Essays in the Comparative Political Economy of Taxation and Redistribution

Dissertation

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By

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Abstract

This dissertation consists of four self-contained essays in the comparative political economy of taxation and redistribution. The first essay empirically explores the underlying dynamics of the well known empirical regularity that democracies that have proportional electoral systems spend substantively more on welfare policies than those that have majoritarian systems. The essay contributes to the literature by bringing new micro-level evidence to bear on theories seeking to explain the phenomena, and as such provides a stronger empirical foundation for evaluating the theories in question. Overall, I find robust support for more proportionality leading to more income-based voting. The second essay provides a theoretically-driven conceptualization of absolute and relative individual income shifts and argues that the conceptualization of income shifts has important implications for how we think about the effects of the economy on redistributive preferences. The essay presents a general theoretical framework, which accounts for empirical findings on both the effects of economic mobility and macroeconomic cycles on redistribution. Based on a novel experimental "redistribution game", the results indicate that expected shifts in absolute and relative income have opposite effects on preferences. The third essay argues that employment insecurity as a critical and salient factor determining

incumbent support and voter turnout. The theory developed goes beyond existing approaches by providing a better conceptualized measure of salient economic experiences, as well as highlighting that the economy can often serve both as a valance and positional issue, which can have important implications for the effects of the economy on voting behavior. Finally, the fourth essay develops and empirically tests a theory of the domestic political foundations of the adoption of the value added tax, or VAT. Building on the recent literature on the relationship between regressive taxation and welfare state generosity, I hypothesize that generous welfare states, left-wing governments, corporatist labor market institutions, and consensus building political institutions should all increase the probability of early adoption of the VAT. I find strong support for the effects of corporatism and proportional representation on early adoption of the VAT. Conversely, I find no support for the proposition that welfare state generosity or left-wing government partisanship facilitate early adoption of the VAT.

For my family

Acknowledgments

Writing a dissertation is a long and arduous process, requiring the support of a great many people. While it would be impossible to name all those who have helped along the way, I wish to express my gratitude to the individuals who have made a lasting difference for me, both professionally and personally.

One of my great fortunes has been the opportunity to work with my committee members. They are consummate scholars and dedicated mentors, and serve as role models for anyone wishing to succeed in academia.

In the case of Janet Box-Steffensmeier, I can honestly say that this dissertation would never have materialized had it not been for her support, when, in my third year at Ohio State, I moved from Columbus back home to Iceland for personal reasons. Janet generously helped me develop and implement an online course in political methodology, which allowed me to keep my funding at Ohio State and continue with my dissertation from afar. For her role at this crucial moment in my career, I will be forever grateful. Janet has helped me develop as a researcher and a teacher, introduced me to the intricacies of the publication process, and given me the confidence to explore and implement sophisticated methodological techniques.

Through generous comments on my own work and seminar discussions, Sarah Brooks has instilled in me a great appreciation for the role of theory in the social sciences. Without her inspiration, much of this dissertation would be a hotchpotch of numbers, tables, and figures, with only rudimentary connections to underlying theoretical implications. Indeed, Sarah's own work has motivated me to constrain my empiricist tendencies and consider more carefully the theoretical aspects of my research. In addition to shaping me as an academic, Sarah, along with Marcus Kurtz, were an invaluable source of support when my family of two doubled in size midway through my graduate career.

Finally, I could not have wished for a better advisor than Philipp Rehm. From my first quarter at Ohio State, Philipp has been a constant source of inspiration, support, and sage advice. Through his own research, numerous conversations and email exchanges, he has time and again demonstrated to me the importance — and possibility — of combining big ideas, rich theories, and sound empirical evidence. For his influence, I now worry less about statistical significance and more about substantive significance. He has pushed me, when I've needed pushing; guided me, when I've needed guiding; and stopped me, when I've veered too far off the path. I can only hope that this dissertation, and my future research, bear the mark of his supreme instruction.

In addition to my committee members, I am grateful for the instruction and support of the faculty and staff of the Political Science Department at Ohio State. Special thanks go to Irfan Nooruddin and Marcus Kurtz, whose insight and guidance have made a lasting impression on my views of social scientific research; to Tony Mughan, who generously took a place on my candidacy examination committee on short notice; and to Courtney Sanders, who was always willing to help with administrative requirements. I also thank

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Pursuing a graduate degree in political science may seem like a questionable life decision for a successful young professional with a lucrative paycheck. But when passion propelled me across the Atlantic to do so, I was lucky enough to have a family that spurred me onward. There is no amount of thanks that can sufficiently convey my gratitude to my parents, siblings, and in-laws, for their unconditional support.

They say life is what happens to you while you're busy making other plans. And so it was when my twin daughters, Anna and Birna, came into this world midway through my graduate career and forever changed my life. They are a constant source of joy and amazement and have made completing this project infinitely more bearable. Finally, no one has suffered more for this dissertation than my wife, Katrín. Her love, wisdom, and companionship, have given me the strength and capacity to complete this project. For her unwavering support and self-sacrifice, and a million other things, my deepest gratitude belongs to her.

Vita

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Chapter 1

Introduction

As befits its title, this dissertation is composed of four self-contained essays in the comparative political economy of taxation and redistribution. The essays are *comparative*, in that they seek to shed light on why politically relevant phenomena differ systematically across countries. The essays follow a *political economy* approach, in that they carefully consider how economic factors and institutional contexts affect the preferences and behavior of political actors. Finally, the essays share a thematic focus on *taxation and redistribution*, or put differently, the ways in which governments raise revenue through taxation and redistribute it through welfare policies, and the inherent distributional conflicts such policies create in a democracy. Though these elements form consistent threads throughout the dissertation, the individual essays are eclectic; they differ in their primary level of analysis, the outcome being explained, and methodological approaches taken. To this end, two of the essays seek to explain individual level outcomes, while the other two are focused on country level outcomes. While the dependent variable in two of the essays is political behavior, in another one it is political preferences, while in yet another it is the decision

to adopt a particular policy. One of the essays is Bayesian, one employes an event history approach, and one uses non-parametric techniques. Thus, while the essays have a common core, they are distinct from one another.

The first essay (Chapter 2) empirically explores the underlying dynamics of the well known empirical regularity that democracies that have proportional electoral systems spend substantively more on welfare policies than those that have majoritarian systems. While theoretical accounts of this finding are generally tested using macro-level data, micro-level implications are usually left untested. The essay contributes to the literature by bringing new micro-level evidence to bear on each of the theories, and as such provides a stronger empirical foundation for evaluating the theories in question. The research design leverages the fact that each of the theories make predictions about the electoral coordination between parties and voters around broad-based redistribution under alternative institutional arrangements. To test the theories, I create a novel measure of income-based voting, which captures the sensitivity of vote choice to changes in income. The measure, which covers 97 elections in 42 democracies from 1996 to 2013, forms the dependent variable in a second stage model that seeks to explain why the association of income and vote choice differs across countries. In addition to the main analysis, I also leverage the unique structure of seven mixed electoral systems included in the sample and test whether the extent of income-based voting differs across electoral tiers in the same country. Overall, I find robust support for more proportionality leading to more income-based voting.

The second essay (Chapter 3), co-authored with Vittorio Merola, provides a theoretically-driven conceptualization of absolute and relative individual income shifts and argues that the conceptualization of income shifts has important implications for how we think about

the effects of the economy on redistributive preferences. Existing approaches provide conflicting predictions about the effects of economic mobility and macroeconomic cycles on redistributive preferences, and we argue that this confusion is mostly due to the crucial distinction between absolute and relative income shifts, both of which are produced during economic cycles, yet rarely separated conceptually or empirically. It is this crucial distinction that separates political economy perspectives on redistribution, which prioritize the role of relative income, and public opinion perspectives on social policy preferences, which emphasize the dominant role of absolute income. We argue that these separate strands of the literature can be combined in a more general theoretical framework, which accounts for empirical findings on both the effects of economic mobility and macroeconomic cycles on redistribution. Fundamentally, we argue that after relative income shifts, differences are made salient, resulting in more self-interested behavior. Conversely, after absolute income shifts, similarities become more apparent, resulting in more in-group solidarity, which increases inequality aversion. We demonstrate this experimentally, using a novel "redistribution game". The results indicate that expected shifts in absolute and relative income have opposite effects on preferences. This highlights the importance of carefully conceptualizing and measuring income shifts, and has implications for how we think about economic perceptions and economic voting.

The third essay (Chapter 4), also co-authored with Vittorio Merola, argues that the unemployment rate in an individual's occupation informs perceptions of employment insecurity, as well as serving as a salient and powerful heuristic for aggregate economic performance. Consequently, high and rising occupational unemployment leads to negative evaluations of the economy and reduces the probability of supporting the incumbent

government. Simultaneously, however, such changes shift support toward left-wing parties. Thus, employment insecurity serves as a valence issue, with voters preferring parties that are most competent to address the issue of unemployment, but it is also inherently a positional, or partisan issue, due to the distributional consequences of welfare policies. This brings about a potential conflict, as under left-wing incumbent governments, the economically insecure are cross-pressured, which increases the likelihood of exiting the electoral arena by abstaining from voting. We test our hypotheses using a Bayesian hierarchical multinomial model, with individual level data from 43 elections in 21 countries. We find strong support for the hypothesized effects of economic insecurity on incumbent support, with a follow-up analysis supporting the posited informational mechanism.

The fourth essay (Chapter 5) develops and empirically tests a theory of the domestic political foundations of the adoption of the value added tax (VAT), which raises over 20 per cent of the world's tax revenue. Existing explanations of the rise of the VAT tend to emphasize the effects of international and economic factors, with domestic processes largely assumed to be apolitical. I argue that this assumption is unwarranted. Building on the recent literature on the relationship between regressive taxation and welfare state generosity, I hypothesize that generous welfare states, left-wing governments, corporatist labor market institutions, and consensus building political institutions, should all increase the probability of early adoption of the VAT. Conversely, residual welfare states, right-wing governments, non-corporatist labor market institutions, and majoritarian political institutions, should impede the adoption of the VAT. I test the hypotheses using an event history approach and data on VAT adoptions in 22 OECD countries from 1960 to 2012. Using a variety of model specifications and robustness tests, I find strong support for the effects of corporatism and proportional representation on early adoption of the

VAT. Conversely, I find no support for the proposition that welfare state generosity or left-wing government partisanship facilitates early adoption of the VAT.

I conclude in Chapter 6 with a summary of the results.

Chapter 2

Income-based Voting and Polarization over Redistribution under Alternative Electoral Systems

Do electoral institutions affect the electoral coordination of parties and voters around broad-based redistributive issues?[1] If so, can that explain why democracies with proportional electoral institutions have considerably more generous welfare states than democracies that have majoritarian electoral systems? In recent years a number of scholars have attempted to explain this empirical regularity, offering a variety of theoretical accounts of how electoral systems affect redistributive outcomes. While the causal mechanism by which one affects the other takes different forms in different accounts, most theories focus on how electoral systems affect the electoral coordination between political parties and voters, which in turn affects policy outcomes. On the elite level, electoral institutions are claimed to have implications for the number of viable political parties that compete, for

[1]I gratefully acknowledge comments and suggestions from Janet Box-Steffensmeier, Sarah Brooks, Raphael Cunha, Vittorio Merola, Marcus Kurtz, Jason Morgan, Anthony Mugan, Irfan Nooruddin, and Philipp Rehm. An earlier version of the paper was presented at the annual meeting of the Midwest Political Science Association in Chicago, 2013, and at the Comparative Politics Dissertation Workshop (CPRW) at The Ohio State University. I thank participants and discussants for helpful comments and suggestions.

the policy platforms that parties choose, and for how credible those platforms are to voters (e.g. Persson and Tabellini, 2003; Iversen and Soskice, 2006). On the mass level, electoral institutions are claimed to affect how citizens translate their interests into vote choice and how those vote choices are translated into seats in assemblies (e.g. Roemer, 1998; Rodden, 2005). All these features have been hypothesized to have systematic implications for the generosity of welfare policies.

The most common approach to testing the empirical implications of the major theories in the literature is at the macro-level, by associating some measure of redistributive outcomes with some feature of electoral systems. While such tests have served to assess the plausibility of the different theories, they are in many ways problematic. This is not least so because the theories in question were, at least partly, inspired by the empirical regularities found in the data. As such, there is value in developing further and testing the theories in a setting that did not form part of the original puzzle motivating the theories.

A number of papers have sought to go beyond the standard approach and focus squarely on specific aspects of the posited mechanisms. For example, Stratmann and Baur (2002) analyze how legislative behavior in the German Bundestag differs between legislators elected through the majoritarian tier and the proportional tier, finding that legislators elected through the former tier are more likely to be members of parliamentary committees that allows them to appropriate funds to their geographically-based constituencies. Similarly, Gagliarducci, Nannicini, and Naticchionia (2011) find that legislators elected through the majoritarian tier of the mixed Italian electoral system between 1994 and 2006 were more prone to sponsor bills targeting spending at their constituencies than their proportionally elected counterparts. Such studies have also analyzed differences in party

discipline across electoral tiers in mixed systems. Sieberer (2010) finds that legislators elected under the majoritarian tier in the German electoral system are more likely to deviate from the party line in parliamentary voting, while Kunicova and Remington (2008) reach a similar conclusion for legislator behavior in the mixed Russian electoral system.

In this paper, I take such an alternative empirical approach to assessing the plausibility of the major theories in the literature. I leverage the fact that the theories in question make predictions about the coordination between parties and voters and use data at the micro-level to test those predictions. The paper thus goes beyond existing approaches by testing the observable implications of established theories in a novel setting. In doing so, it provides a stronger empirical foundation for evaluating the theories in question.

To test the theories, I create a measure of income-based voting, which captures the sensitivity of vote choice to changes in income. The measure, which covers 97 elections in 42 democracies from 1996 to 2013, forms the dependent variable in a second stage model that seeks to explain why the association of income and vote choice differs across countries. In addition to the main analysis, I also leverage the unique structure of seven mixed electoral systems included in the sample and test whether the extent of income-based voting differs across electoral tiers in the same country.

I find robust support for more proportional systems leading to more income-based voting. In particular, my results suggest that larger average district magnitudes are consistently associated with more income-based voting. At the same time, I do not find evidence in support of the claim that the voting behavior of middle-income voters is systematically more similar to the voting behavior of high-income voters in majoritarian systems; nor

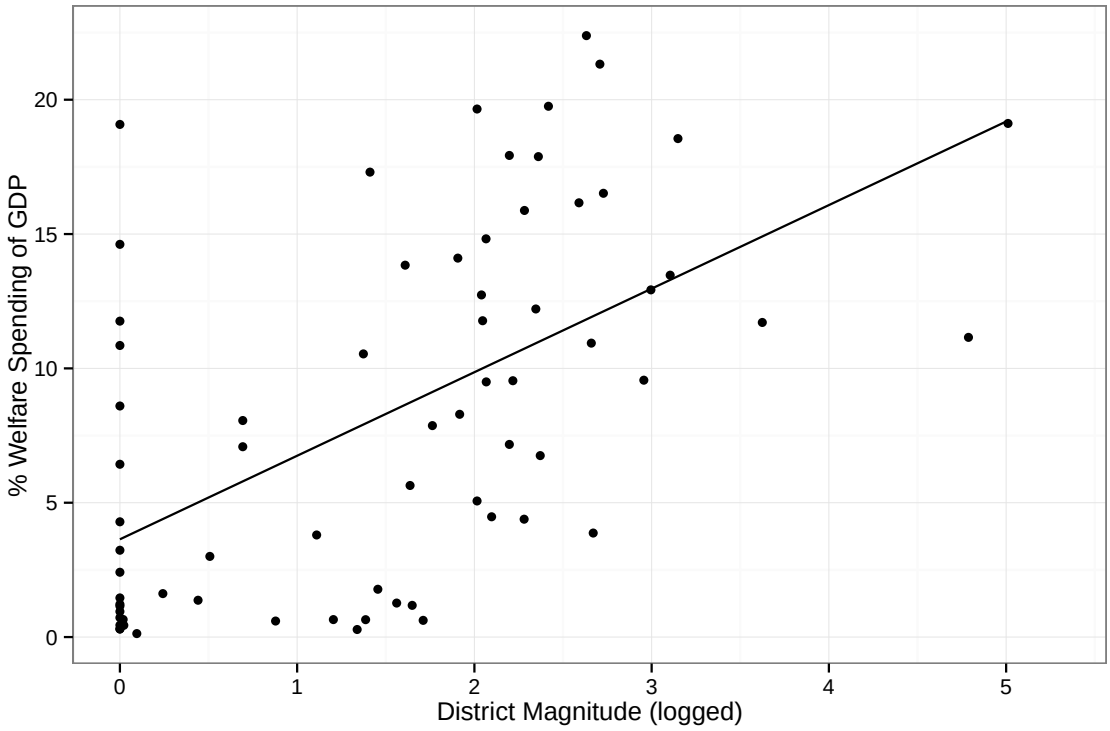
that alternative salient cleavages only affect the extent of income-based voting under majoritarian electoral rules. Furthermore, although the number of mixed electoral systems in the sample is small, the results indicate that income has a larger effect on how individuals vote in the proportional tier than in the majoritarian tier of the system, which is consistent with the evidence from the main analysis.

2.1 Linking Electoral Systems and the Welfare State

Democracies that have proportional electoral systems spend considerably more on welfare policies than those that have majoritarian electoral systems. Persson and Tabellini (2003:179), for example, find that in a sample of 70 democracies in the 1990s "*[m]ajoritarian elections cut welfare spending [...] by as much as 2-3% of GDP.*" Considering that the mean value of their cross-sectional measure of welfare spending is 8.1% of GDP, the difference in spending between countries with different electoral systems is substantively significant. In figure 2.1, I show the bivariate relationship between welfare spending and electoral systems, operationalized in terms of average district magnitude. The graph shows that the simple association between the two is fairly strong, with Persson and Tabellini's (2003) results suggesting that the association holds when analyzed with more sophisticated methods.

In recent years, several mechanisms have been proposed to account for this empirical regularity.[2] Broadly, they can be divided into three categories, based on their primary explanatory factor: First, theories that focus on how electoral rules affect the incentives

[2]Several theories also associate electoral systems with the size of the public sector more generally or the extent of public goods provision (e.g. Lizzeri and Persico, 2001; Persson and Tabellini, 1999, 2000; Bawn and Rosenbluth, 2006; Persson, Roland, and Tabellini, 2007). While these theories pertain to the subject of this paper, I focus only on theoretical mechanisms that have explicitly associated electoral systems with welfare spending or the level of social protection.



Note: The line shows the bivariate OLS relationship between welfare spending as a share of GDP and the log of district magnitude. The figure is based on averages of the variables from 1990 to 1998 for 70 democracies (see Persson and Tabellini, 2003, for details).

Figure 2.1 – Welfare Spending and District Magnitude

of parties to offer broad-based versus geographically targeted policy platforms; second, theories that focus on the effects electoral rules have on the number of viable parties, which in turn affects party platforms; and, finally, theories that posit that majoritarian electoral systems diffuse the effects of economic interests on vote choice in the context of alternative salient cleavages.

Below, I discuss these mechanisms in turn, highlighting the observable implications they have for the electoral coordination between parties and voters around broad-based redistributive issues. In particular, I specify the implications in terms of how the association

of income and vote choice — income-based voting — should differ systematically across electoral systems. As I discuss further below, any systematic differences we observe in income-based voting should be directly related to the extent to which political parties mobilize voters on redistributive issues.

2.1.1 District-based Targeting of Goods

Several related contributions to the literature on the policy implications of electoral systems contrast the platform incentives faced by political parties under majoritarian and proportional electoral rules (e.g. Persson and Tabellini, 2000, 2003; Milesi-Ferretti, Perotti, and Rostagno, 2002). In these accounts the critical feature of electoral systems is to what extent electoral competition between parties is partitioned into districts. When parties compete in a single national district and receive seats in proportion to their vote share, they have an incentive to seek support from broad coalitions of voters and provide broad-based goods, such as social protection. However, when parties compete under plurality rule in single-member districts, electoral competition will to a larger extent be characterized by promises of targeted local goods, or "pork-barrel" (Persson and Tabellini, 2003:17). Importantly, targeting under majoritarian systems is driven by the fact that some districts are "swing" districts and thus play an especially important role in the bid of parties for a majority in the legislature. In the case of proportional electoral systems, however, districts do not have the same discrete effect on electoral outcomes, making appeals to voters across districts a more viable political strategy.

Because theories in this tradition generally do not depend on voters having redistributive interests based on their relative income, they do not have clear implications for how

income-based voting should be affected by different electoral systems. They do, however, indirectly suggest that elections in majoritarian electoral systems should be characterized by more district-based cleavages. Formalizing the micro-level intuition behind that implication, Huber and Ting (2009) develop a model whereby parties can offer two different transfers to voters: Either transfers from rich to poor (i.e. redistribution), or transfers to specific districts from a common pool of funds (i.e. pork). The authors go on to demonstrate that under certain circumstances it can actually be rational for voters to vote against their redistributive interests, if by doing so they gain access to pork barrel spending (2009:22). While Huber and Ting do not contrast different electoral systems, their findings imply that as districts become smaller, the local interests of all voters within districts should become more homogeneous. Majoritarian electoral systems should thus raise the salience of district-based cleavages vis-á-vis class-based cleavages. This intuition sits well with the more macro-focused theories proposed by Persson and Tabellini (2000) and others, and implies the following hypothesis:

> *Hypothesis 1: Smaller electoral districts should weaken the effects of broad-based redistributive interests on vote choice. Income-based voting should thus be weaker in less proportional electoral systems.*

2.1.2 The Credibility of Policy Platforms

Another line of research endogenizes the number of political parties to the electoral system and claims that the primary causal effect electoral systems have on redistributive outcomes is through their effects on the number of viable political parties. The fundamental logic behind such mechanisms is based on the well-known "Duverger's law" and "Duverger's hypothesis", which, respectively, state that majoritarian electoral systems tend

to lead to two party systems, while proportional electoral systems should be associated with multi-party systems (Duverger, 1963; Cox, 1997).

Iversen and Soskice (2006) argue that the effects electoral systems have on the structure of the party system should have predictable implications for government partisanship, which in turn helps explain cross-national differences in welfare spending. Indeed, analyzing government partisanship in 17 advanced democracies from 1945 to 1998 reveals that in countries that employ a majoritarian electoral system, center-right governments have been in power for about 75% of the time, while the exact opposite holds true for countries that use a proportional electoral system. In those countries, center-left governments have been in power for about 75% of the time (2006:166, pure center governments are excluded).

Iversen and Soskice maintain that the reason for this partisan bias is the differences in platforms that parties can credibly commit to under different electoral rules. According to their model, in a proportional electoral system parties will represent the interests of specific classes. Thus, middle-income voters will be represented by a specific party, and so forth. Because no group forms a majority, parties must form a post-election coalition to govern. Because the parties perfectly represent each class, middle-class voters can safely vote for the middle-class party, without fearing that "their" party will jeopardize their interests once in government. The low- and middle-classes can, thus, successfully form a coalition in a proportional electoral system, which will result in a higher incidence of center-left governments, and by extension, more redistribution (2006:170-1).

The situation is somewhat different under majoritarian electoral rules. Because of the restraining effects of the electoral system, only two parties will be viable in equilibrium: a

center-left party and a center-right party. While the calculus of individuals with low income, on the one hand, and individuals with high income, on the other hand, will be the same as under proportional electoral rules (i.e. they each have a "natural" party to support), the same cannot be said for the middle class. Middle class voters must, thus, decide whether or not to align themselves in a coalition with low-income voters in the center-left party or with the high income voters in the center-right party. Because parties cannot make a binding commitment to their platforms before elections, there is a possibility that after the elections the center-left party will veer towards the left and the center-right party will veer towards the right. Since the middle-class has more to fear from a government dominated by the interests of low income individuals, the middle-class will rather form a coalition with the high-income class in the center-right party. Majoritarian electoral systems will, thus, tend to bring about center-right governments, which in turn, will lead to less generous welfare policies (2006:169-70).

