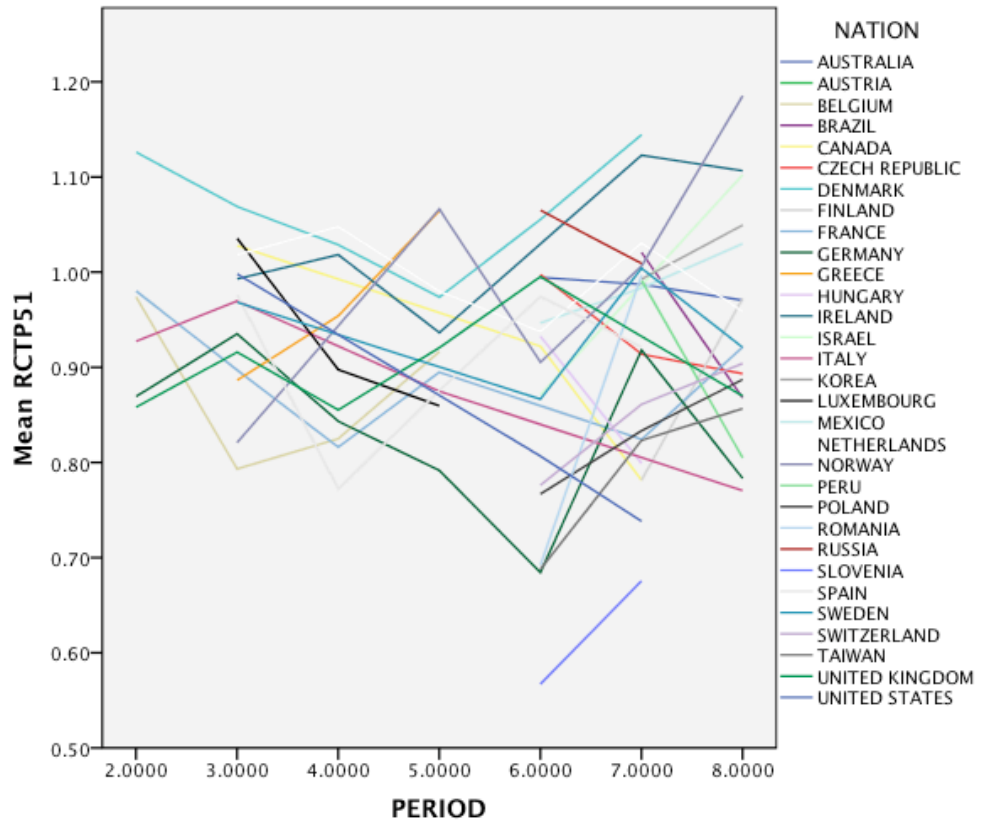
IV. Bivariate correlations

A. Bivariate correlations for inequality measures

		Sharegain51	Share51	Mean:Median
Gini MI	Correlation	1	-0.806**	-0.627**
	Sig. (2-tailed)		0.003	0
Share51	Correlation		1	-.510**
	Sig. (2-tailed)			0
Mean:Median	Correlation			1
	Sig. (2-tailed)			
	**. Correlation is significant at the 0.01 level (2-tailed).			

B. Bivariate correlations for primary regressions

|		Sharegain51	RCPD51	RCTP51	Gini MI	Pct over 65	Unemployment	Median
Sharegain51	Correlation	1	0.117	0.094	.343**	.589**	.406**	-.301**
	Sig. (2-tailed)		0.161	0.339	0	0	0	0
RCPD51	Correlation		1	-.113*	.267**	.207**	0.072	-.173**
	Sig. (2-tailed)			0.032	0.001	0	0.125	0
RCTP51	Correlation			1	.214*	-0.085	-0.081	0.087
	Sig. (2-tailed)				0.027	0.106	0.133	0.099
Gini MI	Correlation				1	0.064	.463**	-0.023
	Sig. (2-tailed)					0.404	0	0.787
Pct over 65	Correlation					1	.103*	-.347**
	Sig. (2-tailed)						0.023	0
Unemploymen	Correlation						1	-.291**
	Sig. (2-tailed)							0
Median	Correlation							1
	Sig. (2-tailed)							
	**. Correlation is significant at the 0.01 level (2-tailed).							
	*. Correlation is significant at the 0.05 level (2-tailed).							

Appendix C: Empirical testing

I. Regression 6.1: The assumptions for the regression from table 6, regression 1 are presented in detail below. This model includes all variables and is estimated on the entire sample, established democracies and non-democracies.
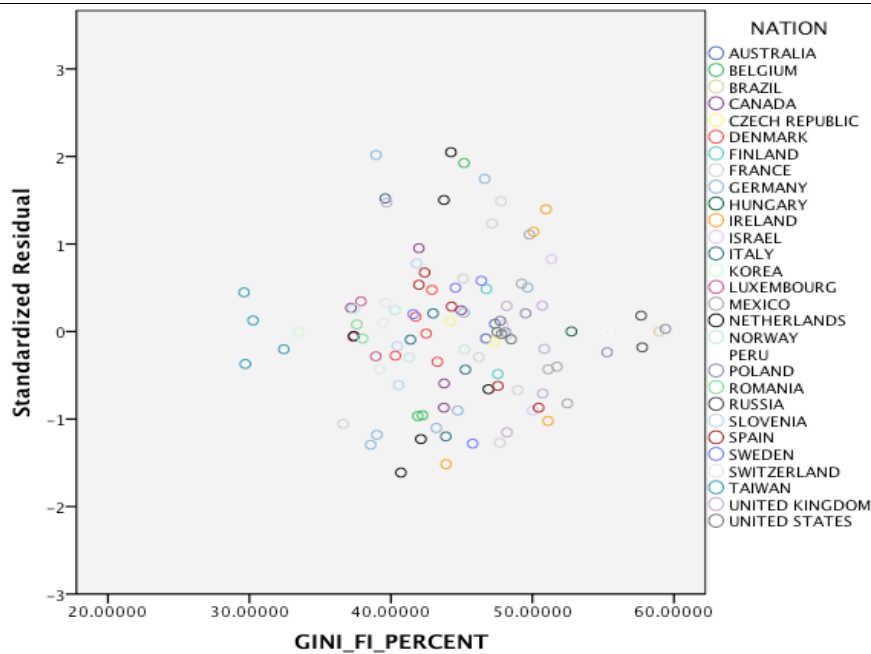
A. Normality of errors
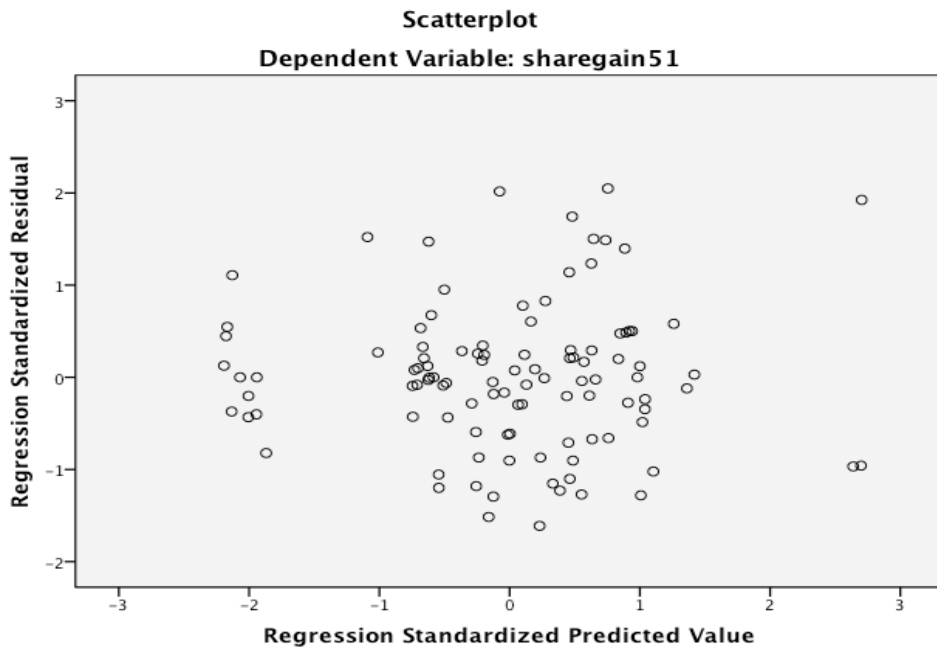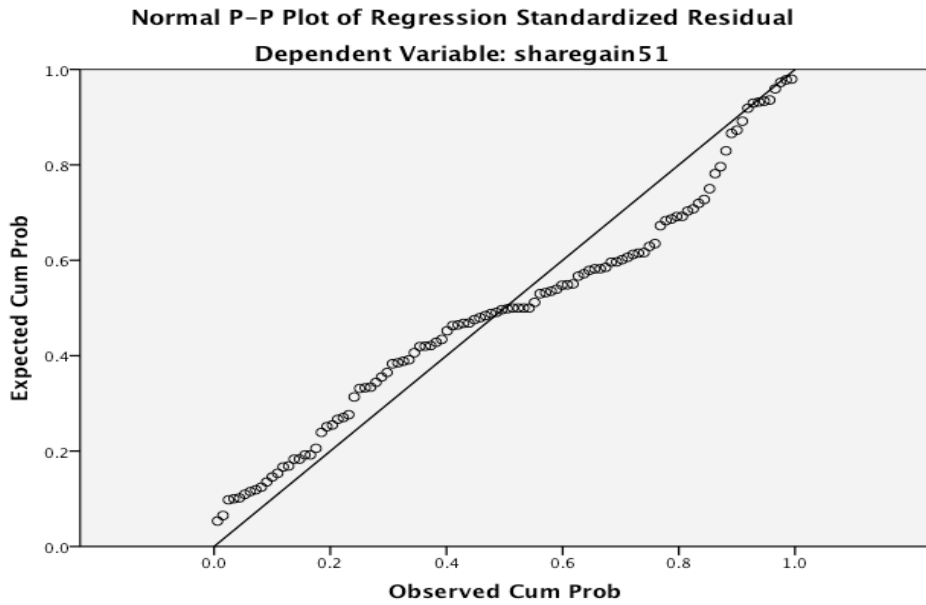
B. Homoskedasticity