Since Iversen and Soskice assume that low- and high-income voters will always vote for left and right parties, respectively, their theory does not have implications for how their behavior should differ across electoral systems. However, their theory does have clear implications for the behavior of middle-class voters: while they should vote directly for a party that represents their interests in proportional electoral systems, their voting should be systematically skewed towards the voting behavior of high-income voters in majoritarian electoral systems. This leads to the following hypothesis:

> *Hypothesis 2: In majoritarian electoral systems, the voting behavior of middle-income individuals should be more similar to the voting behavior of high-income individuals than to the behavior of low income individuals. However, in proportional electoral systems, the voting behavior of middle-income individuals should not be skewed in either direction.*

2.1.3 Alternative Political Cleavages

The literature discussed above generally assumes that there is a single redistributive dimension that determines how individuals vote and how parties compete. Scholars that question that assumption posit that alternative dimensions of policy could affect the coordination between parties and voters, and thus decrease the salience of redistributive issues. This mechanism has been advanced and analyzed formally in a series of works by Roemer (1998); Roemer (2006) and Roemer, Lee, and Van der Straeten (2007). Roemer takes as a given that individuals not only care about their material well-being, but also some secondary issue, such as religion, race or immigration. Because political parties bundle policy positions together, voters can be forced to choose between their preferences on redistribution and the secondary issue if no party offers their ideal policy in both issue domains.

As De La O and Rodden (2008:469) and Huber and Stanig (2009:17) point out, such "forced choice" is likely to be more pronounced in majoritarian electoral systems, where the strategic incentive to bundle issues is larger. Indeed, Roemer, Lee, and Van der Straeten (2007:128) claim that this "policy-bundling effect" has had a considerable effect on the welfare state in the United States, where low income individuals holding strong opinions on racial issues have voted for the anti-redistributive (Republican) party because the redistributive (Democratic) party has not had a policy position to their liking on racial issues. More generally, De La O and Rodden (2008) have applied this insight to the "distracting" effects of religion on the vote choice of low income individuals, while Huber and Stanig (2009) find that in countries where there is no party that is both pro-redistribution and

”right-wing” on issues of individual liberty, support for right-wing parties is generally higher among low income groups.

Importantly, the policy-bundling effect might not only affects the voting decisions of individuals, but likewise affect the policy platforms offered by parties. As Norris (2004:99-101) argues, faced with multiple salient issues, parties in majoritarian systems have strong incentives to develop ”bridging” or ”catch-all” platforms, that is, to moderate their position on individual issues as to not alienate voters. In proportional electoral systems, parties face no such incentives. To the contrary, candidates can gain electoral support by offering distinct policy positions and appeal to narrow segments of the population. In terms of the effects of electoral rules on income-based voting, the following can, thus, be conjectured:

> *Hypothesis 3: Under majoritarian electoral systems, the existence of salient non-redistributive cleavages should lead to less income-based voting. However, under more proportional electoral systems, voters should not be forced to “bundle” policies to the same effect, implying that salient non-redistributive cleavages should not affect income-based voting.*

2.2 Empirical Strategy

In what follows, I begin by discussing how I operationalize the main concepts relevant to the theories being tested. I then discuss the techniques I use to estimate the models in the main analysis.

2.2.1 Measuring Income-based Voting

Income-based voting reflects the extent to which income determines or affects how an individual casts his vote in elections. While the definition is couched in terms of voting behavior, it can also be conceptualized in terms of the extent to which political parties and voters coordinate over redistributive issues. As Rehm and Reilly (2010) point out

(see also Mair, 2001), features of a party's constituency can be used as indicators of the policy positions parties take. Thus, for example, if voters of party A have lower than average income, while the voters of party B have higher than average income, it can be deduced that the platforms of the parties differ in terms of their appeals to redistributive interests. In the present case, I generalize this logic to the country-level, such that income-based voting captures the extent to which parties and voters coordinate over broad-based redistribution.

While this is a fairly innocuous definition, it is not straightforward to measure in a manner that is comparable across countries. This is mainly so because individuals across different countries are faced with different choice sets, as well as a different number of choices, making unmodified vote choice uncomparable across countries. Due to this structure of the dependent variable, a one-step hierarchical model is not suitable, and a two-step estimation strategy is necessary, where the first stage serves to create a measure for each country-election that is comparable across contexts.

A common strategy for the first stage is to simplify the choice set by ordering parties on a single dimension and selecting some value beyond which some parties can be classified as, say, left parties, and simply estimate a logit or a probit model. The outcome variable to be explained, therefore, reflects whether respondents voted for a left party or not. This approach is, however, somewhat problematic for at least two reasons. First, there is considerable loss of information involved in collapsing numerous choices onto a binary indicator variable. Second, and perhaps more fatally, although the variable has the appearance of being comparable across countries, strictly speaking it is not, due to the fact that a one on the binary indicator — say, voting for a left party — is not necessarily

the same choice across different contexts (e.g. voting for the Swedish Social Democratic Party is not necessarily equivalent to voting for the US Democratic Party). As the theories being tested in this paper suggest, party platforms do differ across contexts, suggesting that using a dichotomous variable to capture vote choice can lead to serious measurement error.

A more appropriate strategy involves modeling the entire choice set faced by individuals, using a discrete choice model, such as a multinomial logit (MNL). Such an approach does not, however, provide us with directly comparable measures of income-based voting across countries — the number and composition of choices faced by voters differ across countries, making it unclear what exactly is to be compared, and also making a one-step hierarchical model unsuitable.

It is thus necessary to develop a method to compare the effects of income on vote choice across countries. Duch and Stevenson (2008) provide one such approach, which they use in estimating the effects of the state of the economy on vote choice. With minor adjustments, the strategy is well suited for the task at hand. It involves four steps, which largely mirror the steps taken by Duch and Stevenson: Identifying high-quality comparable data on vote choice, specifying country-level models of vote choice, estimating each country-level model separately, and constructing a measure of income-based voting based on the estimation.[3] While this two-step method is not without its drawbacks (e.g. Beck, 2005), it is necessary to overcome the issue of uncomparable vote choice across different electoral contexts.

[3] See Duch and Stevenson (2008:42-52) for a more detailed account of their approach.

Identifying high-quality comparable data on vote choice

The first step involves identifying country-level surveys with the required questions that are comparable across countries. While there are several available cross-national datasets that one could utilize, the Comparative Study of Electoral Systems (CSES) in many ways contains the most appropriate data for the study: It is designed specifically to allow cross-national comparisons of voting behavior, it includes detailed questions on vote choice and includes a fairly rich set of possible covariates. All in all, I utilize data on 97 lower house elections in 42 democracies from 1996 to 2013 (CSES, 2003, 2007, 2013, 2014).[4]

Besides the vote choice variable, the main variable of interest is the measure of income, and in particular, how income is associated with vote choice. While the CSES-data includes such a variable, it is rather crude: income is measure at the household level, it is not adjusted for household size, and is reported in quintiles, rather than on a continuous scale. Notwithstanding this issue, the benefits of using the data outweigh the costs, considering the alternative datasets available.

Specifying country-level models of vote choice

The second step involves identifying the control variables to be used in the separate multinomial models. There are two main approaches that one could take in choosing the control variables: First, one could specify the same covariates for each separate election regression, and second, one could include country specific covariates where it is deemed necessary to have an adequate statistical model. Duch and Stevenson (2008) argue for the latter strategy. Thus, for example, one might want to include a measure of a voter's race in a model of vote choice for the United States, while doing so for Norway might not matter

[4]The full list of elections included in the analysis are shown in table A.2 in the appendix.

a whole lot. The upside of this approach is that it allows one to specify a model that is sensitive to idiosyncratic determinants of vote choice, which is likelier to lead to an unbiased estimate of the effects of our main independent variable, income. The downside of specifying a specific model for each country, however, is that each model is to some extent affected by the subjective judgment of the researcher himself, as well as the possibility that less fully specified models will be less efficient and systematically overestimate the effects of income.[5]

With that limitation in mind, I follow Duch and Stevenson's (2008) precedence, although I only include covariates that can plausibly be claimed to be causally prior to the income variable. Thus, while they, for example, generally include a variable that captures party identification, I do not do so, since doing so would risk introducing post-treatment bias into the results (i.e. if income affects party identification, including the latter will lead to a systematic underestimation of the effects of income on the outcome).

In my primary specification, income enters the model dummied for each income quintile, save for the third quintile, which is the base category. The model therefore allows the association of income to vary over the range of income and the coefficients on quintile 1, 2, 4, and 5 show the effects of shifting the income of a respondent to that quintile from the baseline category of the third quintile. In addition to the income dummies, all models include a continuous, squared, measure of age, a dummy for gender and a factor variable for education. Furthermore, a dummy for full-time employment, a dummy for living in a rural area, and a dummy for religiosity, are included where they are available, as well as

[5] Ideally, one would specify a full model including the same variables for each election, but due to differences in covariate availability, doing so is not possible. However, unsurprisingly, variables that are not deemed important for vote choice in given countries are often not asked in surveys in those countries, suggesting that this might not be as problematic as it seems.

a control for language in the samples for Belgium, Canada, Israel, Switzerland, Spain, and Ukraine; region for Canada, Germany, the United Kingdom, and the United States; and race for Mexico and the United States.

Estimating country-level models

The third step involves estimating a separate multinomial discrete choice model for each election. Following Duch and Stevenson, I use a multinomial logistic (MNL) model, which allows one to simultaneously estimate the probability of each respondent voting for each possible party. A possible downside to using the MNL model is its assumption of the independence of irrelevant alternatives (IIA), which stipulates that the addition or removal of an alternative from the choice set does not affect the relative probability of choosing between existing choices. In the context of unconditional vote choice this is potentially problematic, since intuitively one would, for example, presume that adding an additional liberal party to the choice set would draw more supporters from existing liberal parties than existing conservative parties and, thus, violate IIA (Alvarez and Nagler, 1998).

However, as Train (2003) points out, and Duch and Stevenson (2008:50fn) emphasize, IIA applies *conditional* on the variables in the model. A well-specified model of vote choice should thus not violate IIA, a condition that further supports the use of country-specific covariates in our models.[6] To prevent separation (i.e. that some predictors perfectly predict some outcomes), I drop all parties from the sample that are supported by less than 50 respondents or less than 5% of the sample. I also limit the sample to only those respondents who voted. Both restrictions are in line with the approach of Duch and Stevenson.

[6] In any case, I use a Hausman-test for each MNL-model to test whether IIA is violated.

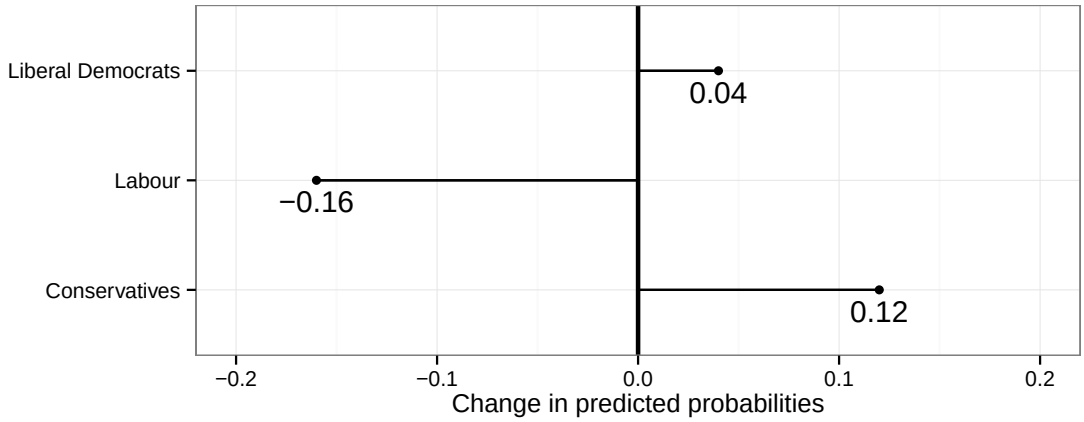
Constructing a country-level measure of income-based voting

Having estimated separate MNL-models for each election, step four involves constructing a measure of income-based voting that is comparable across country-elections. To do so, I use the estimates from the MNL models to produce predicted changes in support for each party for each individual when income changes by a given amount. While Duch and Stevenson (2008) simulate a one unit change on a three point scale towards a worsening economy, I simulate all possible two quintile changes on a five quintile scale. Thus, for example, for an individual with income in the first quintile, the simulation changes his income to the third quintile, and for an individual with income in the fourth quintile, the simulation changes his income to the second quintile. For individuals with income in the third quintile, the simulated changes in predicted probability are the average of a shift to the first and fifth quintile.

Importantly, the simulations are based on the covariate profiles of actual respondents in the sample, rather then a hypothetical average individual. Thus, I set the covariates to the values for the first individual in each sample, simulate a change in his income in either (or both) directions, and sum the absolute differences in the probability of voting for each party. Following Duch and Stevenson (2008:51), I repeat this procedure a thousand times for each individual, drawing the parameters used in each repetition from a multivariate normal distribution, based on the estimates from the model.[7] The resulting measure sums up the absolute value of the change in predicted probability of voting for each party induced by the shift in income. This process is repeated for every individual in the sample, with the overall average giving me a single summary index of how sensitive the voting

[7] I perform the simulated changes in predicted probabilities using the `Clarify`-package for Stata (King, Tomz, and Wittenberg, 2000).

behavior of individuals are to changes in income for each election in the sample. For illustrative purposes, figure 2.2 shows the calculation of the statistic for a randomly chosen individual with income in the second quintile in the 1997 elections in the United Kingdom.



Note: The figure shows the change in predicted probability of voting for each of the three main parties in the 1997 UK elections associated with a shift in income from the second income quintile to the fourth income quintile, for a specific individual in the sample (confidence intervals omitted for clarity). For each individual, the absolute values of predicted changes are summed up (0.04+0.16+0.12=0.32) and divided by two (0.32/2=0.16). This particular individual's income volatility is 0.16*100=16. This procedure is then repeated for each individual in the sample, with the overall average giving a summary measure of income-based voting for each election.

Figure 2.2 – Measuring Income-based Voting: an example from the 1997 United Kingdom elections

The resulting measure is analogous to the commonly used Pedersen index (Pedersen, 1983) for electoral volatility in party systems, but differs in two aspects: It sums over voters, not parties; and the change (volatility) being estimated is the change in the predicted probability of voting for each party, rather than the change in parties' vote share between elections. More formally, the measure can be defined as

$$IBV_j = \frac{\sum_{i=1}^{N} \frac{\sum_{k=1}^{M} | p_{ikq} - p_{ik(q\pm2)} |}{2}}{N} \times 100 \quad (2.1)$$

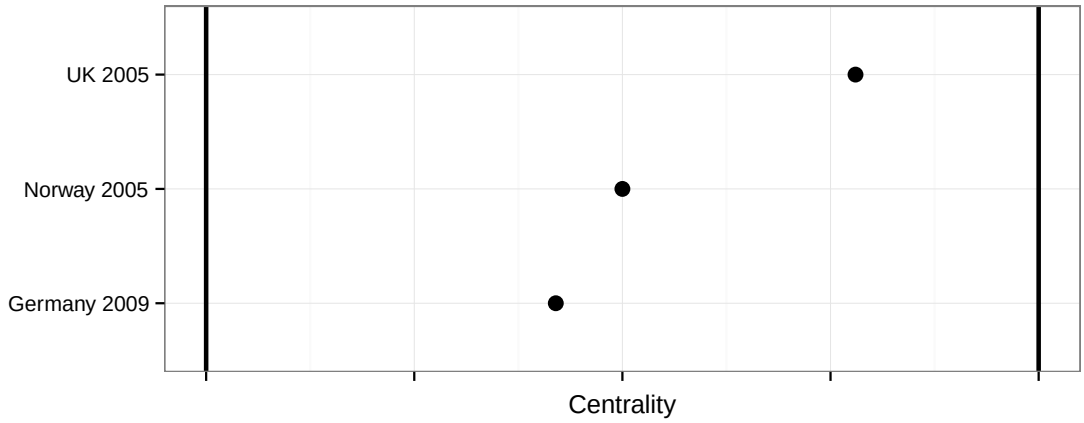
where j, k, i and q are subscripts for country, party, voter and income quintile, respectively. p_{ikq} gives the predicted probability of voter i in income quintile q of voting for party k, while $p_{ik(q\pm2)}$ gives the predicted probability of voting for party k with the simulated change in income. For each voter, the absolute values of the difference between the two is summed over all M parties and dived by two, with the average then taken over all voters and multiplied by 100.

This measure captures what I call income-based voting, and is comparable across countries. The measure is distributed fairly normally and has a mean value of 15. Of the 97 elections for which the measure is available, USA in 2004 has the lowest value (7), while the Slovak Republic in 2010 has the highest value (27).

Middle-Income Asymmetry in Income-based Voting

Because the hypothesis derived from the theory proposed by Iversen and Soskice (2006) does not make predictions about the mean value of income-based voting across electoral systems, but rather how middle-income individuals vote in relation to low- and high-income voters, it is necessary to decompose the measure to test the hypothesis. That is, the country-level estimates of income-based voting developed above are broken down into two parts — one that captures the effects on voting of shifting the income of an individual in the third quintile to the first quintile (middle⇒low), and another that captures the

effects of shifting the income of an individual from the third quintile to the fifth quintile (middle⇒high).



Note: The figure shows three examples of the centrality of middle-income voters compared to low- and high-income voters. In the 2005 elections in the UK, the voting behavior of middle-income voters was more similar to voters with higher income, while the opposite held true in the 2009 German elections. In the 2005 Norwegian elections middle-income voters were equidistant in their voting behavior from low- and high-income voters.

Figure 2.3 – Centrality of Middle-Income Voters

Taking the ratio of these two measures, although quite abstract, captures the "centrality" of middle-income voters as compared to either end of the income distribution. Thus, for example, a ratio of 1 would suggest that the total shift in predicted probabilities from changing income from the middle income category to either low or high income would be the same. A ratio of 2 would suggest that a shift from middle to high income would involve a two times larger change in the predicted probability of vote choice than a corresponding shift from middle to low income. In that case, middle income voters would in some sense be "farther away" from the voting behavior of high income voters than from that of low income voters. Iversen and Soskice's (2006) theory suggests that in a majoritarian system,

the voting behavior of middle income voters should be more skewed toward that of high income voters than in proportional electoral systems. The ratio measure should thus be lower in countries with a less proportional electoral system, reflecting that middle income voters align themselves with high income voters in majoritarian systems and the distance between them is thus shorter than the distance between middle income voters and low income voters.

2.2.2 Operationalizing Electoral Systems

The primary independent variable of interest is the electoral system, which is generally operationalized in one of two ways. In many cases, authors simply use a dummy variable that distinguishes majoritarian electoral systems from proportional electoral systems, often adding another dummy variable that further distinguishes mixed systems, such as the German electoral system, from both majoritarian and proportional systems. Others prefer to utilize a continuous measure of average district magnitude, which generally ranges from 1 for majoritarian electoral systems, up to 150, for the Dutch proportional electoral system (e.g. see Persson and Tabellini, 2003; Iversen and Soskice, 2006; Huber and Stanig, 2009).

While neither measure is perfect, district magnitude better captures the theoretical mechanisms being tested. Following Duverger (1963), larger district are claimed to be more permissive than smaller districts, which implies that more parties should be electorally viable in larger districts. This is, for example, the mechanism that Iversen and Soskice's (2006) theory is based on, and it is thus an appropriate measure to use in the context of testing their theory. District magnitude is, furthermore, approximately inversely related

to district size, which is the mechanism that Persson and Tabellini's (2003) theory, for example, is based on.

In my main empirical specifications, I use the log of average district magnitude, since one would expect that the marginal effects of magnitude should be declining — i.e. a shift from 1 to 2 average seats in a district should be more consequential than a shift from 51 to 52 seats. Furthermore, from an empirical perspective, average district magnitude is quite right skewed, and taking the natural log reduces that skew.[8]

The primary measure of electoral systems I use is thus

$$\ln(\text{Magnitude})_j = \ln\left(\frac{\text{Seats}_j}{\text{Districts}_j}\right) \times 100 \tag{2.2}$$

where j subscripts countries, Seats are the number of seats in the lower house of the country's legislature and Districts are the number of electoral districts for the lower house.[9]

2.2.3 Operationalizing Other Variables

To fully test the hypothesis specified earlier, I also need a measure of an alternative salient political cleavage, as well as controls for factors that might plausibly confound the relationship between our primary explanatory variable and income-based voting.

[8] I also estimated all the models shown in the main text with unlogged average district magnitude and indicator variables for PR and mixed electoral systems. The results are shown in table A.3 in the appendix.

[9] In the case of mixed electoral systems, I take the weighted average of district magnitude across the different tiers of the electoral system. For example, in the German 2009 elections, 299 legislators were elected through 299 single member districts (average district magnitude=1), while another 299 legislators were elected through 16 state level districts (average district magnitude=18.7) . The weighted average district magnitude for the electoral system is, thus, 9.8.

While several possible cleavages could be included in the analysis, I opted for using Alesina et al.'s (2003) measures of ethnic fractionalization and religious fractionalization, since those two dimensions have most often been associated with the logic of Roemer's (1998) argument. The measures are based on the Herfindahl-Hirschman index and capture the probability that two randomly selected individuals from a country belong to different ethnic/religious groups. Higher values imply higher fractionalization.

There are several plausible alternative explanations for differences in income-based voting across countries. Most fundamentally, one could argue that unions and other class-based instruments of collective action might increase the salience of redistribution for voters and, thus, polarize them along the lines of income. I capture these effects with a control for unionization rates. Alternatively, it might well be the case that increased income inequality had the same effect, such that more inequality would lead to more income polarization in voting behavior. I control for income inequality with the GINI coefficient of disposable income in the year nearest to the election.

Due to the diverse set of countries in the sample, I also include controls for economic development (GDP per capita in PPP terms, logged) and for the level of democracy (Polityscore). I also control for executive type with a dummy variable for presidential democracies. A table in the appendix shows descriptive statistics for all variables, as well as their sources.

2.2.4 Estimation

I estimate a model of the general form

$$Y_i = \beta_0 + \beta_1 \ln(\text{Magnitude})_i + \sum_{k=1}^{N} \beta_k Z_{ik} + \beta_j Z_i \times \ln(\text{Magnitude})_i + \epsilon_i$$

where Z_k includes control variables and $Z_i * \text{Ln}(\text{Magnitude})_i$ is included in several specifications that include interaction terms between the electoral system and other factors. Since the dataset contains multiple election-years for many countries, I cluster standard errors by country.

2.3 Results

The results for income-based voting are shown in table 2.1. The first model presents the baseline specification, the second model includes an interaction term between ethnic fractionalization and the electoral system, the third model includes an interaction term between religious fractionalization and the electoral system, while the fourth model includes a measure of unionization and income inequality.

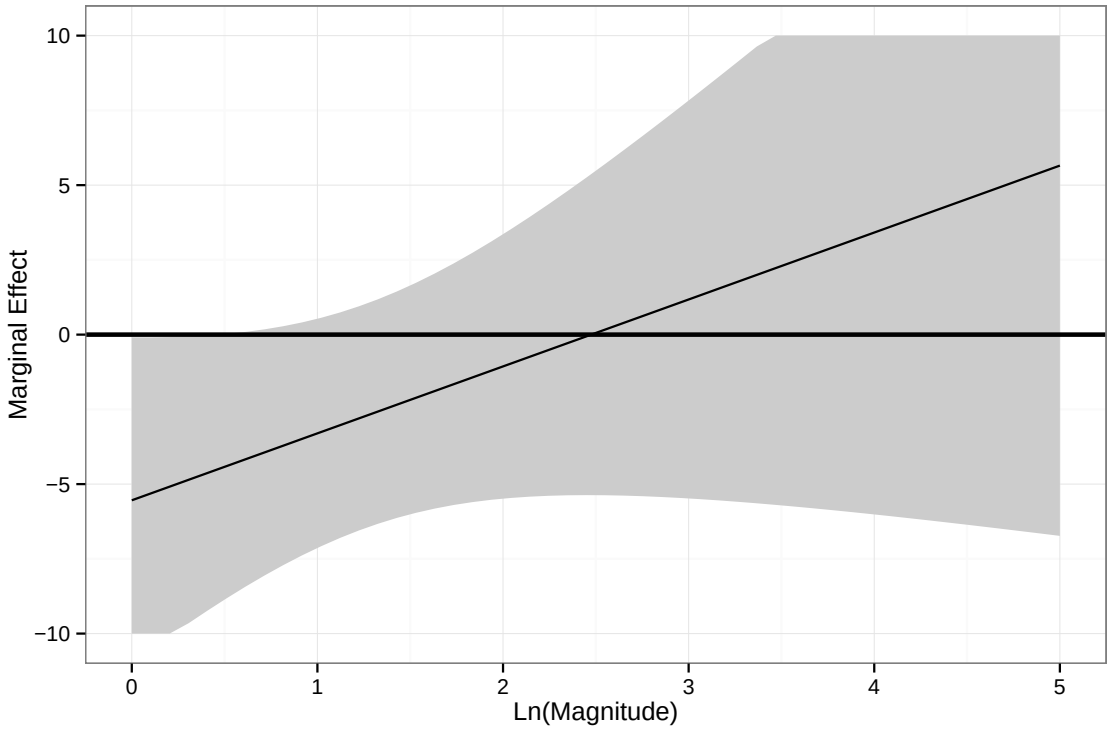
In model 1, the baseline specification, the association of the electoral system (LN(MAGNITUDE)) with income-based voting is significant, both statistically and substantively. The model suggests that a shift from a typical majoritarian electoral systems to the mean value for PR systems, is associated with a 2.7 point increase of income-based voting, which is equivalent to 68% of the standard deviation of the measure for income-based voting. This result strongly supports the micro-level logic behind Persson and Tabellini's (2003) macro-level empirical results. The measure of the electoral system continues to be significant in the other three specifications.