"Eye-ball test": With the exception of a few outliers, the residual plots
conform with our expectations for homoskedastic errors:
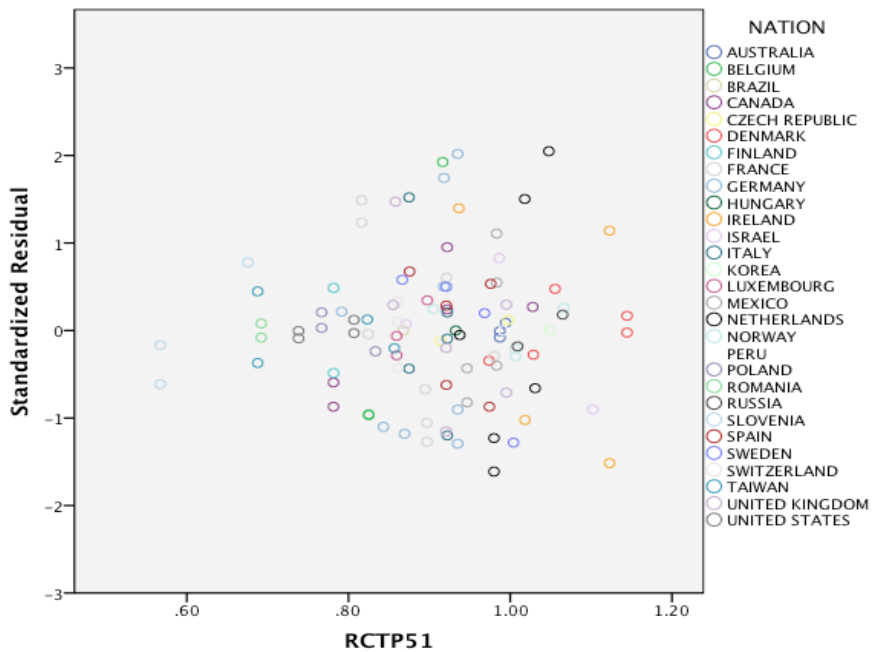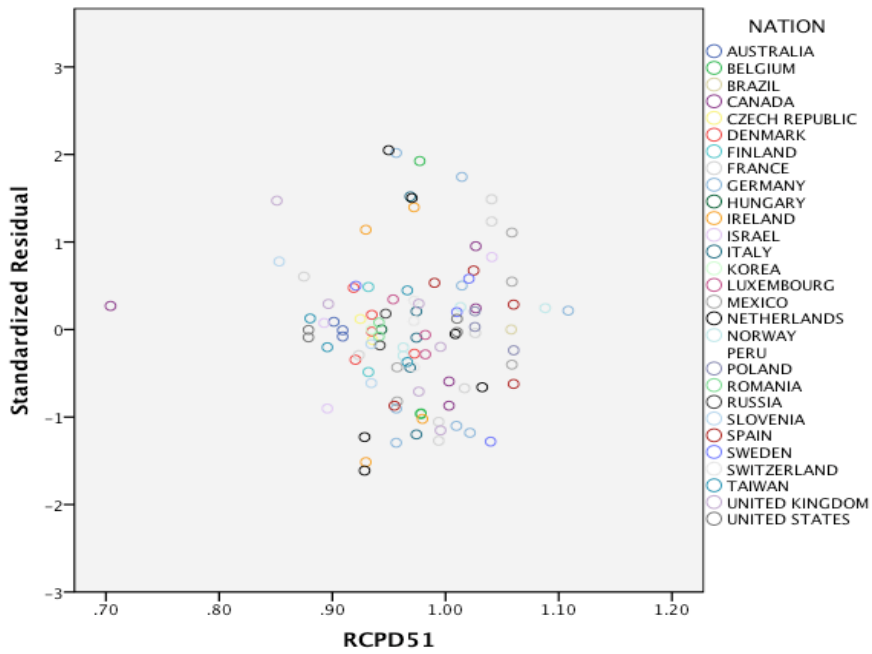
C. Breusch-Pagan tests for homoskedastistic errors: To compute the Breusch-Pagan test (B-P Test) we regress the squared residuals from 6.1 on the regressors to obtain R^2 (Wooldridge 2009, 273). We see that this test is significant at the 0.40 level and the 0.8 level depending on which quadratic terms are included in the specification.

	M	R2	SSREG	B-P Test	Chi2 B-P	Koenker	Chi2 Koenker
Normal	40	0.49	113.77	56.89	0.40	52.05	0.10
Square	46	0.52	119.86	59.93	0.81	54.80	0.18
Cube	47	0.53	122.25	61.13	0.81	55.86	0.18

However, the alternative Koenker test for heteroskedasticity is less sensitive to non-normality in small samples. This statistic is not just barely significant in one version of the test. We therefore do not find significant evidence for heteroskedaticity.

D. Serial Correlation: The Durbin-Watson is included as a default. However, we calculate "Durbin's alternative" in addition to account for violations of strict exogeneity: For this statistic we regress the residuals on all independent variables as well as a one period lagged residual. We do not find that the lagged variable is different from zero. This suggests that heteroskedasticity is not a problem.

E. Functional Form: We initially postulate a linear functional form. We check for non-linearities by including quadratic terms of the independent variables. However, none of these was significant in the presence of the linear term. The non-linear terms were also not significant jointly. We therefore only present results for the linear specification. Note, however, that we did not test logarithmic forms of the equations.

Appendix D: Luxembourg Income Study syntax

I. Example syntax for calculation of income share (Norway 2000)

```
TITLE "******* INCOME SHARES NORWAY 2000 ********" .

DEFINE DOFILE1 () .

SELECT IF DPI NE 0 .
SELECT IF NOT MISSING(DPI) .
SELECT IF NOT MISSING(D4) .

COMPUTE WT = HWEIGHT*D4 .

COMPUTE HFI = (FI + V32 + V33 + V34 + V35 + V36)
COMPUTE EFI = HFI / (D4**0.5)  .

WEIGHT BY WT .

RANK VARIABLES = EFI (A)  /MISSING = EXCLUDE /RFRACTION INTO RANKEFI.

COMPUTE DECILE = 0.

IF RANKEFI GE 0 AND RANKEFI LE .1 DECILE = 1 .
IF RANKEFI GT .1 AND RANKEFI LE .2 DECILE = 2 .
IF RANKEFI GT .2 AND RANKEFI LE .3 DECILE = 3 .
IF RANKEFI GT .3 AND RANKEFI LE .4 DECILE = 4 .
IF RANKEFI GT .4 AND RANKEFI LE .5 DECILE = 5 .
IF RANKEFI GT .5 AND RANKEFI LE .6 DECILE = 6 .
IF RANKEFI GT .6 AND RANKEFI LE .7 DECILE = 7 .
IF RANKEFI GT .7 AND RANKEFI LE .8 DECILE = 8 .
IF RANKEFI GT .8 AND RANKEFI LE .9 DECILE = 9 .
IF RANKEFI GT .9 AND RANKEFI LE 1 DECILE = 10 .

AGGREGATE  /OUTFILE= "kj_hst.sav" /BREAK COUNTRY /TEFI = SUM(HFI) /TEDPI=
SUM(DPI) .

SORT CASES BY COUNTRY.

MATCH FILES /FILE=* /TABLE= "kj_hst.sav" / BY COUNTRY.

AGGREGATE  /OUTFILE= "kj_hsn.sav" /BREAK DECILE /NEFI = SUM(HFI) /NEDPI =
SUM(DPI) .

SORT CASES BY DECILE.

MATCH FILES /FILE=* /TABLE= "kj_hsn.sav" / BY DECILE.

!ENDDEFINE .
GET FILE = NO00h / KEEP = COUNTRY CASENUM HWEIGHT FI DPI D4 V19 V32 V33 V34
V35 V36 .

DOFILE1 .

TITLE "** DECILE SHAREGAIN EQUIVALIZED **".

SORT CASES BY COUNTRY DECILE.
```

SPLIT FILE LAYERED BY COUNTRY DECILE.

DESCRIPTIVES VARIABLES = TEFI TEDPI NEFI NEDPI TEFP TEDPI_FP NEFP NEDPI_FP
/STATISTICS = MEAN.

II. Example syntax for calculation of Gini MI coefficient (Norway 2000)

```
TITLE "******* GINI FI NO 00 *******" .

DEFINE GINCALC () .

SELECT IF DPI NE 0 .
SELECT IF NOT MISSING(DPI) .
SELECT IF NOT MISSING(D4) .

COMPUTE WT = HWEIGHT * D4 .
COMPUTE HFI = (FI + V32 + V33 + V34 + V35 + V36)
COMPUTE GROUP = 1 .

WEIGHT BY WT.

COMPUTE EY = HFI / (D4**0.5) .
SORT CASES BY EY(A).
COMPUTE CUMWGT = CUMWGT + WT .
LEAVE CUMWGT .
AGGREGATE OUTFILE = GINI /PRESORTED /BREAK = GROUP /MEANY = MEAN(EY)
/MEANR = MEAN(CUMWGT) /N = N.
MATCH FILES /FILE=* /TABLE = GINI /BY GROUP .
COMPUTE DEVY = EY - MEANY .
COMPUTE RANK = CUMWGT/N .
COMPUTE DEVR = (RANK - 0.5) .
COMPUTE PROD = DEVY*DEVR .
AGGREGATE OUTFILE = * /PRESORTED /BREAK = GROUP /SUMPROD = SUM(PROD)
/MEANY = MEAN(EY) /N = N .
COMPUTE COV = SUMPROD/(N-1) .
COMPUTE  GINI = COV*2/MEANY .
FORMATS GINI(f10.4) .
LIST VAR GINI .

!ENDDEFINE .

GET FILE = NO00h / KEEP = HWEIGHT D4 D5 FI DPI V32 V33 V34 V35 V36.
GINCALC .
TITLE NO00 .
```

Appendix E: Preliminary thesis report

Appendix E: Preliminary thesis report

Preliminary Thesis Report

- The swing voter hypothesis and income redistribution: An empirical investigation -

Content

Summary

This preliminary thesis report outlines an investigation of the impact of swing voters on programmatic redistribution. Utilizing an extensive panel of micro data, this projects seeks to answer two key political economic questions: (1) Do politicians favor income groups with many indifferent voters? (2) Do politicians favor income groups who are sensitive to favorable policy?

The swing voter model has strong theoretical and intuitive underpinnings. However, there are only a handful of studies that have empirically assessed swing voter effects. The empirical focus of these studies has been on the central allocation of resources to geographically defined constituents—so called "pork barrel" politics. At this time, no studies that examine the role of swing voters in broader "programmatic" policy have been published. This study attempts to address this gap.

Furthermore, given the structure of this research, this study will also enable a re-analysis of the median voter theory. It facilitates a simple comparison of the swing voter model of policy making with the median voter model of policy making.