	Model 1	Model 2	Model 3	Model 4
Constant	7.19	5.76	7.10	5.57
	(6.64)	(6.22)	(6.73)	(8.68)
ln(Magnitude)	1.08**	1.08**	1.36**	1.07*
	(0.52)	(0.46)	(0.54)	(0.55)
Presidentialism	0.12	0.03	−0.01	0.19
	(1.41)	(1.39)	(1.61)	(1.62)
Polity	0.75	0.84	0.70	0.62
	(0.88)	(0.85)	(0.92)	(0.83)
ln(GDP)	−0.70	−0.48	−0.76	−0.45
	(1.01)	(1.00)	(1.13)	(1.11)
Ethnic Fractionalization	−1.99	−5.54*	−2.02	−2.01
	(2.30)	(3.28)	(2.49)	(2.58)
ln(Magnitude)×EF		2.24		
		(1.90)		
Religious Fractionalization			4.23	
			(4.46)	
ln(Magnitude)×RF			−2.52	
			(1.94)	
Unionization				4.10**
				(1.92)
Gini				0.02
				(0.12)
R^2	0.14	0.15	0.17	0.21
Num. obs.	97	97	97	77

**$p < 0.05$, *$p < 0.1$. Robust standard errors clustered by country in parentheses.

Table 2.1 – Income-based Voting Results

Model 2 tests the proposition that the electoral system conditions the effects of a possibly salient non-redistributive cleavages — ethnic fractionalization — on income-based voting. To better gauge whether the conditioning effect is strong, figure 2.4 shows the effect of ethnic fractionalization on income-based voting over the range of electoral system magnitudes. In the case of majoritarian electoral systems (with a logged district magnitude of



Note: The graph is based on results from model 2 in table 2.1. The shaded area shows the 90% confidence interval around the estimate.

Figure 2.4 – The Marginal Effect of Ethnic Fractionalization on Income-based Voting, Conditioned by the Electoral System

0), more ethnic fractionalization, is associated with considerably less income-based voting, while it does not have a statistically significant effect on income-based voting in the context of electoral systems with larger district magnitudes. This is in line with hypothesis 3 – while majoritarian systems lead to a "forced choice" between alternative political dimensions in vote choice, proportional systems do not have the same effect. It should be noted, however, that the results are just barely significant at a 10% significance level. Model 3, however, which tests whether religious fractionalization has similar effects, does not support the hypothesis.

2.3.1 What about middle-income voters?

The above models have tested the extent to which the mean-level of income-based voting can be explained by various macro-level factors, such as the electoral system. However, Iversen and Soskice's (2006) theory speaks to the behavior of middle-income voters and, in particular, how similar or dissimilar their voting behavior is to that of voters with low- and high income, respectively. In table 2.2, I show the results from a test of their theory, where the dependent variable captures the centrality of the voting behavior of middle-income voters, with higher values suggesting more similarity with the voting behavior of low-income voters.

Iversen and Soskice's (2006) theory predicts that under majoritarian electoral institutions, middle-income voters should be closer in their voting behavior to individuals with high-income, rather than low income. However, the results do not support their hypothesis — indeed, the coefficients even have the opposite sign to what is predicted by their theory.

2.3.2 A Closer Look at Mixed Electoral Systems

A lurking concern with any statistical analysis, as the one conducted above, is that the model specifications have not fully accounted for important factors that might confound the relationship between the primary variables of interest. In the present context, one might be particularly concerned about electoral institutions once being the subject of political choice themselves, as well as electoral institutions being highly correlated with other country-level factors, such as interest group structure or salient non-redistributive cleavages (e.g. Boix, 1999; Cusack, Iversen, and Soskice, 2007).

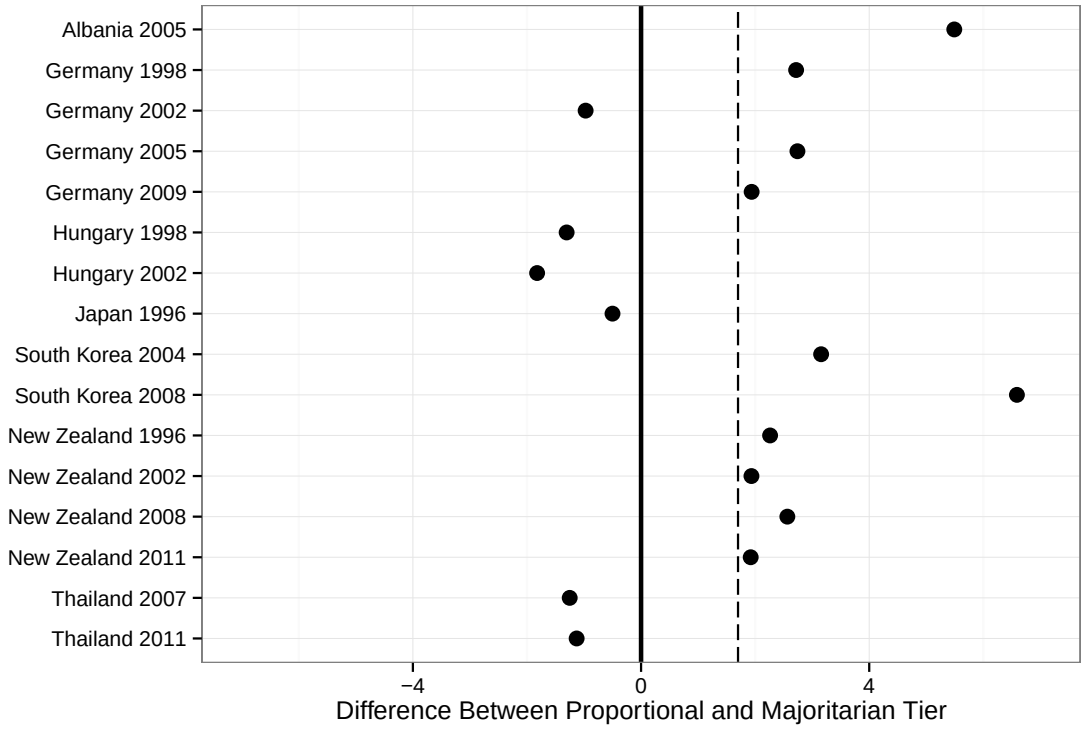
	Model 1	Model 2	Model 3	Model 4
Constant	−3.46	−2.07	−2.76	−2.17
	(6.48)	(5.58)	(5.74)	(5.91)
ln(Magnitude)	−0.40	−0.40	−0.22	−0.50
	(0.30)	(0.30)	(0.20)	(0.33)
Presidentialism	1.99	2.08	1.80	2.86
	(1.80)	(1.73)	(1.58)	(2.42)
Polity	0.37	0.28	0.42	0.50
	(0.57)	(0.50)	(0.53)	(0.60)
ln(GDP)	0.82	0.61	0.33	0.73
	(0.78)	(0.64)	(0.54)	(0.72)
Ethnic Fractionalization	1.84	5.30	1.13	0.77
	(1.92)	(5.24)	(1.90)	(1.96)
ln(Magnitude)×EF		−2.18		
		(2.04)		
Religious Fractionalization			4.77	
			(4.46)	
ln(Magnitude)×RF			−1.31	
			(1.57)	
Unionization				−3.29*
				(1.87)
Gini				−0.05
				(0.08)
R^2	0.17	0.20	0.21	0.23
Num. obs.	97	97	97	77

**$p < 0.05$, *$p < 0.1$. Robust standard errors clustered by country in parentheses.

Table 2.2 – Centrality of Middle-Income Voting Results

In an effort to strengthen the inferences drawn about the effects of electoral institutions on income-based voting, I take advantage of the fact that sixteen elections in seven countries in the sample took place under mixed electoral systems, where instead of voting for a single candidate or party, voters had two ballots to cast, one in a majoritarian tier and another one in a proportional tier. Importantly, by pairwise comparing the extent of income-based voting under each tier, I am able to control for a host of unobserved factors

that might confound the relationship being studied. Any differences between income-based voting across the tiers can thus be attributed to differences in electoral coordination between parties and voters caused by the electoral rules governing each tier.



Note: Each country-election is centered on income-based voting in the majoritarian tier. The dashed line shows the overall mean difference.

Figure 2.5 – Income-based Voting in Mixed Electoral Systems: Proportional Tier Relative to Majoritarian Tier

This test is in many ways biased against finding any differences between the systems. Since both systems operate in parallel in the same country, there is bound to be contagion between them, such that the effects of the electoral system on vote choice is attenuated. This could, for example, be caused by legislators switching between tiers between elections, the popularity of national party leaders affecting district elections, or anything

that in principle blurs the distinction between the two tiers in the minds of voters. This is especially so for mixed systems which have tiers that depend on each other, such that the results from one tier are adjusted to the other tier.

Figure 2.5 shows the mixed electoral systems in the sample and the extent of income-based voting in the proportional tier of each system compared to the baseline of income-based voting in the majoritarian tier. One the one hand, Albania, Germany, and New Zealand, each have mixed systems classified as corrective by Massicotte and Blais (1999), since the allocation of seats in the proportional tier is influenced by the results of the majoritarian tier. On the other hand, Japan, South Korea, and Thailand have a system classified as a superposition system, where each tier is unrelated to the other. Finally, Hungary has a system that has both corrective and superposition features (Massicotte and Blais, 1999). A legitimate concern in the following analysis is that the countries are extremely dissimilar and that it is, therefore, difficult to draw parallels between them, even though they nominally have similar electoral institutions. While I grant that concern, it should be noted that the comparison being made is between systems within countries, rather than across countries, which reduces the effects of unit heterogeneity.

As the figure shows, there tends to be more income-based voting in the proportional tier of mixed systems, which is in line with the results from the previous section. As is to be expected, the difference persists across elections in the same country, with the difference in Albania and South Korea being especially pronounced, while the difference is negative for six of the cases.

Is the difference statistically significant? Table 2.3 shows the results of three different tests of paired differences, with all tests leading to similar inferences. The first line shows

Sample	N	Paired t-test	Wilcoxon Sign Rank test	Fischer-Pitman Permutation Test
Country-Election	16	0.014	0.008	0.013
Country-Election Clustered by Country	16	0.052		
Country	7	0.099	0.088	0.109

Note: The cells show p-values from the associated one-sided tests.

Table 2.3 – Difference between Income-based Voting in Proportional and Majoritarian Tiers in Mixed Electoral Systems

the results of the tests when each election is treated as independent of the other elections in the sample. As the figure above suggests, this approach leads to overconfidence, since income-based voting tends to persist across elections within countries. At the opposite end, the last line accounts for the within-country correlation by aggregating the differences to the country level, which leads us to unnecessarily discard possibly useful data.

The ideal tests, shown in the middle row, analyze the data at the election level, but accounts for the country-level clustering. The paired t-test, clustered by country, suggests that the difference is significant at the 10% level, while the bounds of the Wilcoxon sign rank test and the Fischer-Pitman permutation test suggest similar results. While these results are based on only a handful of countries, they suggest that the tendency of proportional tiers to be associated with more income-based voting than majoritarian tiers in mixed system is not due to chance, but rather due to a systematic difference between the two.

2.4 Conclusion

In recent years a number of scholars have developed nuanced theories to explain the association of majoritarian electoral systems with less generous welfare spending. The theories offered are quite distinct in that they offer different mechanisms by which electoral systems might systematically affect redistributive outcomes. These mechanisms have, among other things, highlighted the possible role of intrinsic features of electoral systems, such as district size and district magnitude, as well as the possibility that features of the social structure of a country, such as ethnic fractionalization, might interact with the electoral system to create the observed empirical phenomena.

Generally, such theories have been tested, and found support, at the macro-level. However, such empirical evidence suffers from two main problems. First, the theories in question are developed to explain a known empirical phenomenon and as such it should perhaps not be surprising that they succeed in doing so. Second, the macro-level relationship between electoral systems and redistributive outcomes is consistent with a number of different theories, as well as the possibility that the relationship is driven by some third, unobserved factor.

In this paper, I go beyond previous empirical tests of the theories by leveraging the fact that each of the theories makes predictions about the behavior of parties and voters under alternative electoral systems. I bring new evidence to bear on each of the theories, and as such put them to a more stringent test than previous efforts. While I acknowledge that analyzing the micro-level alone has weaknesses of its own, the approach serves to reinforce the macro-level empirical strategies that dominate the literature. Theories that

pass such "triangulation" tests, that is, theories that find support in data from different levels of analysis, should have a stronger empirical foundation than those that do not.

Persson and Tabellini's (2003) claim that the electoral system is consequential for the welfare state finds robust support in the data. Simply put, voters in majoritarian systems are less likely to be polarized over redistributive issues. However, Iversen and Soskice's (2006) more nuanced theory of how the voting behavior of middle income voters differs across electoral systems does not finds support in the data. Finally, Roemer's (1998) suggestion that majoritarian electoral systems lead to a "forced choice" between alternative political cleavages finds mixed support in the data.

In addition to my main analysis, I also showed that the expected relationship between income-based voting and the electoral system holds when I restrict the sample to the sixteen elections in mixed electoral systems included in the data. While any conclusions drawn from such a small set of observations should be tentative, the strength of the results are nonetheless impressive, given that the two systems operate side by side in the same context, increasing the possibility of attenuating any differences in income-based voting across them.

More generally, I have extended Duch and Stevenson's (2008) approach to explain differences in the association of income and vote choice across countries. The method could be fruitfully applied to analyze a number of different political dimensions, which currently are only analyzed with less than ideal methods. A fruitful — and necessary — area for future research would be to analyze the statistical properties of the method more thoroughly, particularly the properties of the country-election level measure of income-based voting. Informal observations suggest that the measure might be sensitive to the sample

size of units and the specification of the country-election models. This is an area ripe for methodological innovation.

Finally, I have suggested that mass-level data can be used to examine the extent to which parties and voters coordinate over different issues in elections. The present paper only develops the argument in the context of income and redistribution, but it could easily be adapted to other political cleavages of interest to scholars of voting behavior, party politics, and the effects of political institutions on the electoral coordination between voters and parties.

Chapter 3

Are We in the Same Boat or Not? The Opposite Effects of Absolute and Relative Income Shifts on Redistributive Preferences

Do the effects of economic mobility on redistributive preferences differ from the effects of macroeconomic cycles on redistributive preferences?[1] Within political science, the literatures on these two closely connected relationships have so far failed to speak to each other. The literature on the effects of economic mobility generally adopts a standard political economy approach, assuming that individuals faced with the prospect of moving up or down the income distribution will demand less or more social insurance, respectively (Bénabou and Ok, 2001; Rehm, Hacker, and Schlesinger, 2012). Contrast this with

[1]Co-authored with Vittorio Merola. We acknowledge support from The Ohio State University's Behavioral Decision Making Initiative. We also thank Janet Box-Steffensmeier, Sarah Brooks, Raphael Cunha, Lucy Goodhart, William Minozzi, Irfan Nooruddin, Philipp Rehm, and Wei-Ting Yen, as well as participants at the Seventh Annual NYU-CESS Experimental Political Science Conference, in March, 2014, and participants at the Annual Meeting of the Midwest Political Science Association, in April, 2014, for their comments and suggestions.

the literature on macroeconomic cycles, which is generally framed in terms of the public's policy mood and treats redistribution, and welfare policy more generally, as a luxury good. During times of a growing economy, individuals will demand more of the luxury good, while during times of a struggling economy, individuals demand less of the luxury good (Durr, 1993; Stevenson, 2001). Interestingly, the two separate literatures, thus, make opposite predictions about the effects of income shifts on redistributive preferences.

How do we reconcile such conflicting theories? We argue that the reason for these opposing results is due to the crucial distinction between absolute and relative income shifts, both of which are produced during economic cycles, yet rarely separated conceptually or empirically.[2] It is this crucial distinction that separates political economy perspectives on redistribution, which prioritize the role of relative income (based on the basic Meltzer-Richard logic), and public opinion perspectives on social policy preferences, which emphasize the dominant role of absolute income (based on the idea of leftist policies as luxury goods). The political economy perspective emphasizes the importance of the inequality of risk and income (relative income), while the public opinion perspective emphasizes the importance of wanting to help others, as a luxury good (absolute income). We argue that these separate strands of the literature can be combined in a more general theoretical framework, which accounts for empirical findings on both the effects of economic mobility and macroeconomic cycles on redistribution. This framework builds on recent work on perceptions of economic change (Duch, Palmer, and Anderson, 2000; Stevenson and

[2]An absolute income shift is a proportional increase or decrease in income which affects everyone equally (meaning an inequality- and rank-preserving income shift). A relative income shift, meanwhile, is a proportional increase or decrease in income which varies in size across individuals, such that the relative income rank of individuals also changes.

Duch, 2013), social affinity (Barth, Finseraas, and Moene, n.d.; Lupu and Pontusson, 2011), and other-regarding preferences (Lü and Scheve, 2014).

Fundamentally, we argue that different types of income shifts trigger and make salient differences or similarities among individuals, thus increasing or decreasing in-group solidarity, which affects people's aversion to inequality. Absolute income shifts signal to people that they are in the same boat, which leads to an increased feeling of "we-ness". Under such conditions, inequality aversion becomes a stronger motivation, as divisions within the group are less desired. Such an aversion, however, is conditional on the satisfaction of absolute material needs, thus giving it the character of a luxury good. Conversely, relative income shifts make differences between members of society more salient. Under such conditions, people fend for themselves more and self-interested income maximization more uniformly shapes preferences. Thus, absolute and relative income shifts have opposite effects on redistributive preferences.

We use an experimental design, borrowing on recent innovations in experimental political economy (e.g., Barber, Beramendi, and Wibbels, 2013), to demonstrate that while both the absolute and relative income perspectives are correct, neither theory is capable of explaining the other's results. The design centers on a novel "redistribution game", in which a group of subjects earn income through a real effort task and then vote on a group-wise tax rate. Crucially, the chosen tax rate is "sticky", in that it also applies to a second stage real effort task. The experimental manipulation involves altering the expectations subjects have about their future income, thus isolating the effects of different types of income shifts on self-interested (maximizing one's own income across both rounds) and inequality averse (decreasing post-transfer inequality across both rounds) tax choices.

This is achieved while controlling for a series of relevant factors, which are difficult to properly account for with existing survey data.

While we carefully isolate the role of income, which is known to have an important effect on redistributive preferences across contexts (Gelman et al., 2009; Huber and Stanig, 2009), and emphasize the importance of in-group solidarity and inequality aversion, we are under no illusions that no other factors matter. As such, our argument is emphatically not a rejection of the potential role of other non-economic issues, beliefs about economic fairness, consequences of existing policies, or the independent effect of political partisanship and incumbency. Such factors clearly also help explain people's redistributive preferences, and the effects of economic mobility and macroeconomic cycles on public opinion (Alesina and Giuliano, 2011; Wlezien, 1995). Instead, our goal is to explain the direct consequences of shifts in absolute and relative income, a central effect of macroeconomic cycles, which, through their effects on social affinity and the satisfaction of material needs, we show can account for two different phenomena that have so far not been connected. In doing so, we also advance our understanding of the variation in inequality aversion across contexts, a previously understudied phenomenon (e.g., Engel, 2011; Osberg and Smeeding, 2006), as well as highlight the importance of better measuring and conceptualizing economic mobility (e.g., Clark and D'Angelo, 2013), and identifying the reference point, or the "local information environment", that people use when forming perceptions and opinions on economic change (e.g., Ansolabehere, Meredith, and Snowberg, 2014).

3.1 Relative Income and the Prospect of Economic Mobility

The political economy literature on economic inequality has centered on the median voter model of Meltzer and Richard (1981), which assumes that the redistributive preferences of any particular individual will be determined by their position in the income distribution, where economically self-interested individuals below average income will prefer some positive level of income redistribution. In short, people's relative income in society overwhelmingly determines their redistributive preferences. To date, the Meltzer-Richard model has dominated political economy thinking on redistribution.[3]

A critical assumption of the model is that individuals have perfect information about their position in the income distribution and that expectations of the future have no bearing on preferences. The static nature of the model has received considerable attention, with a number of scholars advancing a position that integrates expectations of future economic mobility into the baseline model. On the one hand, this has resulted in the literature on the prospect of upward mobility (POUM), which captures the self-interested logic that the more people expect to be rich in the future, the less they want to tax the rich right now (Bénabou and Ok, 2001; Ravallion and Lokshin, 2000). On the other hand, the prospect of downward mobility (PODM) introduces the self-interested desire to insure against the possible loss of future income, such that those with higher economic insecurity will prefer more redistribution right now (Iversen and Soskice, 2001; Moene and Wallerstein, 2001; Rehm, 2009).

[3]See Alesina and Giuliano (2011) for a recent review of the literature.

Thus, while there is strong evidence that redistributive preferences are not static, there has been little work analyzing the effects of macroeconomic cycles on redistributive preferences, despite income shifts strongly affecting expectations of economic mobility (Duch and Stevenson, 2010; Hirschman, 1973). In fact, the political economy models discussed above would hardly predict any relationship between absolute income shifts and redistributive preferences.[4] The intuition is straightforward: while having more or less income than you did in the past might slightly affect your desire for income or insurance, and thus would marginally affect your support for a redistributive policy, this effect is trumped by how much you stand to gain or lose from such a policy.[5] This net benefit is overwhelmingly determined by whether you are a net contributor or net beneficiary to the policy, which depends on your relative income (or risk). Ultimately, people are driven by a desire to maximize their income, but indirectly form policy preferences by determining if they are rich or poor through a comparison with others. A desire to help others less fortunate, or an aversion to inequality, is absent in these models.

3.2 Absolute Income and the Public's Policy Mood

Scholars studying public opinion in the American context have established a connection between economic fluctuations and the public's "policy mood", meaning the public's support for more or less government activism, such as government spending on social policy, education and health care (Stimson, 1999). The support for liberal policies, thus, is similar to the support for income redistribution, as it largely focuses on redistributive policies.

[4]There are exceptions to this. For example, if an economic recession increases overall unemployment and increases the risk of income loss for most citizens, then the literature would predict an overall increase in the demand for social insurance and redistribution (e.g., Rehm, Hacker, and Schlesinger, 2012). However, as income and risk are negatively associated, the demand for social insurance would unequivocally decrease with an absolute increase in income.

[5]A recent exception in this literature is Rueda (2014).

Durr (1993) finds that liberal policy mood in the United States is positively associated with expectations of economic conditions in the future. Since the latter is closely connected with economic conditions, the implication is that expected economic growth is positively associated with a liberal policy mood among the public. Stevenson (2001) expands this result to other countries, finding the same pattern across fourteen western democracies.