1. Introduction

It is often observed that politicians bias policies in favor of particular groups of voters. A possible explanation is that politicians target groups of voters in order to maximize political support. A specific manifestation of this effect is when politicians target individuals who can be persuaded to support a politician contingent on favorable policy. Voters with weak political ties and low ideological polarization are generally expected to be most susceptible to persuasion. These individuals are referred to as swing voters because their support is liable to swing between parties depending on policy. It is rational for politicians to focus resources on groups with many swing voters because swing voters are expected to be the most "responsive." In contrast, expending resources on ideologically extreme voters is unproductive: If ideologically aligned, a voter's support is secure. If ideologically opposed, then it is "cheaper" to focus resources on swing voters. Hence, we expect swing voters to shape policy outcomes.

The swing voter perspective has implications for pre-election and post-election politics: Politicians can benefit from targeting swing voters in both electoral competition and in legislative bargaining. Maintaining the assumption of ceteris paribus, specifically we expect that (1) politicians will favor groups with many indifferent voters, and (2) politicians will favor groups who are sensitive to favorable policy.

These hypotheses have primarily been studied in the context of central financing of projects at lower levels of government (Dahlberg and Johansson 2004; Helland and Sørensen 2009). However, can we discern the influence of swing voters on programmatic income redistribution as well?

This preliminary thesis report outlines a research program to investigate the impact of swing voters on income redistribution. Using extensive micro data on income redistribution and political identification, this project proposes to test the key swing voter variables. This project will also re-test the median voter hypothesis. This will facilitate a direct comparison of median voter and probabilistic models of redistribution.

2. Background: Redistribution and Models of Politics

Governments in all developed democracies engage in some degree of income redistribution. However, the extent and pattern of redistribution vary. While some countries pursue progressive policies, transferring substantial amounts from the wealthy to the poor, other countries transfer little. These differences are significant. For instance, Milanovic finds that while the distribution of factor income was relatively similar between the United States and Sweden during the 1990s, taxes and transfers dramatically changed the picture: The United States (1997) had a disposable income Gini of 42.3 while Sweden (1992) had a disposable income Gini of 26 (2000, 374).

What explains these differences in level of redistribution? This question has been a productive and important research agenda in political economy for the better part of a half-century. The question is meaningful because it facilitates the test of models of the political process. It also is important because redistribution has real world welfare effects: Individual welfare is directly affected by redistribution. Because redistribution depends on taxes and transfers, it may also have indirect consequences for economic growth. There is a strong theoretical expectation that swing voters will shape redistributive policy.

The Redistribution Literature and the MVT

To understand the contribution of this study, it is useful to begin by situating the swing voter perspective in the broader redistribution literature. A reasonable place to begin is with Meltzer and Richard's (1981) application of the median voter theorem (MVT). Their prediction is that median voter will set the tax rate to maximize the size of a tax-financed lump-sum transfer, recognizing that the budget constraint is endogenous to the choice of tax rate. In this model the rate of taxation is only moderated by the median voter's recognition that taxation creates deadweight loss. Formally, Meltzer and Richard conclude that the size of the transfer will increase with the (right) skewness of the income

distribution: The greater the difference between the median income and the mean income the greater the size of the transfer. In the presence of a left skew, the median voter is expected to set a zero tax rate. Hence, the median voter should never lose via redistribution.

Despite the attractive features of the Meltzer and Richard model, it has not been supported empirically. Contrary to expectation, many countries with high inequality have low levels of redistribution while many countries with low inequality have high redistribution. Although Milanovic did find a relationship between inequality and redistribution, he concluded that their no support for the MVT because the voter with median income was most often a net loser in the redistribution process: "Out of 68 countries, the fifth decile is a net taxpayer in 49 countries, and gains in 19; the sixth decile is a net taxpayer in 54 countries, and gains in only 14" (Milanovic 2000, 391). Reassessing the data a decade later, Scervini reaches a similar conclusion (2009).

How have researchers have responded? One strategy has been to maintain the median voter mechanism of decision-making but to amend Meltzer and Richard's assumptions. For example, Moene and Wallerstein (2003) criticize the assumption of a lump sum transfer; Benabou and Ok (2001) consider the possibility of upward movement (POUM); and Breddemeier (2010) includes imperfect information. In all three cases the median voter's preference remains the driver of policy. This orientation has much to recommend it. For instance, some cross-sectional studies conducted within one country support the MVT at the municipality level (Borge and Rattsø 2004).

There are, however, reasons to be skeptical that the median voter's preferences will dominate policy outcomes. It is well known from social choice theory that when policies are multidimensional Downsian competition does not yield a Condorcet winner. As a "divide the pie" game, redistribution is susceptible to cycling. Furthermore, the overall level of redistribution results from a combination of large number of policies. Some of these—social insurance, social benefits, etc.—are clearly particularistic; they disproportionately favor certain groups.

Probabilistic Voting Models

An alternative to the median voter models are probabilistic voting models. In these models individuals' voting decisions are uncertain from the perspective of the political parties. Of special relevance are the models of electoral politics developed by Lindbeck and Weibull (1987) and Dixit and Londregan (1996)[1]. The key feature of these models is that voters have unobserved exogenous preferences for one party over the other, in addition to preferences over redistributive transfers. This exogenous policy dimension may be interpreted as ideology. These models are appealing because they resolve the non-existence problem. These models also provide a conceptually satisfying perspective on the dynamics of the political process. Certain groups of voters are more attractive targets of policy for office-seeking politicians. This is the basis for swing voter hypotheses.