The explanation for this general finding rests on the assumption of the diminishing marginal utility of income and wealth, and that liberal policies are "luxury goods" which are increasingly demanded as one's own basic needs are satisfied (Durr, 1993; Stevenson, 2001). Borrowing from Maslow's (1970) Hierarchy of Needs, the idea is that only once people feel satisfied with their economic condition will their need for security be met, which is required for them to pursue higher goals, such as the welfare of others. Compared to the political economy models, the predicted relationship between economic insecurity and redistributive policy preferences is now negative, not positive, as economically insecure individuals do not support liberal policies. In other words, the policy mood model disregards the role of inequality and relative income, such that if actors are poor or rich, or risk becoming poorer or richer in the future, is irrelevant to their predicted level of support for liberal policies.

Recent work finds mixed evidence for the policy mood argument. While De Neve (2014) finds that economic growth is positively associated with more liberal views among voters in the United States, and Kayser (2009) finds that aggregate unemployment is associated with more conservative party support among European voters, other evidence indicates that higher levels of unemployment are translated into more liberal policy views, while higher levels of inflation are translated into more conservative policy views (Enns and

Kellstedt, 2008; Erikson, MacKuen, and Stimson, 2002). Meanwhile, Markussen (2008) finds that across 20 OECD countries both macroeconomic effects on policy mood receive support, depending on the time period analyzed. Finally, Ferguson, Kellstedt, and Linn (2013) extend Durr's (1993) empirical analysis of the American context by incorporating data until 2010 and find that neither prediction of macroeconomic effects on policy mood hold well in the larger time series, as there is no clear pattern in recent decades in particular. They explain these inconclusive results by emphasizing that different aspects of the economy vary in their salience across economic periods, while proposing that macroeconomic effects might be heterogeneous across different groups of the population.

We believe this is an important point that deserves emphasizing. The two broad literatures just described indicate that the predicted effects of a relative income shift on redistributive preferences are the opposite of the predicted effects following an absolute income shift. In other words, whether or not individuals compare their income gain or loss to others, as well as who those others are, might be critical for how it affects their desire for income redistribution. Such reference points and perceptions are rarely clearly identified, as economic mobility and expected changes in individual income are generally measured through self-reported intergenerational mobility (Alesina and Giuliano, 2011), unemployment risk (Rehm, 2009), and expectations of financial and economic change for one's household and country (Anderson, 2007), all of which fail to isolate relative and absolute expectations. Thus, it is possible that both perspectives are valid, but that either income shifts will be more or less widely shared across time periods, or that different types of income shifts will be more or less salient across periods (e.g., Graham and Pettinato,

2002; McCall, 2013). This has the potential of explaining these conflicting results, within one general framework.[6]

3.3 The Effects of Income Shifts on Redistributive Preferences

Our argument builds on recent work emphasizing the importance of group belongingness and social affinity in shaping redistributive preferences (Barth, Finseraas, and Moene, n.d.; Lupu and Pontusson, 2011; Shayo, 2009). Evidence shows that people are more likely to help members of their own group than members of other groups (Chen and Li, 2009; Habyarimana et al., 2007; Stürmer and Snyder, 2009), and that such an effect is driven in large part by a sense of belongingness to a common group, which is shaped by a feeling of common fate (Flippen et al., 1996). Such results persist across a range of tests and settings (Levine et al., 2005), and form the basis of the recent "social distance model" in political economy (Alt and Iversen, 2014). It centers on the notion that people are more concerned about the welfare of others the stronger their sense of belonging with them. Such "social affinity" is shaped by shared experiences and perceptions of similarity, and is positively associated with redistributive preferences toward such similar others (Lupu and Pontusson, 2011; Shayo, 2009).

But how specifically do such concerns for the welfare of others and self-interested motivation manifest themselves together with respect to redistributive preferences? We focus on the specific other-regarding preference of inequality aversion, and assume that individuals care about their own material self-interest, as well as being averse to an inequitable allocation of income (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999). Because such an

[6]While there are other important factors shaping policy mood, from a thermostatic relationship (Wlezien, 1995) to a partisan incumbent effect (Ferguson, Kellstedt, and Linn, 2013), these are less directly related to the purview of this study; the pure income effects of macroeconomic cycles. We do not deny that such effects are moderated by various factors. In fact, we believe these should be areas of future study.

aversion to inequality is fairly context-dependent (Fehr and Fischbacher, 2005; Fehr and Gintis, 2007), and can be expected to vary with income and levels of inequality (Engel, 2011; Korenok, Millner, and Razzolini, 2013), it is more likely to be affected by macroeconomic cycles than the closely-related concept of altruism, which is generally considered a more stable personality disposition (Batson, 1998; Fehr and Fischbacher, 2003).[7] Following Fehr and Schmidt's (1999) well-known formalization, we assume that the utility of players $i \in (1, ..., n)$ is given by

$$U_i(x) = x_i - \frac{\alpha_i}{n-1} \sum_{j \neq i} \max[x_j - x_i, 0] - \frac{\beta_i}{n-1} \sum_{j \neq i} \max[x_i - x_j, 0]$$

where $0 \leq \beta_i < 1$, $\beta_i < \alpha_i$, and x_i measures subject i's level of income.[8] The first term in the equation captures the standard self-interested logic of gaining utility from one's own income level; the second term captures the utility loss from what Fehr and Schmidt (1999) refer to as "disadvantageous inequality", that is to say, inequality in the presence of being worse off than others; the third term captures the utility loss from "advantageous inequality", that is to say, inequality in the presence of being better off than others. For our purposes, we assume that inequality aversion is a luxury good, meaning it increases with income.[9] Furthermore, we assume that the value of an equal distribution of income

[7] Altruism, in its strongest form, is simply a desire to increase the benefits accrued to other individuals instead of oneself, without any ulterior, self-centered, motives (Batson, 1998).

[8] Note that the model does not allow for voting on a tax rate, nor does it include a dynamic component to allow for uncertainty about future income. As such, we use the model for the heuristic purpose of demonstrating how inequality aversion enters the utility function of individuals and how social affinity might alter the effects of inequality aversion on behavior.

[9] The luxury good assumption is founded on rich empirical evidence. The share of household earnings given to charity increases in income in the United States (Andreoni, 2006) and the United Kingdom (Pharoah and Tanner, 1997), while childhood poverty is associated with lower prosocial behavior later in life (Brown and Lichter, 2005). Holland, Silva, and Mace (2012) find that the probability of someone posting a sealed and stamped letter left in the streets of London increases the higher the average income of the neighborhood,

increases the greater the perceived similarity (or social affinity) between an individual and other members of the group. This implies assuming that α_i and β_i are increasing with i's income, and that they are a function of external, contextual factors, such as a perceived common fate from shared income shifts.[10]

Given the above utility function, redistributive preferences in a static environment are a function of absolute income, inequality, and aversion to advantageous and disadvantageous inequality. Additionally, we assume that preferences depend on relative income, such that those with high income will prefer less redistribution than those with low income (for empirical validation of this assumption, see Huber and Stanig, 2009; Finseraas, 2009), and that the level of redistribution is determined with simple majority voting, with the median proposal winning, as in the Meltzer-Richard model.

Shifting from a static environment, where all of the above factors are known with certainty, to a dynamic environment, introduces uncertainty over the final distribution of income. Assuming that the chosen level of redistribution is "sticky", in that it also applies to future periods, the unknown future will affect current redistributive preferences, as people adjust their preferences to account for possible future economic mobility. While our claim does not limit itself only to expectations of future income shifts, we want to establish a baseline comparison between the static and dynamic environment for two reasons. First, like Durr's (1993) original contribution, we believe that economic cycles'

while Hoffman (2011) demonstrates that wealthier people were more likely to help Jews escape capture during the Holocaust. Using a dictator game, Chowdhury and Jeon (2014) find that increasing the show-up fee equally for the dictator and the recipient results in the dictator sharing a greater proportion of their endowment, consistent with this perspective. Recent work also indicates that altruistic preferences (Rueda, 2014) and non-economic concerns (Gelman et al., 2009) increase with income.

[10]Note that such assumptions would not change the implications of their model, as only corner solutions would result. For any $\alpha_i \geq 0$, individual i's utility increases with x_i and decreases with x_j, while i will maximize x_i if $\beta_i < 0.5$ and maximize x_j if $\beta_i > 0.5$ (and will be indifferent at $\beta_i = 0.5$). See DellaVigna (2009) for a useful generalization of this framework.

greatest effect on preferences occurs by shifting expectations of future income. Secondly, the literature has firmly established that redistributive preferences are dynamic in nature, meaning that the expectations of future income will always exert some influence on current preferences, hence they should be modeled and tested accordingly.

Assuming that individuals are moderately risk averse and there is a positive probability of experiencing an external shock and moving up or down the income distribution, redistributive preferences should converge somewhat, across income groups, as uncertainty is introduced. Individuals with higher than average income should prefer higher levels of redistribution, to insure against possible future income loss, while individuals below average income should prefer lower levels of redistribution, to allow for the possibility of future income gain (Hypothesis 1). Our main argument is that the effects of expected income shifts on redistributive preferences will be above and beyond these pure uncertainty effects, and that their effects will critically depend on whether they are relative or absolute in nature.

During periods of absolute income shifts, such as periods of economic growth or recession when everyone seems to benefit or suffer equally, the similarities across members of society will become more salient. In other words, people feel like everyone is on the same boat, and sharing equally in the pains or gains of economic conditions. A more apt metaphor, perhaps, is that of a "rising tide lifting all boats", and, conversely, that of a "falling tide lowering all boats". Clearly, such a context increases the perception of a common fate, which we know to enhance the sense of "we-ness" and produce in-group solidarity. Thus, absolute income shifts are expected to produce an environment of greater group identification and lower felt competition, which increases the concern for other's

welfare, meaning increases the aversion to inequality, and thus affects redistributive preferences. As Lindert (2004) highlights, the Great Depression and the post-war economic boom can be classified as approximating such downward and upward absolute economic shifts, respectively.

However, as we assume that inequality aversion is a luxury good, the effect of absolute income shifts should be asymmetric. This results from an increase in income producing a self-interested preference satiation, such that it enhances the probability of individuals feeling like their material needs are satisfied, which boosts the relative value (or utility) of equalizing outcomes in society. A decrease in income will only increase the relative value of maximizing one's own income, as it decreases the probability of individuals feeling like their material needs are satisfied. One can also think of these as psychological effects, where income shifts affect individuals' perceived goal-pursuits (Austin and Vancouver, 1996; Heath, Larrick, and Wu, 1999). Thus, individuals might feel like their goal (or need) for survival is threatened after an income loss, which makes this goal more cognitively salient and motivationally important. Alternatively, individuals feel more secure in their survival goal after an income gain, which makes other goals (such as inequality aversion) increase in salience and importance (Ermer, Cosmides, and Tooby, 2008).

Consequently, we expect upward absolute income shifts to increase inequality aversion. This should lead to higher redistributive preferences across income groups, with the effects being more pronounced among the "rich", since inequality aversion is a luxury good (Hypothesis 2). Conversely, the effects of downward absolute income shifts should differ between individuals above and below mean income. While low income individuals,

wanting to safeguard their own material position, should have higher redistributive preferences after a negative absolute income shift, high income individuals should have lower preferences (Hypothesis 3). Since safeguarding one's own material position becomes a more important concern for all income groups, redistributive preferences should differ more between the "rich" and the "poor".

In contrast, during periods of relative income shifts the similarities between members of society will become less salient. These are periods of economic growth and recession when people seem to benefit or suffer unequally, thus providing a growing sense of competition, as shifts in relative rank are zero-sum. Consequently, these are periods of lower group identification and connectedness in society, and we would expect self-interest to dominate redistributive preferences under such contexts, with a lower aversion to inequality.

The metaphor in these cases could be described as situations of a "rising tide only lifting some boats", or a "falling tide only lowering some boats". The last two decades (pre-2008) of western economic growth, periods of "inequitable growth" (McCall, 2013), suffering from "frustrated achievers" (Graham and Pettinato, 2002), can be thought of as a stylized example of the former phenomenon. In such a setting, the POUM and PODM dynamics take center stage. In the former case, a positive relative income shift should lower redistributive preferences (Hypothesis 4), while in the latter case, a negative relative income shift should lead to higher redistributive preferences (Hypothesis 5), across all income groups. Note that such expectations of relative mobility entail an expectation of changes to both absolute income and relative rank, meaning that the only difference between an

expected shift (in the same direction) of absolute income and relative income is the expected change to relative rank. It is this expected change which often remains unmeasured in survey data.[11]

In short, we are left with the following hypotheses, which our experiment seeks to evaluate:

H1 **Uncertainty effect**. Moving from a static to a dynamic environment should increase (decrease) redistributive preferences for those above (below) mean income.

H2 **Upward absolute income effect**. Higher expected absolute income should increase redistributive preferences, with a stronger effect for higher income ranks.

H3 **Downward absolute income effect**. Lower expected absolute income should decrease (increase) redistributive preferences for those above (below) average income.

H4 **Prospect of upward mobility effect**. Higher expected relative income should decrease redistributive preferences.

H5 **Prospect of downward mobility effect**. Lower expected relative income should increase redistributive preferences.

Comparing the predictions of our theory to predictions based on competing accounts reveals how we can evaluate them against each other. Table 3.1 displays the predictions,

[11]It is important to point out that while we expect that absolute (relative) income shifts will increase (decrease) the desire for equality, we remain agnostic as to whether these shifts have to be objectively absolute or relative, or whether they need only be perceived as such. We suspect that the latter is to some extent constrained by the former, consistent with the evidence on economic conditions more generally (Ansolabehere, Meredith, and Snowberg, 2014; Stevenson and Duch, 2013), but a greater outline of these differences, or the moderating factors shaping such perceptions, is beyond the scope of this paper.

comparing the theory developed in this paper with a generic political economy account, on the one hand, and a generic policy mood account, on the other hand.

	Income Shift			
Theory	Upturn	Downturn	POUM	PODM
Political Economy	-	-	$\Downarrow$	$\Uparrow$
Policy Mood	$\Uparrow$	$\Downarrow$	$\Uparrow$?	$\Downarrow$?
Our Theory	$\Uparrow$	$\Downarrow$ / $\Uparrow$	$\Downarrow$	$\Uparrow$

Table 3.1 – Comparing Theoretical Predictions for the Effects of Income Shifts on Redistributive Preferences

Unlike the political economy perspective, our theory makes clear predictions about the effects of positive ("Upturn") and negative ("Downturn") absolute income shifts. Conversely, our theory differs from the policy mood perspective, in that we make clear predictions about the effects of positive ("POUM") and negative ("PODM") relative income shifts, and we consider how relative rank affects preferences after negative absolute income shifts. Since the policy mood perspective mostly disregards positional concerns and relative economic mobility, the exact policy mood predictions are uncertain for relative income shifts, although the luxury goods arguments suggests that the effects of relative income shifts should not differ from absolute income shifts. It should be clear that we expect mostly opposite effects from absolute and relative income shifts in the same direction. To the extent that the empirical results line up with our predictions, our theory improves upon these current perspectives.

3.4 Research Design

As the goal of the present study is to provide clear evidence of the theorized opposite effects of absolute and relative income shifts, an experimental framework was employed. By using an experimental design, we are able to control for a variety of factors that are difficult to capture within an observational framework, while at the same time finely manipulating the absolute and relative income of subjects and directly observing the effects on redistributive preferences. The latter aspect is especially important to maximize internal validity and, thus, provide strong evidence for or against our theory.

There have been several different experiments specifically testing redistributive preferences in the tradition of the Meltzer-Richard model. Klor and Shayo (2010), and Cabrales, Nagel, and Mora (2012) both find clear evidence that tax votes (a measure of redistributive preferences) mostly follow a self-interested pattern. However, these studies focus on static, perfect information decisions, and only allow subjects to choose between two tax rates. A more general framework (which also conforms more directly to the theoretical models detailed above) would allow subjects to vote on any tax level, based on the median proposal being selected, as well as take into account expectations of future income, as we do here.

Three recent experiments take into account the dynamic nature of redistribution. Both Barber, Beramendi, and Wibbels (2013) and Durante, Putterman, and van der Weele (2014) make subjects vote on a tax before performing a real-effort task. The former find that as the risk of income loss increases, the tax vote increases, while the latter find a positive relationship between risk aversion and the tax vote under uncertainty. Although these results are important, a stronger result would elicit tax preferences after subjects have

earned their income through a real-effort task, yet have the tax be effective in future rounds as well. Esarey, Salmon, and Barrilleaux (2012) implement such a design, presenting results which are consistent with the findings above. In designing our experiment, we build on these important studies, while at the same time improving upon them, as detailed below.

3.4.1 The "Redistribution Game"

We designed the "redistribution game", using zTree (Fischbacher, 2007), with each experimental game proceeding in the following manner:[12] In each session, ten subjects were seated in front of computers. Each round of the game followed the same design, with subjects randomly (and anonymously) split into two groups of 5 players at the beginning of each round, to reduce reputational and learning effects as the experiment progressed. In the first stage of each round, subjects first engaged in a "slider task" before seeing their results, group rank, and payoff. The slider task was chosen as a real-effort task as it is designed to be an effortful task that varies between highly and poorly motivated individuals (Gill and Prowse, 2012). This ensured that subjects' payoffs seem as earned and deserving as possible, thus minimizing fairness concerns and making this a "hard test" for any other-regarding preferences, given the incentivized nature of the game. In the task, subjects saw 48 "slider bars" on the screen, and had 30 seconds to drag as many bars as possible to the middle of each slider, earning a point (in the default) every time the number to the right of the slider read "50". Subjects were ranked, within each group, after each task, based on their points earned, with payoffs assigned based on rank.

[12]See the appendix for a detailed description of the experimental manipulations, survey questions, the handout presented and read to subjects, manipulation checks, and screenshots of the program.

For subjects in the *Static* condition, a tax vote was completed after each real-effort task. Thus, subjects played the game without uncertainty, voting on a tax already knowing their rank, with no future task. There are two critical benefits to measuring such a baseline condition. First, we are able to replicate the findings in the literature regarding the existence of both self-interest and inequality aversion in static redistribution experiments (Klor and Shayo, 2010). Secondly, it allows us to determine, within the same design and sampling frame, the effect of introducing uncertainty and expectations of future income in redistribution preferences, as measured through the difference in subjects' selected tax in the *Control* condition (2 periods with no income shift) and the *Static* condition (1 period only). As such, we are able to replicate the findings in the literature regarding the positive association between uncertainty and risk with redistribution preferences (Barber, Beramendi, and Wibbels, 2013; Esarey, Salmon, and Barrilleaux, 2012). The results for the *Static* condition, and its comparison with the *Control* condition, thus, give us greater confidence in the validity of our design and sample.

Whereas the *Static* condition only involved a single stage, the other treatment conditions consisted of two stages in each round, reflecting their dynamic nature. In the second stage of each round, subjects in the other treatment conditions first received an information prompt (our between-subject treatment manipulation), before voting on a lump sum, flat tax, and engaging in the task a second time, with the selected tax applied to the payoffs of both tasks. Such a design captures the relevant expected mobility models, where actors vote on a tax that applies to both their current income, as well as their income in a subsequent time period (since the policy is "sticky"). This feature of the design is meant to capture the dynamic in the real-world, where people weight their current income with their expected future income when considering the costs and benefits of social policies

and taxation. This same task → information → vote → task sequence was carried out over 10 rounds, with new groups randomly drawn for each round.

In order to ensure complete information on the voting itself, subjects selected the tax rate (our measure of revealed redistributive preferences) observing the potential earnings of each rank if their suggested tax rate was implemented. Importantly, after the second task was completed, subjects were not told their relative rank nor the chosen tax rate, in order to minimize reputational and learning effects (such as social desirability or norms of reciprocity). At the end of the experiment, the median tax rate selected in each group was applied to the earnings of each subject for that round, and subjects were paid their post-tax earnings from one randomly selected round and task. As such, this is a fully incentivized tax vote, meaning that our measure of redistributive preferences has real consequences for subjects' ultimate monetary payoffs from their participation. Furthermore, pre-tax earnings were fixed, such that subjects actually competed for a higher rank. This was done to ensure that the level of pre-transfer inequality was constant across all conditions, in order to control for any confounding effect from changes to inequality. The pre-tax earnings for the first task were the same across all treatments: \$10, \$6, \$4, \$3, \$2, which is a slightly right-skewed distribution (Gini coefficient of 30.4), thus resembling real-world income distributions.[13]

Subjects received 1 out of 6 possible between-subject experimental information manipulations, before voting on a tax in each round.[14] In the *Static* condition, subjects voted on

[13] A quick glance at OECD data shows that such a level of inequality is approximately equivalent to that in Poland or France in 2010, and, in fact, is very close to the OECD average that same year (OECD, 2014b).

[14] The treatment is the same across rounds within each experimental session, as these are between-subject manipulations. All subjects within each session receive the same information in each round, except in the *POUM* and *PODM* conditions.

a tax rate immediately after engaging in the first task, with complete information about their place in the income distribution. In the *Control* condition, subjects were prompted that for the 2nd task of that round, they would continue to earn 1 point for each correct slider movement (the default from the 1st task) and the possible payoffs would remain the same. Basically, they were told them that the second stage would be identical to the first stage. In the *Upturn* condition, subjects were told that the points earned would remain the same, though they were now informed of a 70 percent probability that the possible payoffs for the 2nd task would increase to \$15, \$8, \$6, \$4.5, and \$3. In the *Downturn* condition, conversely, they were provided the same information, except that the possible payoffs in the 2nd task were now \$5, \$3, \$2, \$1.5, and \$1. These are inequality-preserving payoff shifts, which should only affect the expectations of future absolute income, not future relative income.

In the *POUM* and *PODM* conditions, half of the subjects were informed that the possible payoffs for the 2nd task would remain the same for the other half of the subjects, while they would earn 2 points and 0.5 points, respectively, for each correct slider movement in the subsequent task. Thus, they were informed that they would experience POUM or PODM, respectively, in each round. The other half of the subjects (those not experiencing POUM or PODM) received the same prompt as the subjects in the *Control* condition for the entire game. This manipulation is analogous to the conceptualization of economic mobility in the literature as one of a shift in individual's productivity levels.

In sum, the new redistribution game improves on previous designs in several ways. It avoids reputational and learning effects, controls for confounders such as reciprocity, social desirability, deservingness and fairness norms, while providing a more realistic and

theoretically faithful operationalization of the dynamic aspect of redistribution. It also provides a fully informed tax vote and controls for levels of pre-transfer inequality, thus, ultimately, isolating the effects of shifts in absolute and relative income. This new design, we argue, provides a useful framework for studies on the effects of income shifts on redistribution, and could serve as the basis for future extensions in this literature.

The full experiment was conducted during the months of November, 2013, February, 2014, and April, 2014, in an experimental lab at a The Ohio State University. Subjects were recruited from a department-wide voluntary subjects pool. While we recruited only college students for the experiment, we lack any theoretical basis to suspect treatment effects to only occur among younger or more educated populations. They played the game and answered a short survey, lasting about 20-25 minutes, for extra-credit, a $5 show-up fee, and their earnings from the game. Each session was conducted with 10 subjects, with a total of 20 sessions held, for a combined total of 200 subjects. During each session, subjects played 10 rounds in 1 of 6 treatment conditions. This resulted in 10 observations per subject, for a total of 2,000 observations.

3.5 Results

We analyze the results of the experiment using nonparametric and multilevel regression methods (Keele, McConnaughy, and White, 2012; Gelman and Hill, 2007). When using nonparametric methods, we aggregate the data up to the subject-level, such that the ten observations available for each subject are aggregated into one average score per subject.[15] However, in the second part of the analysis we analyze the data at the observation level, such that each subject contributes ten observations (one for each round) using mixed

[15]This ensures that observations are statistically independent of each other, since repeated observations from any single subject might be correlated.

effects models to account for possible within subject correlations. The online appendix provides descriptive statistics for tax rate choices in each of the treatment conditions, as well as manipulation checks, confirming the validity of our treatments.

Table 3.2 – Descriptive Statistics

Treatment	Subjects	Rounds	Observations	Median Tax (%)
Control	70	10	700	35
Static	30	10	300	33
Upturn	30	10	300	50
POUM	20	10	200	25
Downturn	30	10	300	30
PODM	20	10	200	50

3.5.1 Nonparametric Analysis

Are differences across treatments statistically significant? A Kruskal-Wallis test suggests that the distribution of tax rates differs across the six conditions (χ^2=32, $p << 0.01$). Pairwise comparisons between each treatment with the *Control* condition are shown in table 3.3. The table shows the Hodges-Lehmann estimate for the difference between medians, the p-values from a one-sided Mann-Whitney U test, and a one-sided Fischer-Pitman randomization test.

Conclusion 1: An expected increase (decrease) in relative income decreases (increases) redistributive preferences, on average. As predicted by hypotheses 4 and 5, changes in relative income have an opposite effect on redistributive preferences. When subjects experience the "prospect of upward mobility", where a subset of players (themselves included) receive a boost to their expected monetary earnings, they become less willing to

Treatment	N	Hodges-Lehmann Estimate	Mann-Whitney U P-value	Fischer-Pitman P-value
Control	70			
Static	30	0.6	0.446	0.483
Upturn	30	9.2	0.017	0.007
POUM	20	-11.0	0.006	0.004
Downturn	30	-4.2	0.066	0.045
PODM	20	11.4	0.001	0.003

Note: P-values shown are based on one-sided alternative hypotheses. The Mann-Whitney U test is a rank-based nonparametric test, while the Fischer-Pitman test is a permutation-based test.

Table 3.3 – Non-Parametric Tests

vote for higher taxes, presumably so that they pay less in the next period if their rank increases. However, when subjects experience the "prospect of downward mobility", where a subset of players (themselves included) receive a blow to their expected monetary earnings, they become more willing to vote for higher taxes, presumably so that they gain more in the next period if their rank decreases. This is clear evidence for the self-interested effect of relative economic mobility on redistributive preferences. The results are approximately symmetrical across the *Control* condition, with the *POUM* (*PODM*) condition leading to a 11% point decrease (increase) in desired tax rates, on average. Clearly, our results provide strong evidence in favor of the POUM and PODM hypotheses following a relative income shift.

Conclusion 2: An expected increase (decrease) in absolute income increases (decreases) redistributive preferences, on average. As predicted by hypothesis 2 and 3, increases in absolute income also have an opposite effect from decreases in absolute income. When subjects experience the prospect of an upturn, where all players have a positive probability of receiving a boost to their expected monetary earnings, they become more willing

to vote for higher taxes, and reduce inequality even at a cost to themselves, consistent with the argument that inequality aversion is a luxury good. However, when subjects experience the prospect of a downturn, where all player have a positive probability of receiving a blow to their expected monetary earnings, they become less willing to vote for higher taxes, and, by implication, are less concerned with inequality. Importantly, while the results for the *Upturn* condition are similar in strength to the effects of the *POUM* and *PODM* conditions, leading to a 9% point increase in desired tax rates, the results for the *Downturn* condition are considerably weaker, and only statistically significant at the 10% level.

Conclusion 3: Relative and absolute income changes have an opposite effects on redistributive preferences. Taken together, conclusions 1 and 2 support the paradoxical juxtaposition of the underlying hypotheses. On the one hand, experiencing the "prospect of upward mobility" has an opposite effect to experiencing an upturn, while on the other hand, experiencing the "prospect of downward mobility" also has an opposite effect to experiencing a downturn. Existing theories of the effects of income changes on redistributive preferences cannot account for this pattern, with the results supportive of the theory advanced in this paper.

Before proceeding, it is important to also note that we do not find any difference, across the experimental conditions, in subjects' risk preferences (χ^2=1.7, $p = 0.89$), which undermines the argument that our results are driven by prospect theory (Kahneman and Tversky, 1979). Such an alternative explanation is also less probable given that the economic shifts in the game are expectations of future income, not actualized gains or losses. Such a situation should be less likely to induce feelings of domains of gains and losses,

as such domains are characterized by the significant cognitive influence of the reference point of potentially losing one's realized gains, or recovering one's realized loses.

3.5.2 Regression Analysis

There are three primary weaknesses with the previous analysis. First, since the data are aggregated up to the subject-level, a considerable amount of variation in the data is discarded. This may contribute to the statistically weak results found for the *Downturn* condition. Second, since the sample size of each treatment condition is somewhat limited, it might be the case that there are actual differences between the finite samples, unrelated to the treatment conditions. Third, the previous analysis does not allow for treatment effect heterogeneity, such as the possibility that the treatments might have differential effects across ranks, which needs to be analyzed in order to properly evaluate hypotheses 1 and 3, in particular. To strengthen the analysis, we report the results of linear mixed effects models, which include random effects at the subject-level. This allows us to analyze the data at the observation-level, instead of the subject-level, while also accounting for possible within subject correlations in tax rate choice.

Table 3.4 reports the main results of the mixed effects analysis. Model 1 only includes the treatment indicators and is primarily reported to confirm that the results of the mixed effects analysis line up with the nonparametric analysis. In model 2, indicator variables for the ranking from the first task have been added, with the third rank serving as the reference category. Finally, model 3 includes measures for gender, family income, party identification (7 point scale, higher values correspond with being more Republican), and

	Model 1	Model 2	Model 3
Constant	40.47 (1.89)*	44.39 (1.83)*	52.80 (3.80)*
Static	0.16 (3.45)	0.40 (2.90)	−1.29 (2.99)
Upturn	9.55 (3.45)*	9.79 (2.90)*	8.88 (3.01)*
POUM	−11.38 (4.01)*	−10.30 (3.37)*	−10.58 (3.37)*
Downturn	−5.99 (3.45)	−5.75 (2.90)*	−6.19 (2.93)*
PODM	11.55 (4.01)*	11.81 (3.37)*	10.10 (3.51)*
Rank 1		−27.76 (1.48)*	−26.79 (1.53)*
Rank 2		−20.39 (1.44)*	−19.67 (1.47)*
Rank 4		7.47 (1.44)*	7.85 (1.46)*
Rank 5		19.85 (1.52)*	20.15 (1.54)*
Female			−3.01 (1.95)
Family Income			−0.90 (0.43)*
Republican			−1.76 (1.02)
Conservatism			0.69 (1.12)
BIC	18808.89	17907.95	16921.44
Observations	2000	2000	1890
Subjects	200	200	189
Subject Variance	189.30	138.97	131.30
Residual Variance	607.62	378.76	375.42

*$p < 0.05$. Standard errors in parentheses. Kenward and Roger (1997) approximation for degrees of freedom used for hypothesis tests.

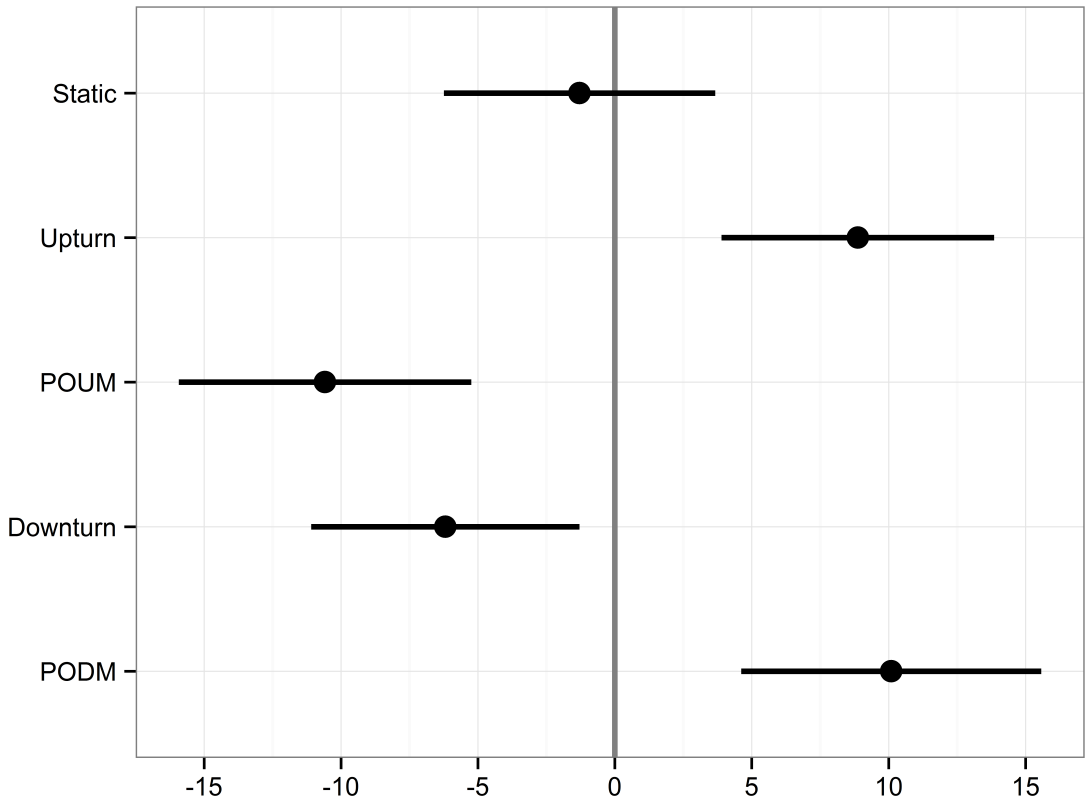
Table 3.4 – Mixed Effects Models

ideology (7 point scale, higher values correspond with being more conservative).[16] Figure 3.1 shows the results for the treatment effects from model 3.

In all three models, the size of the treatment effects remain substantively and statistically similar to the nonparametric analysis.[17] The additional variables added in model 3 provide some added explanatory power, although overall their effects are weak. A higher level

[16]Descriptive statistics for all variables can be found in the appendix.

[17]The *Control* condition is omitted from the model, meaning the coefficients for each treatment can be interpreted with respect to the *Control* condition.



Note: Point estimate and 95% confidence intervals shown.

Figure 3.1 – Treatment Effects on Tax Choice: Results from Model 3

of family income resulted in a lower tax vote, and being more Republican and/or Conservative was jointly associated with voting for less redistribution. Clearly, theses effects are weaker than most of the treatment effects, and pale in comparison to the laboratory income effects shown in models 2 and 3, with those ranked at the top voting, on average, for a 27% point lower tax, compared to the median rank, and those at the bottom voting, on average, for a 20% point higher tax. More importantly, the laboratory income and

treatment effects holds even after controlling for subjects' political ideology and family income.[18]

Do Treatment Effects Differ Across Ranks?

As expected, the tax rate chosen by subjects differs considerably across rankings from the first task. Thus, subjects in the top rank (Rank 1), voted for a 12.5% tax on average, while subjects in the bottom rank (Rank 5) voted for a 65% tax on average. The average tax rate preferred by the median rank (Rank 3) was 46%. Self-interest clearly mattered a great deal to subjects across conditions, consistent with the experimental findings above, and supportive of the "hard case" that this represents for other-regarding preferences, given the real-effort task and monetary payoffs.

Given the strength of the ranking effects and the fact that the hypotheses predict that treatment effects should vary between ranks, it is of interest to analyze the results separately for ranks. Table 4.2 shows the results for the rank-based analysis across conditions, with ranks 1 and 2 grouped together and 4 and 5 grouped together. Since the original sample consists of 2000 observations, each of the rank conditions consists of 400 observations (or 800 observations for two ranks together). However, as not all subjects experience all ranks (e.g. some subjects consistently ranked at the top across periods) the number of subjects in each of the models differs.

Conclusion 4: The effects of the *Static* treatment is highly conditional on the ranking from the effort task. Subjects that rank first or second in the *Static* condition, vote for a considerably lower tax than subjects with the same rank in the *Control* condition. The

[18]Including an egalitarianism scale, based on a factor analysis of 5 questions about egalitarian values, did not alter the results, while the scale remained insignificant.

	Rank 1 and 2	Rank 3	Rank 4 and 5
Constant	20.73 (1.73)*	44.71 (2.25)*	55.88 (2.34)*
Static	−10.21 (3.02)*	−6.64 (4.13)	15.66 (4.13)*
Upturn	16.30 (3.13)*	17.66 (4.15)*	1.79 (4.28)
POUM	−9.08 (3.73)*	−11.07 (4.89)*	−10.56 (5.05)*
Downturn	−4.96 (3.07)	−6.60 (4.16)	−1.74 (4.27)
PODM	18.92 (3.67)*	14.98 (4.72)*	4.76 (4.89)
BIC	6744.38	3622.56	7378.20
Observations	800	400	800
Subjects	181	172	179
Subject Variance	126.97	129.42	211.69
Residual Variance	195.29	378.58	455.87

*$p < 0.05$. Standard errors in parentheses. Kenward and Roger (1997) approximation for degrees of freedom used for hypothesis tests.

Table 3.5 – Mixed Effects Models, by Rank

reverse holds true for ranks four and five. This suggests that the added uncertainty of the second task effort introduced in the *Control* condition does indeed play a role, although it is masked when analyzing all ranks collectively. Put simply, the certainty of income rank in the *Static* condition leads to redistributive demands more directly based on current income, while the uncertainty of income rank in the *Control* condition reduces the effects of income on preferences. This replicates the positive association found in the literature between risk of income loss with redistributive preferences (Esarey, Salmon, and Barrilleaux, 2012). It also strongly supports hypothesis 1 and highlights the importance of properly accounting for the dynamic nature of redistributive preferences. Figure 3.2 further illustrates the conditional nature of the relationship, underlining the strength of the uncertainty effect.

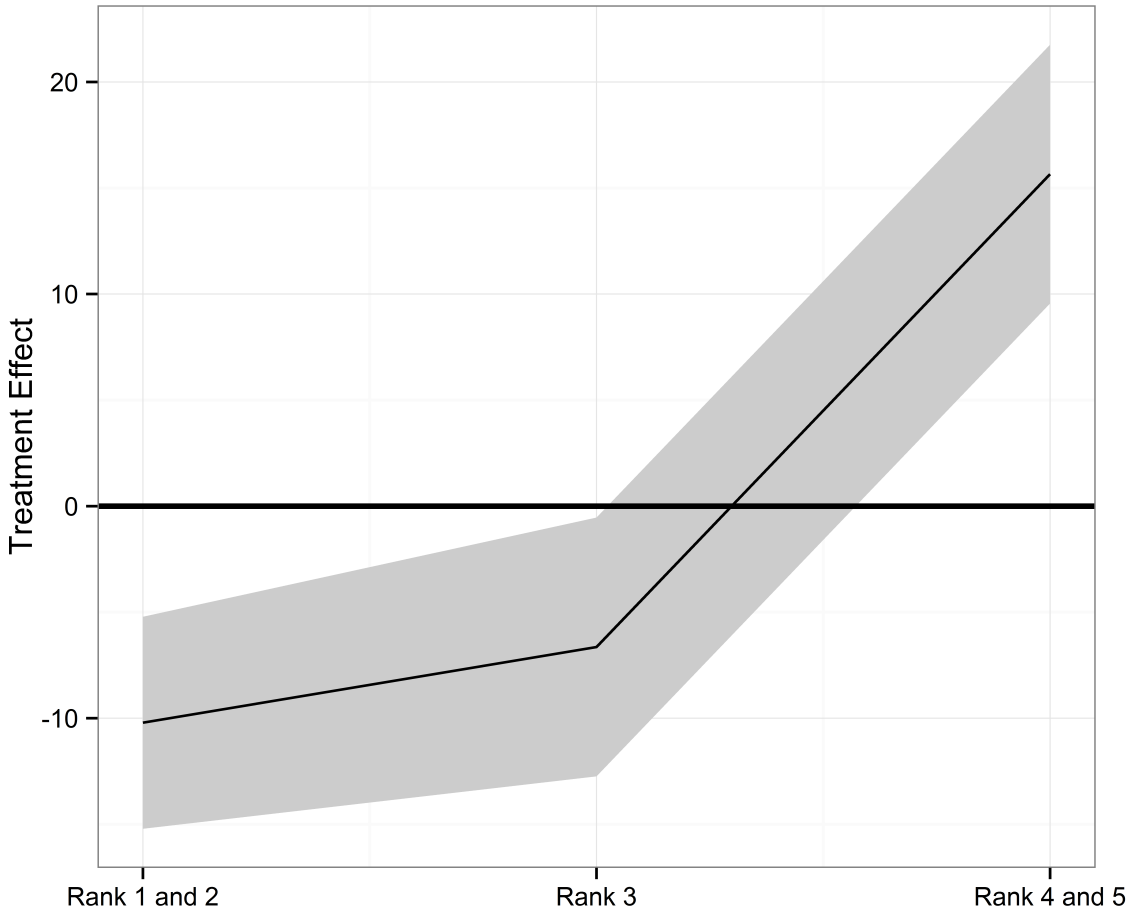
Conclusion 5: The results for the *Upturn* condition are entirely driven by the first three ranks. For ranks 1, 2, and 3, a shift from the *Control* to the *Upturn* condition leads,

on average, to a 17% point increase in tax vote, while there is no such effect for ranks four and five detected. For the "rich" in the game, an expected increase in absolute income seems to trigger a greater desire for equality, as subjects are more willing to sacrifice their own earnings for others. While the difference in treatment effects across ranks are consistent with our expectation, the null effects among the lowest two ranks are not. One possibility is that the expected absolute income gain was not large enough for the bottom ranks to trigger the sense of material satisfaction that would induce a greater concern for inequality. Another possibility is that these ranks experienced a ceiling effect, in that the bottom two ranks already supported such a high level of redistribution that growing inequality aversion was not able to significantly increase the tax vote.

Conclusion 6: The results for the *Downturn* condition are insignificant for all ranks. Given that we expected the effects of the *Downturn* condition to differ by income, these null results are surprising, especially for the lower ranks. It is possible that any feelings of similarity among the lower ranked groups were nullified by their perceived difference as "losers" of the game, or that the treatments were simply too weak for the lowest ranks. However, this still fails to explain why the top ranks no longer exhibited significant differences from the *Control*. Alternatively, perhaps our sample was not large enough to reliably detect significant differences.

As we are unable to convincingly explain these patterns with our data, future studies should replicate this finding while seeking to separate the different potential mechanisms at work. More importantly, given that effects are clearly more pronounced after positive absolute income shifts, which is when we expected inequality aversion to exert the greatest effect, our finding highlights the potential problem of focusing mostly on economic

recessions when studying the effects of macroeconomic cycles on preferences and behavior. Other-regarding preferences, here in the form of inequality aversion, are clearly more pronounced after absolute income growth, which, paradoxically, is when others are less in need.



Note: Point estimate and 95% confidence intervals from models in table 4.2 shown.

Figure 3.2 – Effects of *Static* Condition on Tax Choice by Rank

The results for the *POUM* and *PODM* conditions are consistent across all ranks, although they do not always reach statistical significance. They also exhibit the strongest treatment effects among ranks 1-3, highlighting the strength of self-interested, income-based effects on redistributive preferences. Expecting to be relatively better off in the future seems to make you less concerned with helping others, which is striking given the strong effect on inequality aversion of expecting to be absolutely better off. The same applies in the opposite scenario of expecting downward relative shifts. Clearly, relative income shifts trigger more self-interested motivation than absolute income shifts.

Overall, the results of the experiment are mostly in line with the theory proposed in this paper, with competing theoretical accounts failing to explain all results simultaneously. Thus, while the political economy literature can account for the relative income effects of the *POUM* and *PODM* conditions, it does not offer an explanation for the absolute income effects seen in the *Upturn* and *Downturn* conditions. Conversely, while the policy mood literature can account for the absolute income effects of the *Upturn* and, to a lesser extent, *Downturn* conditions, it fails to predict relative income effects on redistributive preferences. In short, none of the previously advanced theories can explain why income shifts have the opposite effects on individual preferences, depending on whether others also experience the income shift or not.

3.6 Conclusion

The broader literature on the effects of economic mobility and macroeconomic cycles on the support for income redistribution remains at odds. We believe that conflicting theoretical predictions and empirical evidence can potentially be explained through a common framework. People's current and expected relative income shapes their preference for

redistribution. However, people also have a tendency to prefer greater equality, in particular for members of their in-group and once their own absolute material needs have been satisfied. The story is not complete without considering both perspectives.

It is our argument that different types of economic cycles have the potential of triggering these different dynamics. By their very nature, income shifts that (objectively or subjectively) vary across individuals will highlight the social distance in society, while income shifts that (objectively or subjectively) are the same across individuals will increase the feeling of social affinity with other members of society. Such feelings of group belongingness will increase concerns with inequality, resulting in individuals supporting greater redistribution, even at a cost to themselves, conditional on their material needs being satisfied. Using an improved experimental design, we tested this argument and found results consistent with these predictions. More generally, the implication of our findings is that the effects of economic cycles on public opinion cannot be fully understood without considering how they affect social affinity to other members of society, and individual material satisfaction.

In combining the policy mood and political economy literatures, we emphasize a behavioral political economy perspective (e.g., DellaVigna, 2009; Minozzi, 2013). It is incumbent on us to no longer treat such behavior as "an unpredictable 'social noise' to be randomly sprinkled over individuals" (Alesina and Giuliano, 2011:94), but instead systematically incorporate it into our models of political behavior. In our model we focus on inequality aversion and in-group solidarity, which we believe are especially critical when deciding on how to redistribute income in society. At the same time, we do not disregard the central role played by self-interest in shaping redistributive preferences. Concern for

fellow group members and individual self-interest are both inherent and fundamental human motivations. However, as demonstrated above, each underlying motivation becomes more pronounced under different contextual triggers, and depending on to whom people compare their economic gains and losses, such as after relative or absolute income shifts. Such a conditional understanding is necessary in explaining cross-national and temporal variation in other-regarding preferences (e.g., Fehr and Gintis, 2007).

The experimental results presented in this paper should be consider a first step – a proof of concept – in separating the effects of absolute and relative income shifts on redistributive preferences. While the results have high internal validity, their applicability outside the laboratory is uncertain. Clearly, an important next step is to establish whether the same dynamics are at play using observational data. Such an endeavor might require using proxies, or developing new measures, for perceptions of absolute and relative income shifts, attempting to objectively estimate how widely shared economic gains or losses are across the public during macroeconomic cycles, and demonstrating the effects on social affinity and group belongingness.

It is incumbent upon researchers to heed this plea to take seriously the crucial difference between absolute and relative income shifts, both theoretically and empirically. At the very least, greater attention should be given to the specific reference points that people use when forming opinions and perceptions on economic change; something we have shown should matter a great deal for policy preferences. Therefore, next steps could focus on the variation in perceptions of economic change (e.g., Duch, Palmer, and Anderson, 2000; Evans and Andersen, 2006) across contexts, in particular as it relates to absolute or different relative reference points (e.g., Ansolabehere, Meredith, and Snowberg, 2014),

and how sociotropic and egocentric perceptions (Anderson, 2007; Conover, Feldman, and Knight, 1986) relate to the distinct expectations of absolute and relative income shifts. Ultimately, greater attention should also be given to how such perceptions might be affected by other concerns (such as political ideology or group identification) and characteristics of the information environment (e.g., local conditions, elite discourse and media framing).

In sum, more work needs to be done to connect these experimental results with observational data, as well as to more firmly establish the mechanisms at work. Yet, our preliminary data are quite robust and not properly explained by the main theoretical frameworks in political science. It is our hope that we have provided a compelling and testable theoretical model, along with conceptual clarity on the effects of economic mobility, which can advance our knowledge of the relationship between economic cycles and redistributive policy preferences, as well as provided a useful experimental design by which to engage in controlled tests of these and related issues regarding taxation and government spending.

Chapter 4

Economic Insecurity, Incumbent Partisanship, and Voting Behavior in Comparative Perspective

The Great Recession ended in June 2009, at least on average.[1] While the recovery has been rapid and robust at the top of the income distribution, the middle and lower classes continue to struggle and fall further behind (Saez, 2013). These developments are not without consequences: Individuals in middle and low income households are more worried about their future financial situation, are less likely to be receiving good economic news, and are more likely to support welfare policies that buffer the risk of economic shocks (Hacker, Rehm, and Schlesinger, 2013; Pew Research Center, 2014). Even though the Great Recession may have formally ended, many individuals, thus, continue to feel its effects.

Mainstream approaches to economic voting would have it that these differences in economic experiences are inconsequential for how voters evaluate the performance of the

[1]Co-authored with Vittorio Merola. We thank Sarah Brooks, Paul DeBell, Philipp Rehm, and Sara Watson, as well as participants at the Comparative Politics Dissertation Workshop (CPRW) at The Ohio State University, for their comments and suggestions.

incumbent government. Voters are assumed to be informed by sociotropic, rather than egocentric, evaluations of the economy, suggesting that the state of the aggregate economy matters more for the voting calculus of individuals, than the state of their household finances (Kiewiet, 1983). While perceptions of the economy may differ systematically across individuals, based on factors such as partisanship and political sophistication (Stevenson and Duch, 2013), the divergent economic experiences of individuals at the top and bottom of the income distribution should not affect evaluations of the incumbent government. Moreover, any effect that such differences might have on support for welfare policies that buffer the risk of economic shocks are assumed to be immaterial to the evaluation of the incumbent.