3. A Swing Voter Model of Redistribution

Swing voters will shape redistribution if they are distributed unequally across income levels. This seems like a plausible scenario. For instance, it might be the case that in some countries political identification is strongest amongst the poor because of the historical importance of the labour movement. It is equally easy to imagine a situation where the middle class or the wealthy have ideological or political convictions. This is supported by evidence from a pilot study that analyzed a model of rich and poor: Although on average the relative attractiveness of rich and poor was a about the same, the range of the data revealed that the rich were swing voters in some societies while the poor were the swing voters in others.

The model employed in this study is a swing voter model of redistribution. For expositional purposes, assume that two parties, $P = \{L, R\}$, compete for votes across

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[1] In terms of clarity of exposition, Persson and Tabellini's treatement is unparalleled (2000, 52-58).

groups of voters. Each party simultaneously proposes an electoral platform. This consists of a vector of intergroup transfers, t_P, subject to a balanced budget constraint. Voting decisions are based on economic policy but also ideology. The politically relevant features of the voter groups are the swing characteristics: (1) The "cut point" density, and (2) the strength of party affiliation.

Cut-Point Density

The first swing characteristic, "cut-point" density, describes the number of voters that are indifferent between the parties: We expect these voters to be susceptible to changing their voting behavior as the result of a marginal increases in utility. In each group of voters there is a distribution of preferences, with some point defining a voter who is indifferent between the parties:

$$X_i = U_i(Y_i, t_L) - U_i(Y_i, t_R)$$

The number of individuals voting for party L will be $N_g * \varphi_g(X_i)$, where $\varphi_g(X_i)$ is a cumulative distribution function and N_g the total number of individuals in the group. Crucially, it is the density at this "cut point" that impacts political targeting of benefits, not the total size of the group. We expect that groups in which there are many indifferent "swing" voters to attract more favorable policies than groups in which marginal changes don't shift many voters. In other words, politicians will target their transfers to those groups where the greatest number of votes are gained contingent on favorable policy.

Strength of Ideological Affinity

The second swing characteristic reflects the "cost" of shifting the cut point: If an individual is highly ideological, then it will be costly to change their voting preferences. Conversely, if an individual is non-ideological, then a small marginal benefit should induce a shift in preferences. We define a utility function of the following form:

$$U_i(Y_i, t_p) = \kappa_i (Y_i + t_p)$$

The parameter κ measures importance of consumption relative to ideology. A higher value of κ means that consumption is valued more highly. Thus, groups in which party affiliation is weaker—this is to say, consumption is relatively more valuable (a high κ)—

will attract more favorable policies because these voters are more cheaply "bought," i.e. the cut point shifts by a larger amount per unit transfer.

Equilibrium policy proposal

Provided that the two parties are equal in their ability to tax and provide benefits, in equilibrium the parties to pursue "symmetrical stagies" (Dixit and Londregan 1996, 1144). The theoretical expectations are related to cut point density and ideological affinity (as always, ceteris paribus): (1) Parties will transfer resources to the income group with lower ideological affinity (stronger consumption preferences) and (2) parties will transfer resources to the group with a higher number of indifferent voters. These are the two primary research hypotheses.

Although swing voter effects do not drive all redistribution—policy making depends on many factors, including a notion of fairness—there is plenty of reason to believe that swing voter effects impinge on redistributive policy making. If an income group is consistently an attractive target for redistribution then politicians will be rewarded if they direct policies to those groups. This provides a strong incentive. In addition, despite the conceptualization of this model as representing electoral politics, the swing voter model has implications for all policy-making activities.

4. Existing Studies

Despite these clear theoretical expectations, there have been few systematic tests of these hypotheses. This is surprising given the many qualitative observations of swing voter effects. Only during the past decade have the core swing voter variables been rigorously tested against micro data. Although a few older studies of New Deal spending include political variables, their theoretical formulations differ somewhat from the models presented in this paper; researchers have also questioned the validity of these results (Larcinese, Snyder, and Testa 2009, 5). Helland and Sørensen are undoubtedly correct when they state that "empirical research based on swing-voter models is in its infancy"

(2009, 7).

Almost all recent studies of swing voter effects focus on "pork-barrel" politics rather than programmatic redistribution. Johansson and Dahlberg (2002) and Johansson (2003) examine intergovernmental grants; Helland and Sørensen (2009) investigate the allocation of road projects; and Helland, Sørensen, and Thorkildsen (2010) consider the patterns of defense outlays. These studies provide consistent evidence for the swing voter effects. Apart from the study of defense outlays, all these studies conform to the theoretical expectations. Both of the key hypotheses can be endorsed: Johansson finds convincing support for the influence of cut-point density of grant allocation (2003) while Helland and Sørensen make a convincing case that ideological identification has shaped the pattern of road investment.

The focus on intergovernmental grants to geographically defined constituents is understandable. As Johansson and Dahlberg point out, the ideal opportunity to test the swing voter hypothesis is when politicians have discretion to make policy, especially if closely preceding an election (2002, 27).

The only systematic study of swing voter effects that addresses programmatic policy that affects redistribution between groups of voters—as opposed to tactical redistribution to districts—is a study by Sørensen (2010). This study considers the political "clout" of elderly voters relative to young voters. Sørensen uses extensive micro data to construct his swing-voter indicators and tests these against public expenditure data.

Like the study by Sørensen, this thesis examines programmatic redistribution between groups of voters, but focuses on income groups rather than age groups. A primary contribution of this study is that it fills a gap in the swing voter literature. It also serves to synthesize the two previously separate strands in political economy: The study of redistribution and the study of swing voting.