In this paper, we argue that such broad differences in economic experiences do matter a great deal for how individuals evaluate the performance of the incumbent government. While we are not the first to make this argument (see, in particular, Lewis-Beck and Nadeau, 2011; Palmer and Whitten, 2011; and Singer 2013), we go beyond previous work in important aspects. Echoing Hacker, Rehm, and Schlesinger's (2013) sentiment, we argue that mainstream approaches have relied on poorly conceptualized measures of people's economic experiences, in particular, overlooking the role of group-based heuristics in shaping economic evaluations, and, as such, have not provided an adequate test of their effects on voting behavior. We employ such a measure, building on recent work on the effects of economic insecurity on policy preferences (Rehm, 2009), as well as recent work on the effects of "local", or group-based, information environments on economic voting, which highlight the importance of heuristics, based on "similar others", on perceptions of economic insecurity and the aggregate economy (Ansolabehere, Meredith, and Snowberg,

2014). In the simplest sense, we maintain that people experience different economies, and that such experiences matter for their voting behavior.

More specifically, we argue that an individual's occupation forms such a group-based information environment and that occupational unemployment rates, as measures of employment insecurity, thus, inform perceptions of the state of the aggregate economy, as well as the potential for personal unemployment. Consequently, high and rising occupational unemployment, a salient economic heuristic, leads to negative evaluations of economic performance and reduces the probability of supporting the incumbent government, regardless of the actual state of the overall economy. Simultaneously, however, high and rising occupational unemployment shifts support toward left-wing parties, the traditional champions of welfare policies that buffer the risk of economic shocks. Thus, employment insecurity, like many other economic issues, serves as a valence issue, with voters preferring parties that are most competent to address the issue of unemployment, but it is also inherently a partisan issue, due to the distributional consequences of welfare policies (Lewis-Beck and Nadeau, 2011; Wright, 2012). As such, we argue that employment insecurity is an issue that combines aspects of the performance-based, government accountability logic of economic voting (e.g., Duch and Stevenson, 2008), with the issue-based, positional logic of voting for a party based on policy preferences (e.g., Ansolabehere, Rodden, and Snyder, 2008).

This dual electoral nature of employment insecurity brings about a potential conflict. Namely, under a left-wing incumbent government, economically insecure individuals are cross–pressured: Higher employment insecurity pushes them to punish the incumbent, while at the same time, it pushes them to support left-wing parties, which are one and

the same in this scenario. Accordingly, we argue that high and rising occupational unemployment reduces the probability of electorally supporting the incumbent and that this effect will be largest for right-wing incumbents, since insecure individuals will, in a sense, have both a valence and a positional reason to vote against right-wing incumbents. At the same time, we argue that high and rising occupational unemployment also affects the more fundamental decision of turning out to vote, especially under left-wing incumbents, as a poorly performing left-wing government will leave insecure individuals ambivalent and alienated from the political process. Consequently, occupationally insecure individuals will be less likely to vote, and if they vote, less likely to vote for the incumbent, with the effects being magnified by a left-wing incumbent government and attenuated by a right-wing incumbent government, respectively.

We test our hypotheses using a Bayesian hierarchical multinomial modeling approach, with individual level data from 43 elections in 21 European democracies from 1996 to 2013. We capture employment insecurity with the group-based heuristic of occupational unemployment rates and find robust support for our theory, with high and rising occupational unemployment consistently reducing the probability of voting for the incumbent, as well as increasing the probability of abstaining. The former effect is stronger (more negative) for right-wing incumbents, while the latter is stronger for left-wing incumbents, as we hypothesize. In a follow up to the main analysis, we explore more fully the mechanism connecting occupational unemployment rates with concerns about employment insecurity and judgments about national unemployment rates. We find that high occupational unemployment rates are associated with more subjective employment insecurity and a systematic overestimation of aggregate unemployment, as predicted by our theory.

Our findings demonstrate the importance of using better conceptualized measures of salient economic experiences, such as group-based heuristics, in order to properly evaluate the effects of the economy on voting behavior. We argue that an individual's occupation forms such a group-based information environment and show that occupational unemployment rates affect perceptions of both employment insecurity and the aggregate economy. We also emphasize the need to move beyond the traditional assumptions of previous work on economic voting. This vibrant and rich literature has mostly neglected the essential insight that economic issues are often inherently partisan, which can have important implications for the effects of the economy on voting behavior. This dual nature of much economic change, whereby a signal about the economic competence of the incumbent government is produced at the same time that voters shift their issue position in reaction to new economic circumstances, is exemplified by the fundamental issue of employment insecurity.

4.1 Literature Review

Our argument integrates and extends the literatures on economic voting and the effects of economic insecurity on policy and partisan preferences, which all too often fail to speak to each other, despite their obvious connection.[2] We build on three important insights from these literatures: That voters use group-based heuristics to form evaluations of the performance of the economy; that concerns about unemployment are an especially salient consideration in evaluations of the economy; and that employment insecurity not only affects economic evaluations, but also support for welfare policies that buffer the risk of

[2]See Anderson (2007) and Healy and Malhotra (2013) for recent reviews of the literature on economic voting, and Alesina and Giuliano (2011) for a recent review of the determinants of redistributive preferences.

economic shocks. Our primary theoretical contribution to the literature involves synthesizing the implication of these arguments, to better understand the effects of the economy on voting behavior. Below, we discuss how our argument relates to the extant literature.

4.1.1 The Basis of Economic Evaluations

In mainstream accounts of economic voting, individuals are assumed to be motivated by sociotropic, rather than egocentric ("pocketbook"), evaluations of economic performance. In other words, the state of the aggregate economy is assumed to matter more for the voting calculus of individuals, than the state of their household finances (Kinder and Kiewiet, 1979; Lewis-Beck, 1990; Evans and Andersen, 2006). The dominant theoretical justification for this assumption centers on the appropriateness of each as a basis for evaluating the performance of the incumbent: Voters are assumed to hold the government responsible for the state of the overall economy, but attribute responsibility for household finances to more localized factors, and as such, the latter should not inform their judgments of government performance (Kinder and Kiewiet, 1981). Importantly, while the basis of evaluation of economic performance may be sociotropic, it doesn't necessarily imply that voters are motivated by altruism. Thus, self-interested individuals may rely on sociotropic, rather than egocentric, evaluations of economic performance, believing that it serves as a better signal of how the incumbent government might affect their individual well-being in the future (Hibbs, 1993).

Collectively, individuals are fairly good at sensing the very broad objective state of the economy (Erikson, MacKuen, and Stimson, 2000; Sanders, 2000). Individually, however, perceptions of economic performance often differ radically, even though people are nominally exposed to the same aggregate economy (Duch, Palmer, and Anderson, 2000). This

is perhaps unsurprising: Individuals have limited incentives to acquire detailed information about the performance of political candidates (Downs, 1957), and as such, can be expected to act as "cognitive misers" (Fiske and Taylor, 2013), relying on heuristics when evaluating the performance of the incumbent government. Any systematic differences across individuals in the informational heuristics they use to make sense of the economy, will affect observed differences in economic perceptions, explaining how perceptions can vary within the same economy (Stevenson and Duch, 2013).

The sources of such heuristics are generally attributed to cognitive or motivational factors, rather than people's own economic environment.[3] An alternative perspective, however, emphasizes the role played by local information environments, which people interact with more directly than the aggregate economy. Books and Prysby (1999), for example, demonstrate how the fear of unemployment is shaped more by direct, personal experiences with unemployment, than by national or state unemployment levels. More generally, it has also been shown that the local economic context can have a direct impact on general beliefs about the economy (Newman, Johnston, and Lown, 2015), perceptions of the overall performance of the economy (Newman et al., 2015), as well as social policy preferences (Cutler, 2007).

Related to this work is the argument that group-based heuristics provide an important source of economic evaluations, distinct from both egocentric and sociotropic considerations (Brady and Sniderman, 1985; Conover, 1985), and can independently affect voting behavior (Mutz and Mondak, 1997). More recently, Ansolabehere, Meredith, and Snowberg

[3]Hetherington (1996), for example, highlights the role played by media exposure, and Holbrook and Garand (1996) demonstrate how differences in political and economic interests, as well as political sophistication, can alter perceptions of the economy. The most important "perceptual screen" is generally considered to be political partisanship, with partisans viewing the state of the economy more favorably when their party is in office (Campbell et al., 1960; Bartels, 2002; Gerber and Huber, 2010)

(2014) develop this account by emphasizing the importance of information gained from the "mecro-economy", a level somewhere between the aggregate and household economy, composed of "similar others", on dimensions such as location, race, education, and gender. Importantly, the authors show how negative mecro-economic conditions lead to negative evaluations of the aggregate economy, and, in turn, less support for the incumbent, regardless of the actual performance of the economy.

While the suggestion that individuals use group-based heuristics to make sense of political issues is well taken, we believe greater attention should be given to what specific groups form the basis for such heuristics. Conover (1985), providing a rare example, finds that, in general, a majority of individuals identify, first and foremost, with their economic group. Once we narrow the issue under consideration to economic performance, it seems even more pertinent to consider groups related to an individual's economic position, rather than demographic characteristics, such as age and gender, as the basis for group-based heuristics. As other studies suggest, occupation may form such a group-based information environment (Rehm, 2011a).

4.1.2 The Political Relevance of Employment Insecurity and Unemployment

The state of the economy can be evaluated based on a number of dimensions. In poor economic conditions, stagnant growth, high inflation, and high unemployment each play a prominent role in the media, but the detrimental effects of high unemployment are the factor with which people most easily relate to on a personal level, not least because it is often directly experienced by friends, family members, and neighbors (Conover, Feldman, and Knight, 1986). These factors contribute to making unemployment an especially salient

consideration in evaluations of the economy and, by extension, the performance of the incumbent government (Kiewiet and Udell, 1998).

The role of employment insecurity and concerns about unemployment can, however, differ across individuals. Weatherford (1978) suggests that middle and working class voters are more responsive to economic fluctuations than upper class voters, while Hibbs (1982) and Palmer and Whitten (2011) show that low income voters tend to be more concerned about national unemployment rates, while rich voters are more concerned about inflation (see also Fraile and Pardos-Prado, 2014). Similarly, Singer (2013) and Fossati (2014) argue that employment insecurity increases sensitivity to economic fluctuations and, as such, makes economically vulnerable individuals more likely to vote based on economic performance. Mughan and Lacy (2002), furthermore, show how voters' employment insecurity can directly affect their voting behavior.

Not only have concerns over unemployment been theorized to affect support for the incumbent, but also the more fundamental decision of whether to turn out to vote in the first place. Unlike traditional theories of economic voting, however, theories about the effects of the economy on turnout make contradictory predictions about the direction of the relationship. While one school of thought considers economic hardship a force for mobilization, another views it as an impetus for political withdrawal. The former argument is based on the idea that economic difficulties push people to participate in the political realm in order to voice their grievances (Arceneaux, 2003; Burden and Wichowsky, 2014; Schlozman and Verba, 1979). The latter argument, meanwhile, is based on the notion that economic hardship fosters a preoccupation with personal problems that reduce the time

and attention paid to the secondary concern of politics (Lacy and Burden, 1999; Radcliff, 1992; Tillman, 2007; Brooks, 2014).

In each of these theories, unemployment is generally conceptualized as a valence issue. That is to say, voters are assumed to have a common (albeit differing in intensity) preference for low unemployment, and a desire to elect the political actor most likely to reduce unemployment, with changes in unemployment under the incumbent serving as a signal of his or her competence. Importantly, this conceptualization disregards the distributional conflict inherent to most policy issues and, in particular, the prospect of unemployment. Thus, concerns about unemployment are not assumed to have implications for policy or partisan preferences, above and beyond their effects on incumbent support or the decision to turn out to vote.

4.1.3 Positional and Partisan Aspects of Employment Insecurity and Unemployment

Just as unemployment can be framed in terms of a valence issue, it can also be framed as a positional issue, where voters have distinct preferences based on their ideology or economic interests (Lewis-Beck and Nadeau, 2011). Indeed, a growing literature on the determinants of redistributive preferences starkly demonstrates the importance of employment insecurity on policy preferences. Hacker, Rehm, and Schlesinger (2013), for example, show how insecure individuals are more concerned about their future financial position and more supportive of welfare policies that buffer the risk of economic shocks. Their work supports previous research, which suggests various factors that lead individuals to become more economically insecure and, in turn, more supportive of the welfare state (Iversen and Soskice, 2001; Rehm, 2009, 2011b).

Whether one considers their historical roots (Lipset and Rokkan, 1967) or track record while in office (Hibbs, 1977), left-wing parties are generally considered the champions of the welfare state and should, thus, be particularly appealing to voters concerned about unemployment. Rehm (2011a) suggests as much, when he argues that the greater polarization of economic risk in the United States has contributed to the polarization of the electorate, with insecure voters more likely to support the left-wing Democratic Party. Employing an alternative approach, Wright (2012) claims that in the context of US politics, the Democratic party "owns" the issue of unemployment, such that voters will turn to the Democratic party when unemployment becomes a problem, regardless of whether they are in office or not. Finally, Powell and Whitten (1993), building on Hibbs (1982), find that left-wing incumbents that preside over rising unemployment fare worse in elections than right-wing incumbents that see the same rise in unemployment, since voters hold left-wing governments to a higher standard on employment performance.

In the accounts of both Rehm (2011a) and Wright (2012), concerns about unemployment are a purely positional issue or partisan issue, respectively. As such, support for left-wing parties should rise with increasing concerns about unemployment, regardless of whether a left-wing incumbent government is in office or not. Conversely, Powell and Whitten's (1993) findings suggest that unemployment may be a partisan valence issue, whereby only left-wing incumbents are punished for high unemployment. Yet, their account disregards the positional effects of unemployment, as voters in their perspective do not become more supportive of left-wing parties in the context of high unemployment. As we discuss next, we believe both positional and valence perspectives should to be combined in the same framework. Thus, growing concerns about unemployment should increase support for left-wing parties and decrease support for the incumbent, implying that neither left-wing

parties nor opposition parties will unequivocally benefit electorally from rising employment insecurity.

4.2 The Effects of Employment Insecurity on Voting Behavior

We base our theory on the preceding discussion. We assume that economic performance figures prominently in the electoral calculus of individuals and that employment insecurity forms an especially salient consideration in evaluations of the economy. Given the cognitive constraints faced by individuals, we assume they use informational shortcuts when forming opinions about the economy and that their occupation forms a relevant basis for such heuristics. Thus, occupational unemployment rates will inform perceptions of the state of the aggregate economy, as well as the potential for personal unemployment.

Assuming that individuals attribute, at least partly, responsibility for their economic situation to the government, we argue that it stands to reason that variation in employment insecurity will have implications for the voting calculus of individuals. Thus, an increase in employment insecurity attributed to the action or inaction of the government, will reduce the probability of electorally supporting the incumbent. Likewise, high employment insecurity by itself should also prompt a withdrawal of incumbent support, as it is equivalent to a persistent negative economic outcome, and hence is interpreted by voters as a sign of the negative economic performance of the incumbent, on top of any changes in insecurity. The literature on employment insecurity often conceptualizes the effect in such absolute terms (e.g., Mughan and Lacy, 2002; Rehm, 2009). This leads to the following hypothesis:

> **Hypothesis 1**. High (and rising) employment insecurity reduces the probability of voting for the incumbent.

While exposure to unemployment risk will push some individuals to prefer the opposition to the incumbent, it also has the potential to affect the more fundamental calculus of whether to vote at all for other individuals. In the present case, we argue that employment insecurity propels voters not only to choose "voice" (vote for opposition) over "loyalty" (vote for incumbent), but also to "exit" (abstain) the electoral arena altogether (Hirschman, 1970). Employment insecurity should, thus, be a particularly strong demobilization force. This leads to the following hypothesis:

> **Hypothesis 2**. High (and rising) employment insecurity increases the probability of abstention.

Thus, all else equal, we argue that employment insecurity increases the probability of voting for the opposition, as well increasing the probability of abstaining. However, these effects are not unconditional. As mentioned above, employment insecurity and the prospect of unemployment have a distinct positional effect on preference, in addition to the valence effects outlined above. Insecure individuals are more supportive of government redistribution, and as such, one can expect them to be more supportive of parties that advocate for welfare policies that buffer the risk of economic shocks, regardless of the partisanship of the incumbent.

This brings about a potential conflict. Namely, under a left-wing incumbent government, individuals insecure about their employment are cross-pressured: Higher employment insecurity pushes them to punish the incumbent, while at the same time, it pushes them to support left-wing parties, who are one and the same in this scenario. In this case, these cross-pressures would attenuate the effects of insecurity on support for the incumbent.

Conversely, the clearest case of reinforcing pressures is for individuals with high employment insecurity under right-wing governments. In a sense, they have both a valence and a positional reason to vote against the incumbent - thus, the effects of employment insecurity should be larger under right-wing governments. This leads to the following contextual hypothesis:

> **Hypothesis 3**. The effects of employment insecurity on incumbent support will be larger (more negative) under right-wing incumbent governments.

What then of the occurrence of high employment insecurity under a left-wing government? We argue that an "incompetent left" has a different effect on the behavior of insecure individuals than an "incompetent right". In the former case, due to factors such as alienation and indifference, the incumbent is more likely to push voters to prefer abstention to voting for any of the suboptimal candidates. As individuals insecure about their employment are both unhappy with the incumbent and in need for a left-wing policy alternative to alleviate their insecurity, they become either alienated with the entire political system which has failed to provide for them, or simply believe that all candidates are too similar and not worth their time. This leads to the following contextual hypothesis:

> **Hypothesis 4**. The effects of employment insecurity on abstention will be larger (more negative) under left-wing incumbent governments.

In sum, we expect high (and rising) employment insecurity to reduce the probability of voting for the incumbent regardless of the partisanship of the incumbent government, but that the effect will be stronger (more negative) under right-wing incumbents. Furthermore, we expect high (and rising) employment insecurity to reduce the probability of casting a vote regardless of the partisanship of the incumbent government, but that the

effect will be stronger (more negative) under left-wing incumbents. Figure 4.1 illustrates the four hypotheses.

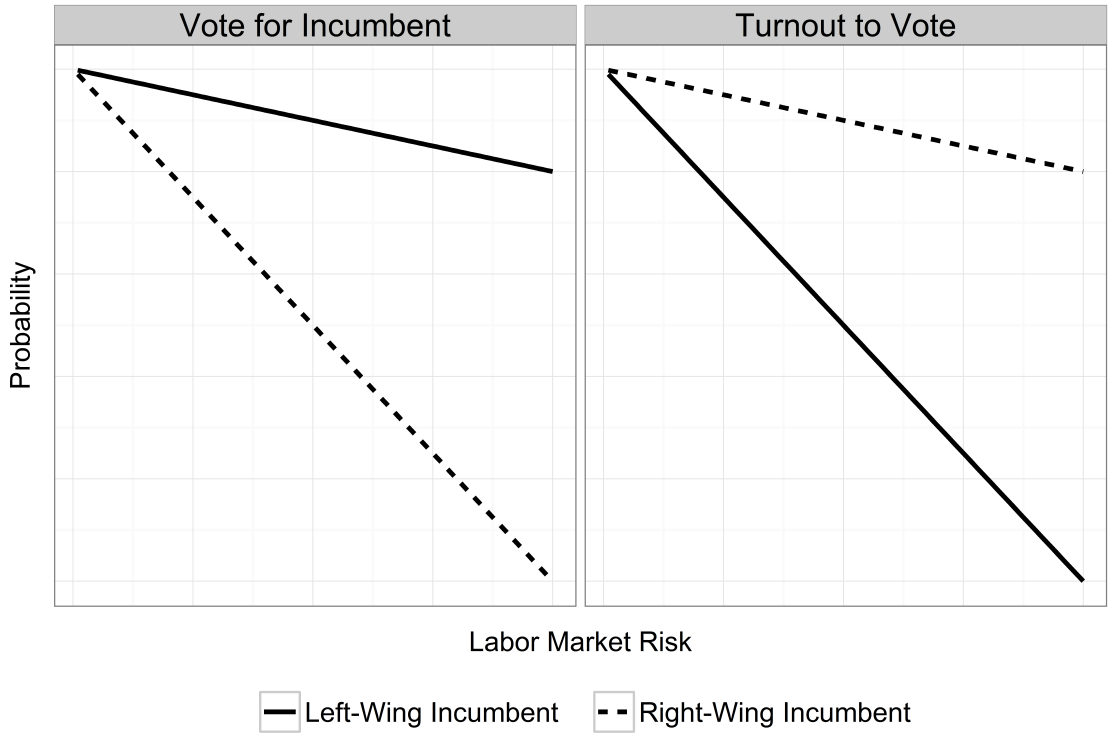


Figure 4.1 – Graphical Summary of Hypotheses

4.3 Data and Methods

To test these hypotheses, we require individual-level data on voting behavior and employment insecurity under different political settings. In particular, we require a large number of national-level elections under both left-wing and right-wing governments. Since elections and government turnover in any single country occur infrequently, this requires

us to employ a cross-national approach, with multiple elections in multiple countries allowing us to properly test the hypotheses. The Comparative Study of Electoral Systems (CSES) provides the basis for such a dataset, with surveys of nationally representative samples of eligible voters after general elections in a large number of countries, as well as measures of a number of relevant individual and contextual factors. In our main analysis, we end up with a sample of 28,299 individuals across 43 elections in 21 countries.[4]

The dependent variable used in the analysis is the respondent's self-reported voting behavior in the preceding lower house election. The CSES dataset records the party voted for by respondents who turned out to vote. Separately, the dataset also provides information on which party (or parties) formed the incumbent government at the time of election. Combining the information from these two measures, we code responses into the three categories of "Abstained", "Voted for an incumbent government party", and "Voted for an opposition party".[5]

[4]We included election studies from modules 1, 2, 3, and 4 of the CSES in our analysis (2003; 2007; 2013; 2014). We limit the sample to individuals in the labor market, as well as countries that are widely accepted as modern democracies. Due to the limited coverage of our measure of economic insecurity, we only include European countries. The countries (and the year of the election) are the following: Austria (2008, 2013), Belgium (1999), Croatia (2007), Czech Republic (2002, 2006, 2010), Denmark (2007), Finland (2003, 2007), Germany (1998, 2002, 2005, 2009), Great Britain (1997, 2005), Greece (2009), Hungary (1998, 2002), Ireland (2002, 2007), Iceland (2003, 2007, 2009), Netherlands (2010), Norway (1997, 2001, 2009), Poland (2001, 2005, 2007), Portugal (2002, 2005, 2009), Romania (2004), Slovenia (2004), Spain (1996, 2000, 2004), Sweden (2006), and Switzerland (1999, 2003, 2007). Surveys differ in their specific sampling and interview methodologies (see documentation for further details).

[5]In cases where no information was provided on the composition of the incumbent government, we consulted the Database of Political Institutions (Keefer, 2012) and the European Election Database (2015). Note that no distinction is made between voting for the party of the chief executive or other coalition parties in multiparty governments. Although this has the potential to be consequential for analyses of incumbent voting, Duch and Stevenson (2008:58) find that it rarely is in practice.

To capture our main independent variable, employment insecurity, we use the unemployment rate in respondent's main occupation, reflecting the heuristical basis of its perceptions.[6] We use a measure compiled by Philipp Rehm (2009; 2011b), which captures occupational unemployment rates (OURs) by gender, and are calculated as the share of the unemployed workforce in each of the nine ISCO-88 professional categories, separately for each gender, using yearly data from the International Labour Office's (ILO) Database on Labour Statistics and the European Union's Labour Force Survey.[7] In order to capture the effects of both high and rising employment insecurity, two measures are used: a measure of the level of occupational unemployment in the year of an election, and a measure of the change in occupational unemployment in the year prior to an election. Thus, our approach in many ways mirrors previous work using aggregate unemployment as a measure of economic performance, which is operationalized using both unemployment levels and changes in unemployment prior to an election (e.g., Powell and Whitten, 1993; Wright, 2012).

The measure of incumbent government partisanship was derived from an indicator included in the CSES dataset on the "ideological family" of each political party, as determined by country experts. Based on the indicator, parties were classified as right-wing (0), left-wing (1), or other (0.5).[8] The partisanship of the incumbent government was then calculated as the weighted (by cabinet seats) average of the score for each party holding a

[6]Unemployed individuals are categorized by their last occupation before becoming unemployed.

[7]We are grateful to Philipp Rehm for making his occupational unemployment data available to us.

[8]This follows the approach taken by Keefer (2012). Parties classified as communist, socialist, social democratic, or other left-wing family were coded as left-wing. Parties classified as conservative, Christian democratic, or other right-wing family were coded as right-wing. All other parties were coded as other.

cabinet seat in government. The final scale ranges continuously from 0 (all cabinet seats held by right-wing parties) to 1 (all cabinet seats held by left-wing parties).

We included several other explanatory variables in the analysis. At the individual-level, we control for respondent's age, age-squared, gender, education, household income, political left-right ideology, as well as whether they are union members or unemployed. At the national-level, we control for several alternative hypotheses. To account for the potential importance of aggregate economic conditions, we include a measure of economic growth (Heston, Summers, and Aten, 2012) and aggregate unemployment rates (World Bank, 2012). The inclusion of the latter measure allows us to directly compare the relative importance of aggregate and individual level measures of unemployment and employment insecurity.

Finally, we also control for the vote share of the incumbent parties and voter turnout in the previous election. These last two measures control for a host of unobserved cross-national contextual difference, thus giving us more confidence in the results from our cross-national variables, while also ensuring our analysis controls for baseline levels of turnout and incumbent support.

4.3.1 Estimation

We model the data using Bayesian hierarchical multinomial regression (Gelman and Hill, 2007; Jackman, 2009; Kruschke, 2014).[9] Each respondent i in the data reported one of $J = 3$ vote choices: Voting for the incumbent, voting for the opposition, or abstaining.

[9]See Stegmueller (2013) for a large-scale Monte Carlo simulation study demonstrating the superiority of a Bayesian approach over maximum likelihood estimation, when few level-2 units are available, as is usually the case in the analysis of political behavior across country-elections.

Furthermore, each respondent *i* is nested within country-election *s*, and we expect that systematic country-election differences affect voting behavior.

We estimate variations on the following baseline two-level model:

$$\Pr(y_{is} = j) = \frac{\exp(\beta_{js0} + \beta_{js1}\mathrm{OUR}_{is} + \beta_{js2}\mathrm{OUR}_{is} \times W_s k + \beta_{jZ}X_{iZ})}{\sum_{r=0}^{J=3} \exp(\beta_{rs0} + \beta_{rs1}\mathrm{OUR}_{is} + \beta_{rs2}\mathrm{OUR}_{is} \times W_s k + \beta_{rZ}X_{iZ})}$$

$$\begin{bmatrix} \beta_{1s0} \\ \beta_{2s0} \end{bmatrix} \sim N\left(\begin{bmatrix} \gamma_{10} + \gamma_{1k}W_{sk} \\ \gamma_{20} + \gamma_{2k}W_{sk} \end{bmatrix}, \Omega \right)$$

$$\Omega = \begin{bmatrix} \omega_{11}^2 & \omega_{12} \\ \omega_{21} & \omega_{22}^2 \end{bmatrix}$$

Without some restrictions, the parameters of the model are unidentified. We follow standard procedure and set one of the three choices as a baseline category, by restricting all coefficients to be equal 0 for that choice. In our case, voting for the incumbent, *j=0*, serves as the baseline, with other coefficients being interpreted with respect to that baseline. Since our primary interest is in the within-country effects of occupational unemployment and how contextual country-election level factors, such as government partisanship, condition the effects of occupational unemployment, we group mean center all level 1 predictors at the country-election level (Enders and Tofighi, 2007).

The probability of respondent *i* in country-election *s* choosing *j* is a function of the following components.

1. The choice-specific random intercept β_{js0}, which represent the probability that an "average" respondent in country-election *s* chooses *j*. The two random intercepts —

one for the probability of voting for the opposition, β_{1s0}, and one for the probability of abstaining, β_{2s0}, — are, in turn, a function of country-election predictors $\gamma_{10} + \gamma_{1k}W_{sk}$ and $\gamma 20 + \gamma_{2k}W_{sk}$, respectively., with K indexing the number of country-election predictor variables. We allow the random intercepts, β_{1s0} and β_{2s0}, to be correlated.

2. The choice-specific effect of OUR_{is}, β_{js1}. In several specifications, we employ a cross-level interaction of OUR_{is} with a level 2 covariate to examine if the effects of employment insecurity are modified by contextual factors (e.g. government partisanship). β_{js2} represents the choice-specific effect of such interactions.

3. β_{jZ}, which represents a matrix of Z choice specific unmodeled coefficients multiplied by individual characteristics.

Following Jackman (2009), we specify independent normal priors for each of the β and γ parameters and an inverse Wishart prior for the covariance matrix, Ω. The priors are noninformative and do not affect the results of the analysis. We run three chains of a Gibbs sampler for 15,000 iterations, discarding the first 5,000 iterations of each chain. The resulting 30,000 iterations form the sample which we base our inferences on. Diagnostics based on Gelman and Rubin (1992) and Cowles and Carlin (1996) suggest that the chains mix well and show no sign of nonconvergence.

4.4 Empirical Analysis

The main results of the analysis are presented in table 4.1. The first column contains the results from a baseline specification using both the level and change of occupational unemployment (OUR) to capture employment insecurity, but without contextual variables

allowed to shape these effects. The second column, on the other hand, provides the results from a model where incumbent partisanship is interacted with both OUR level and OUR change, thus allowing it to condition the effects of both variables on voting behavior. These two models directly test the four hypotheses outlined earlier.

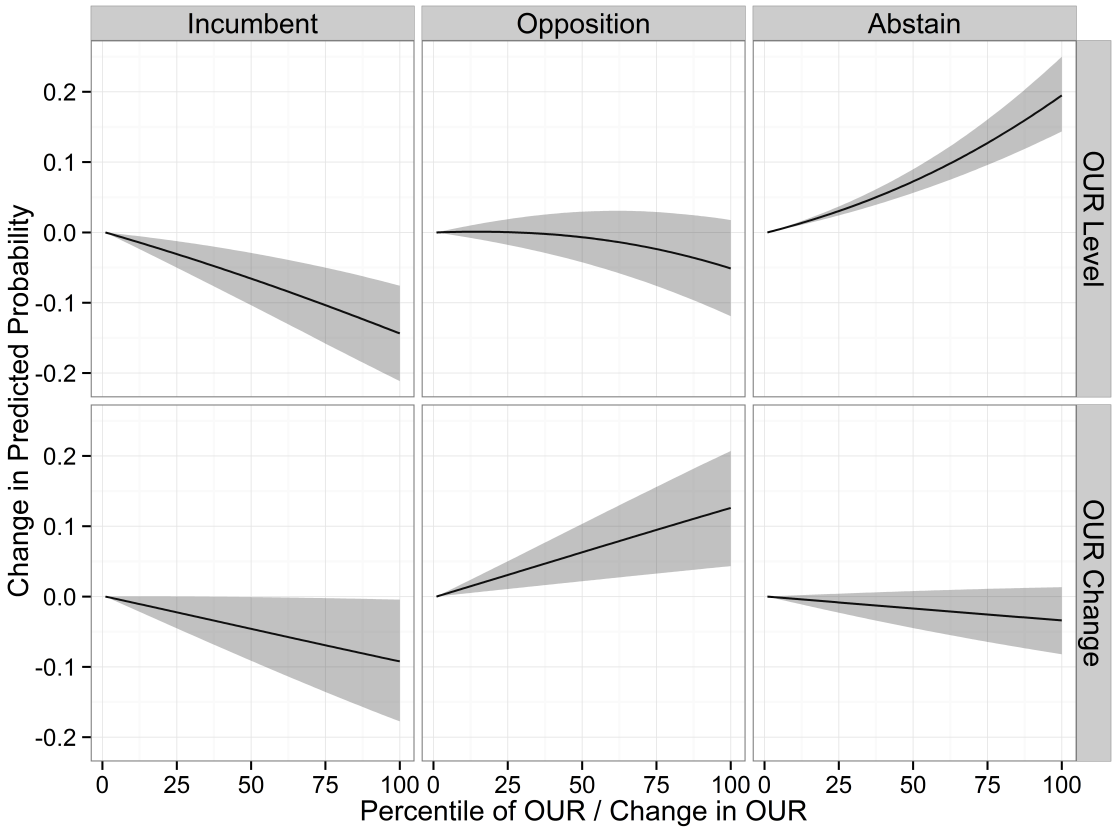
Overall, the results in table 4.1 are supportive of the hypotheses being tested. In model 1, we see that both levels and changes in occupational unemployment increase the probability of individuals voting for the opposition, compared to the incumbent. Perhaps unsurprisingly, higher national unemployment rates are also associated with a higher proportion of voters supporting the opposition, compared to the incumbent. These are standard economic voting effects, whereby voters punish the incumbent for poor performance. Importantly, for our purposes, the measures for occupational unemployment are statistically and substantively significant, even when controlling for the state of the aggregate economy. This provides evidence in favor of hypothesis 1, which suggested that high and rising employment insecurity should reduce the probability of voting for the incumbent, regardless of the actual state of the aggregate economy.

Model 1 also provides evidence for the effects of employment insecurity on turnout, although levels of and changes in occupational unemployment seem to have divergent effects on the decision to abstain. Thus, while proximate changes in employment insecurity have no discernible effects, the level of employment insecurity has large and robust effects. These results strongly suggest, at least when it comes to occupational insecurity, that prolonged economic vulnerability results in a greater preoccupation with personal matters and decreases the time afforded to politics (e.g., Tillman, 2007).

Variable	Model 1	Model 2
Opposition Vote results		
Intercept Equation, β_{1s0}		
Intercept	0.35 (0.09)*	0.35 (0.09)*
Unemployment Rate (%)	0.26 (0.10)*	0.24 (0.10)*
Economic Growth (%)	0.02 (0.09)	-0.01 (0.09)
Inc. Partisanship		-0.15 (0.09)
Inc. Last Vote Share (%)	-0.45 (0.09)*	-0.48 (0.08)*
OUR Equations, β_{1s1} **and** β_{1s2}		
OUR Level	0.03 (0.02)*	0.03 (0.02)
OUR Level $\times$ Inc. Partisanship		-0.04 (0.01)*
OUR Δ	0.04 (0.02)*	0.04 (0.02)*
OUR Δ $\times$ Inc. Partisanship		0.02 (0.02)
Abstain results		
Intercept Equation, β_{2s0}		
Intercept	-0.95 (0.11)*	-0.96 (0.12)*
Unemployment Rate (%)	0.39 (0.12)*	0.38 (0.12)*
Economic Growth (%)	0.04 (0.12)	0.06 (0.12)
Inc. Partisanship		0.14 (0.12)
Last Turnout (%)	-0.66 (0.10)*	-0.69 (0.09)*
OUR Equations β_{2s1} **and** β_{2s2}		
OUR Level	0.17 (0.02)*	0.18 (0.02)*
OUR Level $\times$ Inc. Partisanship		0.04 (0.02)
OUR Δ	0.00 (0.02)	0.00 (0.02)
OUR Δ $\times$ Inc. Partisanship		0.05 (0.02)*
Variance Components		
Opposition Vote, ω_{11}	0.58 (0.29)*	0.56 (0.28)*
Abstain, ω_{22}	0.73 (0.36)*	0.73 (0.36)*
Correlation, ρ	0.53 (0.12)*	0.61 (0.11)*

Note: Posterior means and posterior standard deviations (in parentheses) shown, based on 30,000 MCMC samples. N=28,299 (respondents); J=43 (country-elections). Voting for an incumbent party is the baseline outcome. Control variables not reported: Age, age-squared, gender, education, income, left-right ideology, union-membership, and an indicator for unemployment. An * signifies that the 95% HDI does not include 0.

Table 4.1 – Hierarchical Multinomial Regression Model for Voting Behavior



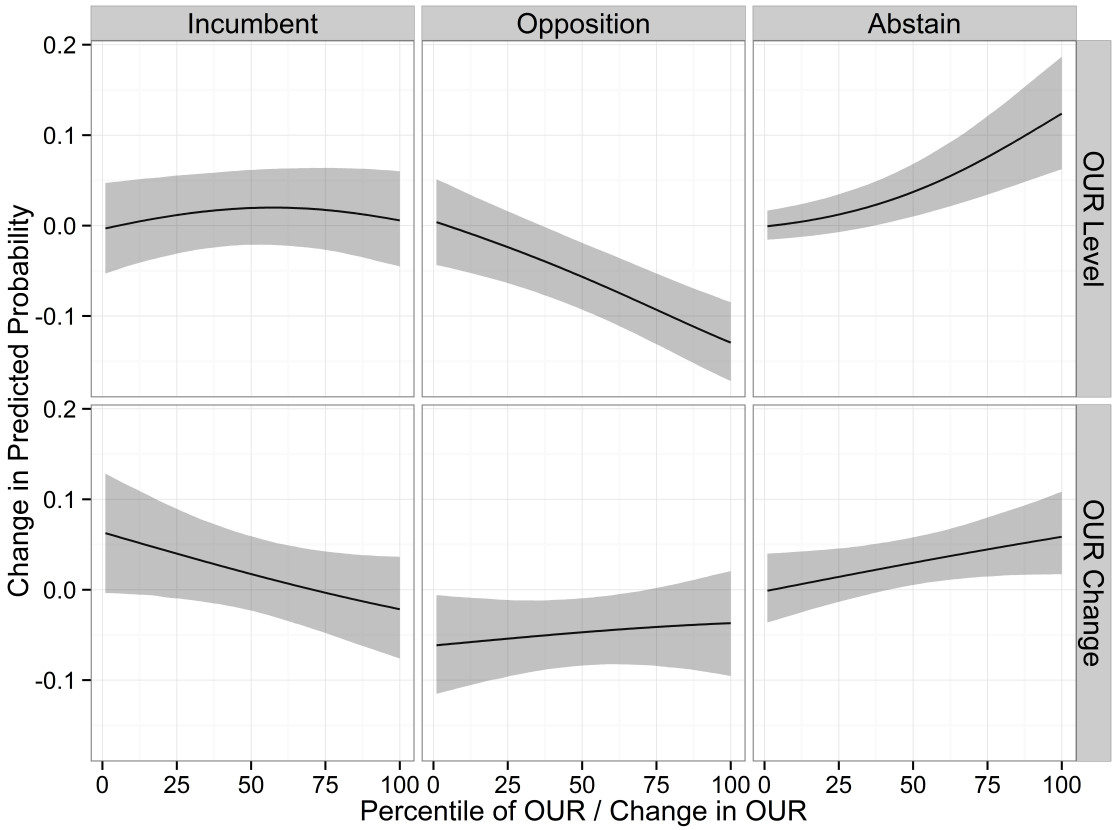
Note: The figure is based on results from model 1 in table 4.1. The upper row shows the estimated effects of the level of occupational unemployment (OUR Level), while the lower row shows the estimated effects of yearly changes in occupational unemployment (OUR Change). The columns show, respectively, changes in the predicted probability of voting for an incumbent party, an opposition party, and abstaining from voting, when OUR (separately for level and change) moves from its lowest value to its highest value. Positive values indicate a higher probability of a specific vote choice. All other variables are held at their mean. 95% highest density intervals shown.

Figure 4.2 – Predicted Changes in Voting Behavior Under High (or Rising) Occupational Unemployment

Figure 4.2 shows the substantive effects of these results on voting behavior. In the case of occupational unemployment levels, a shift from the lowest to the highest value recorded in the dataset is associated with a 14% lower probability of voting for the incumbent, a 5% lower probability of voting for the opposition, and a 19% higher probability of abstaining.

Thus, the figure clearly illustrates the demobilizing effects of high occupational unemployment. For changes in occupational unemployment, the results are more in line with standard results from the economic voting literature — thus, shifting from the minimum to the maximum increase in occupational unemployment in the dataset is associated with a 9% lower probability of supporting the incumbent, 4% lower probability of abstaining, and a 13% higher probability of voting for the opposition. These results hint that there may be important differences in the short-term and long-term effects of employment insecurity, with the short-term effects being more in line with the valence effect posited in the economic voting literature, while the long-term effects are more in line with the literature on redistributive preferences.

Model 2 sheds more light on these results, as it relaxes the assumption that the effects of employment insecurity are unaffected by the partisanship of the incumbent government. Model 2, thus, allows us to directly test hypotheses 3 and 4. Considering first the effects of the level of occupational unemployment, we see that they differ significantly based on the partisan identify of the incumbent — thus, higher employment insecurity is associated with a lower likelihood of voting for the opposition, relative to the incumbent, under more left-wing incumbents. Conversely, the effects of changes in employment insecurity on voting for the opposition do not seem to vary across incumbent partisanship. On the other hand, changes do increase the likelihood of abstaining, relative to voting for the incumbent, under more left-wing governments. Levels of occupational unemployment, however, do not exert varying effects across incumbent partisanship on the likelihood of voting for the incumbent.



Note: The figure is based on results from model 2 in table 4.1. Each subfigure shows the difference in the effects of OUR (Level or Change) on voting behavior under left-wing incumbent governments, on the one hand, and right-wing incumbent governments, on the other hand. Positive values indicate a higher probability of a specific vote choice under left-wing incumbents, as compared to right-wing incumbents. All other variables are held at their mean. 95% highest density intervals shown.

Figure 4.3 – Marginal Effect of Government Partisanship on Voting Behavior Under High (or Rising) Occupational Unemployment

Figure 4.3 presents these results graphically. The subfigures capture the marginal effect, at various values of levels and change in occupational unemployment, of shifting from a right-wing incumbent government to a left-wing incumbent government. Thus, the figure allows us to discern how incumbent partisanship conditions the effects of employment

insecurity. Comparing the probability of voting for the opposition and abstaining for voting at different levels of occupational unemployment is particularly telling (upper row, two rightmost figures). At low levels of occupational unemployment (left side of figures), government partisanship has negligible conditioning effects on the relationship between vote choice and employment insecurity — the marginal change in voting behavior is essentially 0. However, at high levels of occupational unemployment, the economically insecure respond in markedly different ways, based on the partisan identity of the incumbent. Namely, under a left-wing incumbent they are considerably more likely to abstain from voting all together and less likely to support the opposition. These results confirm the findings in model 1, and provide strong support for our positional account. Under a right-wing government, economically insecure voters shift to the opposition more than they exit the electoral arena. Meanwhile, under a left-wing government, voters leave the electoral arena at a greater rate than they cast a vote for the opposition. Thus, the partisanship of the incumbent matters greatly for the voting behavior observed, because of how occupational unemployment makes voters more insecure, and thus more supportive of left-wing parties. This can also be seen using the change in occupational unemployment measure, as voters are more like to abstain as occupational unemployment increases under a left-wing government than under a right-wing government. Hypotheses 3 and 4 are, thus, mostly supported in this analysis.

As the previous discussion should make clear, the results differ somewhat depending on what measure of occupational unemployment is used. For levels of the variable, the results are distinctly representative of a positional effect, while for changes in the variable, the results are mostly representative of a valence effect. What might explain these heterogenous effects across levels and changes of occupational unemployment? One possibility is that

levels of occupational unemployment capture more long-term effects, such as changes in policy preferences and broader attitudes (or perhaps even values), while changes in occupational unemployment capture short-term effects, such as immediate reactions toward the incumbent government for changes in the economy. The latter judgments are about the economy and the incumbent, while the former are attitudes about policies and the distribution of resources in society, which are more deeply held perspectives. We further discuss this possibility below.

4.4.1 Exploring the Mechanism

The previous analysis has shown that high unemployment in an individual's occupation is associated with a greater tendency to support opposition parties, rather than incumbent parties, and a higher probability of abstaining from voting. Importantly, the question still remains: Are these findings driven by the informational heuristic suggested in the theoretical section or an alternative mechanism?

A plausible alternative explanation is that individuals employed in occupations with a high unemployment rate are simply more concerned about unemployment, and while their perceptions of unemployment levels, and economic performance more broadly, might be similar to those less insecure, they are more likely to react to the same indicators through their voting behavior (e.g., Powell and Whitten, 1993; Weatherford, 1978). Secondly, such greater sensitivity toward economic insecurity might lead to a higher motivation to seek out information about unemployment, as well as economic information more broadly (Holbrook and Garand, 1996; Singer, 2013). The recent work on the "salience" of the economy (e.g., Fossati, 2014; Singer, 2011), for example, implicitly assumes that voters perceive the same economic performance, but that their personal circumstances lead

them to pay greater attention, or attach a greater weight, to economic conditions, in determining their voting behavior. As such, this perspective is consistent with both of these alternative explanations.

These two plausible alternative explanations produce specific expectations. First, voters should not differ in their perception of overall unemplyoment rates in the economy, but would differ in the value, or importance, they attach to such indicators. Second, voters might be more motivated to understand the overall unemployment rates in the economy, due to their greater sensitivity to this outcome, resulting in a more accurate perception of the level of aggregate unemployment.

Our claim, on the other hand, is that voters simply perceive different economies, based in large part on the group-based information heuristic of occupational unemployment. We agree with Stevenson and Duch (2013) that people vary in their perception of the "true" economy, but we emphasize how this perception is strongly driven by their experiences in the workplace and the labor market. This implies that a higher level of occupational unemployment should increase the tendency of overestimating the aggregate level of unemployment in the economy, and, equally, have less accurate perceptions of aggregate unemployment levels. As an informational heuristic, there should be no difference in voters' motivation to seek out further information across levels of occupational insecurity. To put it differently, our theory assumes that more economically insecure voters are not any more sensitive or motivated around the issue of unemployment; only that they have a different set of beliefs about the level of unemployment, resulting from the different informational heuristics that they use. This is perfectly compatible with a stronger association between economic conditions and voting behavior, although the mechanism

is one of different perceptions and beliefs, not heterogeneity in salience or sensitivity. More directly, a higher level of occupational unemployment should also be associated with greater subjective insecurity, thus confirming the validity of our measure of employment insecurity.

Unfortunately, the CSES dataset employed in the main analysis does not offer specific measures of these intervening factors, which could be used to test the posited mechanism directly. To test our predictions against these alternative hypotheses, we turn to the European Social Survey (ESS, 2008), which provides a battery of questions related to the subject of unemployment and employment insecurity. In particular, the survey gauged individuals' perception of the national unemployment rate, with a question asking how many working age individuals (out of 100) in their country they believed were currently unemployed and looking for work, with respondents' answers given on a 11 point scale. In order to estimate the accuracy of respondents' perceptions, we recoded the actual unemployment rate to match the 11 point scale of respondents' perceptions, with the absolute difference between the measures serving as our operationalization of respondent's accuracy about national unemployment. We reverse code the variable, such that higher values imply greater accuracy in perceptions. Additionally, respondents were asked how likely they thought that they would become unemployed for at least four consecutive weeks in the next 12 months after the survey, with answers ranging from "not at all likely" to "very likely".

We limit the sample to employed respondent and end up with a sample of 13,536 respondents in 22 European countries, surveyed between 2008 and 2010. We include a series

of controls when estimating the effect of OUR on these three measures. We include respondents's age, age-squared, household income, education, political left-right ideology, as well as dummies for respondents who are female or union members. Moreover, to control for heterogeneity in respondents' political interest, we create an additive index of their self-reported interest in politics, their belief that politics is too complicated to understand, and their difficulty in making up their mind about political issues (α=0.79). Finally, we include random intercepts at the country level, to allow for unobserved correlation between respondents living within the same country. Descriptive statistics of all variables are included in the appendix.