5. Variables and Data

In this project, three groups of variables are in focus: Those measuring redistribution (the dependent variable), those measuring inequality, and those measuring the swing voter variables (the explanatory variables). Sørensen (2010) contributes the method for constructing the swing voter variables while Milanovic (2000) contributes the method for measuring redistribution and inequality. This section reviews these approaches and points out some of the outstanding challenges.

Data for the swing voter variables is gathered from the World Value Survey (WVS), the Comparative Study of Electoral Systems (CSES), and the Eurobarometer. Data on redistribution and inequality is collected from the Luxembourg Income Study (LIS). Additional sources of data are currently being investigated

Income Groups

According to the model developed above, to test the swing-voter hypotheses it is necessary to divide individuals into income groups. In a pilot study, I employed a two-group design in which income groups were designated relative to the median income. However, in the final thesis I may also employ a three groups design; this has the benefit of enabling a direct comparison of the median voter model with the swing voter model.

With respect to the redistribution data, it is straightforward to create income groups because the LIS data is continuous. However, in the case of the data on income taken from the WVS, CSES, and EB, it is difficult to achieve an exact 50/50 or 33/33/33 split because the data is categorical. In the pilot study, I addressed this problem by first calculating the "true median" using the procedure from Powell (2000) and then assigning individuals to rich and poor groups based on whether they were above or below this category. Although this yielded groups of approximately equal size for many country-years, there were a number of instances where the groups were of disproportionate size. No solution presents itself at the present time, but the issue will be re-examined.

Swing-Voter Variables

Sørensen investigates how swing voter characteristics of young and old impact age-dependent spending. An insight of his approach is to utilize *relative* measures of swing voter characteristics. This paper adapts this approach to analyze relative swing voter characteristics across income groups rather than age.

Party Identification ("Greed")

Consistent with previous studies, I expect individuals who report low party affiliation to be more sensitive to transfers or other benefits. An appropriate measure of party identification is available in the WVS, CSES, and EB. The WVS asks "Do you consider yourself to be close or not to any particular party." The CSES asks a nearly identical question. In both cases, "close to a party" is scored as 1 while "not close to any party" is given a 0. In contrast, the Eurobarometer survey uses a question with a four point scale. Here, "very involved" and "fairly involved" were given scores of 1, while "merely a sympathizer" and "no affinities with any parties" were given scores of 0. This operationalization of appears to be relatively uncontroversial.

The actual variable is constructed by aggregating the party identification variable within each country-year-income group. This yields the proportion of individuals who have strong party identification. Relative measures of party identification can then be calculated by comparing the proportions of voters across income groups, but within a country-year.

Cut-Point Density

The second swing-voter characteristic is the "cut point density." This variable captures the number of indifferent voters in each group. In the ideal case a precise estimate is based on election data. For example, studies by Johansson (2003) and Johansson and Dahlberg (2002) use an elegant method to estimate the cut-point. Unfortunately, these methods will likely be unavailable for this project. Instead I anticipate utilizing the simplification made by Sørensen (2010): As a proxy for cut-point density, to calculate the number of individuals located between 4 and 6 on the left right self-placement scale.

Again, the exact form of the variable used in the regression will be a ratio of the proportion of indifferent voters in different income groups—for example, the relative proportion of cut-point voters in the poor group relative to the proportion of cut-point voters in the rich group.

Redistribution Variables

In important respect, this paper takes its basic orientation from Milanovic's (2000) paper *The median-voter hypothesis, income inequality, and income redistribution: an empirical test with the required data*. Although Milanovic focuses on the relationship between income inequality and redistribution rather than the swing voter hypothesis, it is a useful stating point because it rectified a common misspecification: While previous studies of income redistribution tended to examine the relationship between disposable income and government expenditure, Milanovic utilizes data from the Luxembourg Income Study to examine the change in income as measured by the difference between disposable income and factor income[2]. Milanovic calls this variable *Sharegain*. This represents the percentage increase (or decrease) in income after taxes and transfers. *Sharegain* can be calculated for whatever income group is relevant. The *Sharegain of the Bottom Half* and the *Sharegain of the Median Group* will most likely be the operationalizations of redistribution that I employ.

In addition, two versions of *Sharegain* will be calculated: The first treats pensions as redistributive transfers (factor income, "FI") while the second treats pensions as factor income (factor pension, "FP"). Pensions redistribute significantly between individuals—the measured redistribution is much larger for FI than FP. Testing both is worthwhile given that swing voter effects may affect pension policy.

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[2] Government expenditure is misspecification because government expenditure may be high in the absence of redistribution. Furthermore, the use of inequality in disposable income as an independent variable is a misspecification because voting decisions are contingent on the effects of taxation on factor income (i.e. disposable income inequality is an *outcome*). However, despite the improvements, Milanovic's approach also been criticized.

Combining Redistribution and Swing Variables

Unfortunately, both the LIS data and the data on party identification are fairly limited. As a result combining the redistribution and swing voter variables poses challenges because of "mismatch" between the data. In some country years, redistribution data is available but swing voter data are not. In other country years the reverse is true. In order to exploit the data that is available, data will be aggregated over five year periods. This provides another compelling reason to seek out additional data sources.

Inequality

In addition to the swing voter variables, a measure of income inequality will be included to facilitate a direct test of the median voter model against the swing voter model and to serve as a control variable. The LIS database will be used to calculate the Gini coefficient in factor income, *Gini FI,* as this is conceptually relevant pre-tax, pre-transfer measure of inequality (alternatively, the Gini coefficient in factor income inclusive of pensions, *Gini FP*).

Notably, Milanovic found that increased inequality predicted higher redistribution to the bottom half of the income distribution. Although Milanovic did not find support for the median voter hypothesis as conceived by Meltzer and Richard, the effect of income inequality was robust in all his specifications. Including a measure of inequality therefore also enables us to comment on how the inclusion of the swing variables affects Milanovic's estimates.

6. Research Design and Model Specification

Given the orientation of this project, the appropriateness of a statistical design utilizing panel data may be apparent. Nevertheless, it is worthwhile to highlight key features and advantages of this approach.

Because idiosyncratic factors impact redistribution, comparison of different countries over multiple periods provides one of the best opportunities for identifying the unique effect of swing voters. If the relevant variables are controlled, the parameter estimates yield the expected average impact of swing voter effects. If these effects are statistically significant and different from zero, this will provide support for the swing voter effects (Of course, the validity of these estimates depends largely on the quality of the underlying indicators and the extent of data).

Another feature of the statistical approach to the research question is that it accommodates a test of an inclusive concept of swing voter effects. Overall levels of redistribution are the combined outcome of a large number of policies. Each of these components of redistribution—maternity benefits, disability pay, unemployment insurance, etc.—are potential arenas for swing voter effects to manifest themselves. The parameter estimates subsume all of these. The proper interpretation of a significant swing voter coefficient is therefore support for a persistent swing voter effect but not necessarily for a specific model of swing voter effects.

Control

Because this study focuses on identifying the effect of swing voters, it is necessary to control other variables that may affect the tax and transfer regime. One method of controlling these other sources of variance is by including all variables that affect both the swing voter variables and redistribution. This might include measures of religion, education, etc. The challenge of this approach is the availability of the necessary historical data. It may be impossible to retrieve the necessary data to make this a viable approach.

A more attractive approach is therefore to control extraneous variables using "fixed effects." This exploits the panel nature of the data. Conceptually, fixed effects accounts for the time-invariant characteristics of the units. It makes sense to utilize country—and

possibly time—fixed effects. The approach is consistent with our interest in the variation in redistribution within a country. The fact that data only needs to be collected on the time-varying variables greatly lessens the data collection burden. However, this approach also has some drawbacks given that only a few countries have a large number of observations—in fact, for some countries only a single LIS observation is available.

Considering the dearth of data, perhaps the best specification for addressing the research question is therefore a random effects specifications. This approach is more efficient than the fixed effects estimation because it also utilizes the variation between countries to estimate the effects. However, the random effects model has somewhat stricter assumptions: Unlike the fixed-effects model, random effects regression assumes the idiosyncratic (time varying) error is uncorrelated with the individual (time invariant) error (Peterson 2000, 334).

Model Specification

I anticipate testing both fixed and random effects models. A standard OLS model may also be tested even though initial forays yielded relatively huge residuals (albeit with an incomplete set of controls).

Example Model Specification

In the fixed effects specification, dummies are included for country effects (possibly also for fixed effects). Below I present one possible formulation of this model:

$$\textit{Sharegain FI of the Bottom Half}_{it} = \beta_{0t} + \beta_{1t} * \textit{Income Inequality}_{it} + \beta_{2t} * \textit{Age Over 65\%}_{it}$$
$$+ \beta_{3t} * \textit{Relative Party Indentification}_{it} + \beta_{4t} * \textit{Relative Cut Point Density}_{it}$$
$$+ \textit{Country Effects}_i + \textit{Error}_{it}$$

The dependent variable measures the level of redistribution for a given group—consider, for example, the *Sharegain of the Bottom Half*. Income inequality and the percentage of older individuals are included as controls; this is consistent with Milanovic's specification. On the second line are the swing voter variables. If these are measured as

the relative proportion of rich voters relative to the proportion of poor voters (representing, respectively, the top and bottom half of voters), then we have a clear expectation for the coefficients on the swing voter variables. We expect a positive estimate on the coefficient of relative party identification (if the number of ideological rich increases relative to the poor, then the redistribution to the bottom half should increases) and a negative estimate on the relative cut point density (if the number of indifferent voters in the rich group increases relative to the poor group then the bottom half should be less attractive for redistribution). We thus have a clear test of both swing voter hypotheses. The same expectations hold for the random effects specification.

7. Preliminary Results and Outstanding Questions

During the previous term, I performed a preliminary study of the swing voter hypothesis using a two income-group design (above and below median income). This study did not yield strong evidence for the swing voter effects. The party identification variable displayed the expected direction throughout, while the coefficient of the cut-point density variable changed sign depending on the specification. Overall, the only results significant at the 5% level were observed with a random effects specification.

The inconsistency of the results is perhaps not surprising. This initial study suffered from a number of shortcomings. One was a relatively small amount of data. If the full LIS database had been exploited—as I hope to do in the full project—the number of possible observations could potentially have been doubled. Another shortcoming was the relative simple model specification. Is it possible that the inclusion of additional controls would have improved the model estimates?

The outstanding questions reflect the threats to validity. It seems especially worthwhile to look for additional ways of operationalizing the swing voter variables. Although the indicators used in the initial study seem to capture the relevant constructs, I don't have supreme confidence in the construction of either variable. One possibility would be to try to exploit richer political survey data to create an index of political alignment that

depends on multiple indicators. These possibilities will be investigated in the coming weeks.

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