	Model 3	Model 4	Model 5
Intercept	2.00 (0.07)*	4.71 (0.30)*	−2.70 (0.20)*
OUR Level	0.02 (0.00)*	0.04 (0.01)*	−0.04 (0.01)*
OUR Δ	0.02 (0.01)*	−0.03 (0.02)	0.05 (0.02)*
Age	−0.03 (0.00)*	−0.07 (0.01)*	0.06 (0.01)*
Age Squared	0.00 (0.00)*	0.00 (0.00)*	−0.00 (0.00)*
Female	−0.04 (0.02)*	0.62 (0.04)*	−0.57 (0.04)*
Education	−0.01 (0.00)*	−0.08 (0.01)*	0.08 (0.01)*
Household Income	−0.05 (0.00)*	−0.11 (0.01)*	0.10 (0.01)*
Union Member	−0.00 (0.02)	0.11 (0.05)*	−0.11 (0.04)*
Political Ideology	−0.01 (0.00)*	−0.02 (0.01)*	0.02 (0.01)
Political Interest	−0.01 (0.00)*	−0.12 (0.01)*	0.12 (0.01)*
BIC	36599.40	65190.79	63770.59
Respondents	14296	14191	14191
Countries	22	22	22
Election Variance	0.12	1.98	0.90
Residual Variance	0.74	5.66	5.13

*p $<$ 0.05. Standard errors in parentheses. Model 3 DV: Likelihood of becoming unemployed in next 12 months. Model 4 DV: Perception of national unemployment rate. Model 5 DV: Accuracy of perceptions.

Table 4.2 – Mixed Effects Models of Economic Perception

Table 4.2 reports the results from a series of multilevel linear regression, which are mostly consistent with the mechanism posited in this paper. Model 3 demonstrates that individuals suffering from greater occupational unemployment, both as an overall level and as a change from the previous year, do indeed feel more insecure about their employment, reporting a significantly greater likelihood of becoming unemployed in the next 12 months. More importantly, model 4 indicates that greater level of occupational unemployment result in higher perceptions of national unemployment rates. Thus, beliefs about unemployment vary systematically with occupational unemployment. As model 5 shows, such beliefs are not necessary more accurate, with regards to the overall level of unemployment in the country. In fact, we see that as levels of occupational unemployment increase, individuals become *less* accurate in their perceptions of national unemployment rates.

On the other hand, a larger increase in occupational unemployment is associated with a more accurate perception, which is not consistent with an information shortcut explanation, as the perception of greater occupational unemployment should not result in a lower perceived level of aggregate unemployment. This raises the possibility that changes in employment insecurity might have a motivational effect, as individuals become more interested in learning about aggregate economic conditions. It is possible that short-term changes in employment insecurity are more closely associated with anxiety and worry (e.g., De Witte, 2005), emotional reactions which are known to trigger greater information search and learning (Valentino et al., 2009). In other words, while we find evidence for our informational mechanism, there is also some support for the idea that voters become more motivated to learn about the economy as their insecurity increases.

Both of these accounts imply that people are perceiving different economies, since perceptions vary systematically, based on employment insecurity. Thus, while further highlighting the need for more research on the different effects of levels and changes in insecurity, these results make clear the importance of no longer assuming that voters experience the same economy. The reason that voters attach different weights to various economic performances is likely not due to different sensitivities or the particular salience of these issues, but to variation in their knowledge about the state of the economy. As voters live in different information environments, the salient heuristics and cues available to them, such as occupational insecurity, are likely to play an important role in how they form an understanding of economic performance.

4.5 Conclusion

Theoretically, the literature on economic voting suggests that individuals reward and punish incumbent governments based on their evaluation of aggregate economic performance. Yet, people have fundamentally different experiences of the economy, providing them with dissimilar perceptions of the economy (Ansolabehere, Meredith, and Snowberg, 2014; Stevenson and Duch, 2013). An important predictor of such experiences, we argue and show, is people's occupation, and, in particular, the employment insecurity they experience in their profession, which is best captured by occupational unemployment. Occupational unemployment has been shown to affect economic voting (Mughan and Lacy, 2002; Rehm, 2011a), yet unemployment to date has either been conceptualized as a purely positional issue (Wright, 2012), or as a valence issue important only to some subgroups (Powell and Whitten, 1993).

We challenge these accounts by offering a theory of how high (and rising) occupational unemployment, as a measure of employment insecurity, affects both the propensity of individuals to vote for incumbent government parties and left-wing parties, as well as their likelihood of abstaining from the ballot box altogether. We argue that employment insecurity is both a valence and a positional issue. By doing so, we also heed the call of both Hacker, Rehm, and Schlesinger (2013), who argue for more careful theorizing about the mechanisms linking personal economic experiences with voting behavior, and Lewis-Beck and Nadeau (2011), who emphasize the importance of accounting for positional effects on economic voting.

More specifically, we find strong support for the hypothesis that employment insecurity increases the probability of voting for opposition parties, and that this effect is mitigated by the incumbency of a left-wing government. However, while voters suffering from high and increasing employment insecurity might be less likely to vote for the opposition against a left-wing incumbent, they are nonetheless more likely to abstain from the election completely. This supports the argument that employment insecurity can be conceptualized both as a valence issue, over which voters punish or reward the incumbent government based on its performance, and a positional issue, over which voters have distinct partisan preferences based on their exposure to economic risk. By ignoring either of these distinct effects, scholars risk misestimating the effects of employment insecurity on voting behavior.

When voters encounter a situation of high employment insecurity under a left-wing incumbent government, they face a dilemma. Their heightened insecurity makes them prefer a left-wing party, who might increase social protections or employment securities.

But, the poor performance of the current left-wing incumbent on this issue makes them less confident about their competence and ability to deliver this desired outcome. Hence, insecure voters become more likely to exit the electoral arena altogether, as our results show across 43 elections in Europe. Thus, left-wing incumbents are punished, albeit in a less direct way. The electoral effects might be comparable on aggregate (as lower turnout among the incumbent's supporters benefits the opposition), but the voting behavior is quite different, as are the implications for democratic accountability and party strategies.

While the empirical analysis is largely consistent with our hypotheses, our results hint at the possibility of differences in the long-term and short-term effects of employment insecurity. Short-term changes in occupational insecurity might operate in a manner similar to a valence issue, with individuals becoming more likely to punish the incumbent or exit the electoral arena when faced with an underperforming left-wing government. Alternatively, the long-term effects of employment insecurity, as represented by levels of occupational unemployment, might be more distinctly positional, with those that are economically vulnerable becoming more likely to shift their vote toward left-of-center parties. In fact, under a left-wing incumbent, higher levels of occupational unemployment are associated with a greater likelihood of voting for the incumbent, instead of the opposition, at the same time that increases in occupational unemployment are associated with a greater likelihood of abstaining, than voting for the incumbent.

Such a distinction between levels and change in occupational unemployment is speculative and future research should tease these two mechanism apart, perhaps using panel data. However, such a temporal differentiation is plausible, given how policy preferences

tend to be more stable and slow-moving, while reactions to current economic performances tend to result in more immediate shifts in attitudes toward economic conditions. In a sense, these results are consistent with the idea that people are slow to update their distributional interests and learn the full consequences of economic condition change, but that their immidiate reaction to the economic changes tend to be more abrupt.

All told, this paper presented evidence for the theory that occupational unemployment serves as an informational heuristic for voters making up their mind about the state of the economy and individual insecurity. This heuristic is associated with vote choice and turnout, and exhibits both valence and positional effects. By utilizing a salient and influential group-based heuristic, we seek to provide a clear measure relevant for people's economic experience. While we know that people are fairly accurate at understanding and perceiving economic factors that are directly and closely relevant to them, they do tend to be quite misinformed or unaware about more distant factors, such as most national indicators of the economy (Ansolabehere, Meredith, and Snowberg, 2014).

As is becomes clear that we should avoid assuming that voters experience and perceive the same economy, the impetus is on advancing our understanding of how voters actually form opinions about the economy. It is our hope that this paper takes a step in that direction, complementing the growing work on partisan biases in economic judgments (e.g., Evans and Andersen, 2006). Next steps could include comparisons of local versus group-based informational heuristics, as well as a fuller analysis of the connection between such heuristics and egocentric and sociotropic economic evaluations, as well as their mediating role in affecting economic voting. It is time that we start taking differences in individual

economic experiences seriously, as it has important consequences their for voting behavior.

Chapter 5

Unleashing the "Money Machine": The Domestic Political Foundations of VAT Adoption

> *"...some members were [...] concerned that introducing a VAT would lead to higher total tax collections over time and facilitate the development of a larger federal government – in other words, that the VAT would be a 'money machine.'"*
>
> President's Advisory Panel on Federal Tax Reform in the United States (2005:192).

The spread of the value added tax, or VAT, has been called "the most important development in taxation over the last half century" (OECD, 2010:50).[1] After being introduced in a handful of countries in the 1960s, it is now collected in over 160 countries of the world. This includes all members of the OECD, save for the United States. The amount of government revenue raised through the VAT is also considerable: it accounts for over 20% of collected taxes in the world, with only the personal income tax and social security contributions providing a larger share of government revenues (2010:50). Given its importance for most governments of the world, it is rather surprising that relatively little attention

[1]I gratefully acknowledge comments and suggestions from Janet Box-Steffensmeier, Sarah Brooks, and Philipp Rehm, on earlier versions of this paper.

has been given to its political causes and consequences. Indeed, why has the VAT risen to prominence?

While existing explanations tend to emphasize the effects of international and economic factors on VAT adoption (e.g. Keen and Lockwood, 2010; Cizek, Lei, and Ligthart, 2012; Ufier, 2014), I take an alternative approach and theorize about the domestic political factors leading to adoption of the VAT. Building on the recent literature on the puzzling relationship between regressive taxation and welfare state generosity (Prasad and Deng, 2009; OECD, 2008), I hypothesize that generous welfare states, left-wing governments, corporatist labor market institutions and consensus building political institutions should all increase the probability of early adoption of the VAT. Conversely, residual welfare states, right-wing governments, non-corporatist labor market institutions and majoritarian political institutions should impede the adoption of the VAT.

I test the hypotheses using an event history approach and data on VAT adoptions in 22 OECD countries from 1960 to 2012. Using a variety of model specifications and robustness tests, I find strong support for the effects of corporatism and proportional representation on early adoption of the VAT. A stylized simulation suggests that countries with high levels of corporatism adopt the VAT on average 15 years earlier than countries with low levels of corporatism and countries with proportional representation adopt the VAT on average 10 years earlier than countries with a majoritarian electoral system, holding all else constant. Conversely, I find no support for the proposition that welfare state generosity or left-wing government partisanship facilitate early adoption of the VAT.

In this paper, I make three contributions to the emerging literature on the intersection of the politics of taxation and welfare states. First, I theoretically develop and provide the

first empirical analysis of the domestic political foundations of VAT adoption in the OECD. The preexisting literature has overwhelmingly focused on international and economic factors, with domestic processes largely assumed to be apolitical. Second, the research design focuses on the political decision of adopting the VAT, rather than on aggregate revenue collection through the VAT. Most work in the literature takes the latter approach, making this a novel methodological contribution to the literature, but also a qualitatively different test of the underlying theoretical mechanisms. To the extent that theories are robust to tests using different approaches, we should be more confident in their validity. Finally, I provide a test of competing theories concerning the puzzling relationship between generous welfare states and regressive taxation, finding that corporatism and proportional representation play an important role in the adoption of the VAT.

5.1 The Rise of Value Added Taxation

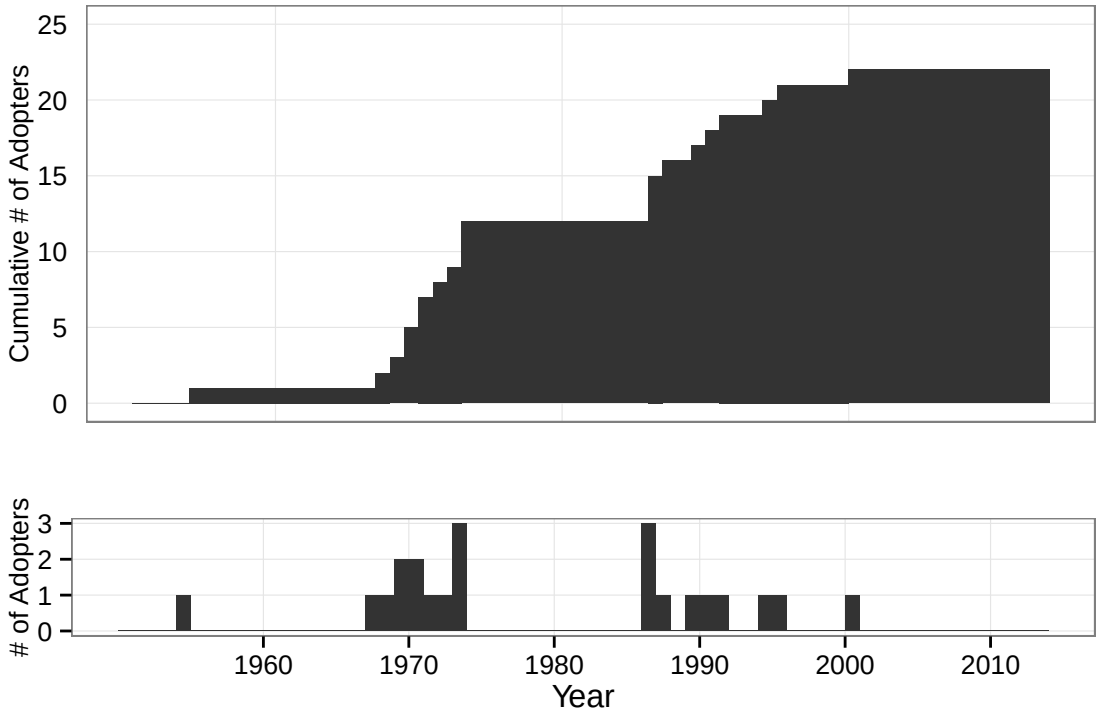
In the first half of the twentieth century, most industrialized countries adopted some form of a general consumption tax. France and Germany, the first countries to introduce the tax, did so during World War I, with many countries following their lead in the following years (Messere, de Kam, and Heady, 2003:138). The tax did not take the same form everywhere: in some countries it was only collected at the retail stage, in other countries it was only collected at the wholesale or manufacturing stage, while in still other countries it had a somewhat complex, multistage structure (2003:139). From an economic standpoint, all taxes were considered distortionary, causing inefficiencies in production and investment decisions (Ebrill et al., 2001:4).

It was in this climate that the VAT was originally formulated and implemented. The defining feature of the VAT is that it is collected at all stages of production, but allows firms

to offset the taxes they pay on their own purchases against the taxes they charge on their sales (Ebrill et al., 2001:1). The tax is thus revenue neutral, i.e. only the value-added by each firm is taxed, and is equivalent in its economic effects to a retail tax. However, and this is its main advantage from a revenue perspective, it is collected throughout the production chain, leading to smaller, but more frequent and secure tax receipts for the state (2001:3).

While the first proposals for the tax were made in the 1920s, it was fully implemented on an economy-wide basis for the first time in France in the 1950s (Ebrill et al., 2001). From there, the VAT spread across Europe and other continents in the following decades. Of the 23 OECD countries under study, five had adopted it by 1970, 12 by 1980, 17 in 1990, and 21 by 2000. As of 2014, 22 of the countries had adopted the VAT, with the US being the only country remaining without a VAT. The development of the number of countries using the VAT can be seen in Figure 1, below.

The relative ease with which governments can raise revenue with a value added tax has caused scholars and politicians alike to refer to it is a "money machine" (Keen and Lockwood, 2010; President's Advisory Panel on Federal Tax Reform, 2005). In point of fact, one estimate puts the average long run revenue effects of adopting the VAT at a 4.5% increase in overall revenue-to-GDP ratio (Keen and Lockwood, 2010:148). It does so in a relatively efficient manner compared to other major tax sources, with evidence suggesting that adoption is associated with subsequent higher economic growth and more private investment (Ufier, 2014). At the same time, however, the VAT (and consumption taxes in general) is regressive, i.e. those with lower incomes pay a larger share of their income on the VAT than those with higher incomes (Ebrill et al., 2001).



Sources: Ebrill et al. (2001) and OECD (2014a).

Figure 5.1 – VAT Adoptions in 22 OECD Countries, 1950-2014

But what explains the spread of the VAT across countries? Existing accounts tend to emphasize international pressures and the structure of the domestic economy as the most important factors contributing to VAT adoption. Empirically, the VAT has spread in regional bursts, which Keen and Lockwood (2010) attribute to some form of yardstick competition, wherein the stronger economic performance of VAT adopters spurs other countries in the region to adopt the tax. Cizek, Lei, and Ligthart (2012) reach a similar conclusion, although they argue that direct international spillover effects of adoption are limited to neighboring countries. Within the European Union this process has gone furthest, with

VAT implementation being required of all member states from 1967 onward (European Commission, 2015).

For developing countries, participation in an IMF program is another important international factor contributing to adoption. As Ufier (2014) notes, the IMF tends to encourage adoption to countries lacking a VAT and participation in a fund program is likely to increase the probability of adoption, since the fund can provide technical assistance and also because the fund can exercise direct pressure on a country via lending program conditionalities (Cizek, Lei, and Ligthart, 2012).

Although international factors are considered the most important factors contributing to adopting the VAT, domestic economic conditions also play a role in existing accounts. Thus, a high share of agricultural production to GDP is associated with a lower probability of adopting the VAT, with both Keen and Lockwood (2010) and Cizek, Lei, and Ligthart (2012) arguing that the agricultural sector is harder to tax than other sectors of the economy because of its informal nature. As such, imposing a VAT will yield less revenue for the state in an economy dominated by the agricultural sector. Another important domestic factor is whether a country has a federal structure or not. Keen and Lockwood (2010) argue that lower level federal units often have extensive power over taxation at the local level, making the implementation of a national VAT more difficult in federal states.

While both international and economic factors may play an important role, existing accounts of VAT adoption lack a clear domestic political logic for adoption. As previously noted, the VAT is costly to implement and it is relatively regressive compared to other forms of taxation. As such, adopting the tax may both involve political risk, due to high

upfront costs and uncertain benefits (especially early in the process), but it may also involve distributional conflicts — reliance on the VAT over and above other forms of taxation (such as personal income or capital taxes) shifts the overall burden of taxation from the rich to the less well off. Finally, as is echoed in the sentiment of members of the US President's Advisory Panel on Federal Tax Reform, proponents of a small state may oppose adoption of the VAT on grounds of its revenue raising capabilities. Collectively, these factors suggest that greater attention needs to be paid to the domestic political foundations of adoption.

5.2 The Domestic Political Foundations of VAT Adoption

Although value added taxation has received relatively little attention within political science, consumption taxes — and regressive taxation more generally — have been the subject of a number of contributions in recent years. This is not least so due to the puzzling empirical relationship between regressive taxation and welfare state generosity: namely, there exists an inverse relationship between the two in cross-national perspective, such that where social spending is relatively high, the tax system is relatively regressive (Prasad and Deng, 2009; OECD, 2008). From the perspective of accounts of welfare state development that emphasize either the importance of power resources (e.g. Stephens, 1979; Korpi, 1983) or income inequality (e.g. Meltzer and Richard, 1981) in the development of a generous welfare state, this poses a problem. It suggests that analyzing the welfare state purely in terms of redistribution from rich to poor oversimplifies, and possibly misrepresents, the actual distributional implications of the welfare state. For present purposes, it also suggests that VAT adoption may be intrinsically linked to welfare state developments.

A number of scholars have attempted to make sense of the puzzling relationship in those terms. In an early effort, Kato (2003) argued that large welfare states are path dependent upon the previous implementation of regressive taxes, including the VAT. Countries that adopted such taxes early on, during times of economic growth, were thus better able to expand and maintain the welfare state when they hit harder times. As Ganghof (2006) shows, however, Kato's account gets the temporality wrong: regressive taxes did not temporally precede large welfare states — large welfare states preceded reliance on regressive tax instruments.

Ganghof (2006) suggests that not only did large welfare states temporally precede reliance on regressive taxation, but that they actively caused countries to turn toward regressive tax instruments. Fundamentally, large welfare states have large revenue needs, which requires revenue from major sources of taxes to be high. The greater the need for high revenue, the higher the stakes are for implementing an efficient tax structure — what Lindert (2004:297) calls the "budget-stakes principle". Given the structural dependence of the state on capital (Przeworski and Wallerstein, 1988), this leads large welfare states to increasingly shift the burden of taxation toward regressive tax instruments, including the VAT.

In another, but more general, line of reasoning, Timmons (2005) argues that there is a long-run equilibrium between what groups in society pay in taxes and what they receive in benefits, broadly construed. Thus, the more revenue states raise from lower income groups, the more they provide services and benefits important to individuals with low income, such as basic public services and social welfare. Timmons' theory is ambivalent about the direction of causality between rising taxation and rising benefits. However,

it clearly suggests that adopting the regressive VAT should either be preceded by the growth of the welfare state or lead to such a development. The insight of Ganghof (2006) suggests that empirically the temporality ran in the former direction, i.e. large welfare states preceded regressive taxation. This leads to the following hypothesis:

H1 **Fiscal Needs of the Welfare State**. States with a generous welfare system are more likely to be early adopters of the VAT.

If the VAT plays an integral part in funding the welfare state, it stands to reason that advocates of the welfare state are more likely to have implemented the tax than opponents of the tax. According to power resources theory, the dominant approach in explaining welfare state development in advanced democracies, left-wing parties played such a role. Thus, left-wing governments which seek to redistribute income through the welfare state should be more concerned with securing the financial basis for the welfare state than other governments. As such, left-wing governments should be especially likely to adopt the VAT. Conversely, right-wing proponents of a small state should be especially likely to oppose the adoption of a new revenue-raising tax. This leads to the following hypothesis:

H2 **Left-Wing Government Partisanship**. Left-wing governments are more likely to be early adopters of the VAT.

As Beramendi and Rueda (2007) note, however, the suggestion that left-wing governments willingly promote regressive forms of taxation seems incomplete. Why shouldn't left-wing governments pursue both progressive taxation and welfare state generosity? Beramendi and Rueda (2007) propose an alternative to the "budget-stakes principle", arguing that the political-economic institutional context within which left-wing parties operate is

a critical factor in determining their taxation strategies. Unconstrained, left-wing governments should not pursue regressive forms of taxation. However, if the institutional context constrains the actions of left-wing governments, they might be forced to accept regressive taxes in order to promote a generous welfare state.

Beramendi and Rueda (2007) argue that corporatist institutions create such a constraining institutional context. Under high corporatism, labor and capital bargain with the government, with labor demanding a generous welfare state and capital demanding low taxation on profits. Paradoxically, because of the structural dependence of the state on capital, left-wing governments must, thus, support the welfare state by taxing labor more than capital. This leads to the following hypothesis:

H3 **Left-Wing Governments Constrained**. Left-wing governments are more likely to be early adopters of the VAT, but only in the context of high levels of corporatism.

But to what extent are left-wing governments then necessary actors in establishing and maintaining a regressive revenue system? Martin (2015) goes beyond Beramendi and Rueda (2007) and argues that institutions for collective political engagement — corporatist institutions included — are the primary factors leading to regressive taxation. In her account, corporatist institutions foster employers' acceptance of the welfare state, give employers influence over the structure of taxation, and build greater support for risky but collectively beneficial policies (Martin, 2015:38). Thus, corporatism itself is sufficient to shift revenue collection toward regressive forms of taxation, with government partisanship being largely irrelevant. This leads to the following hypothesis:

H4 **Corporatist Bargain**. Countries with corporatist institutions are more likely to be early adopters of the VAT.

As Martin (2015) notes, the influences of corporatism are often reinforced by political institutions that emphasize consensus building and cooperation. In such systems, coalition governments are common, parties often share responsibility for potentially unpopular decisions, such as the introduction of a new tax, and political actors are better able to pursue longer term goals, such as the funding of the welfare state (Martin, 2015:39). Hays (2003) offers a similar line of reasoning, arguing that proportional representation and coalition governments restrain the majoritarian urge to soak the risk with high capital taxation. Thus, countries with consensus building institutions are more likely to resort to regressive forms of taxation. This leads to the following hypothesis about political institutions:

H5 **Consensus Democracy**. Countries with consensus building political institutions are more likely to be early adopters of the VAT.

In sum, the hypotheses suggest that generous welfare states, left-wing governments, corporatist labor market institutions, and consensus building political institutions should all increase the probability of early adoption of the VAT. Conversely, residual welfare states, right-wing governments, non-corporatist labor market institutions, and majoritarian political institutions should impede the adoption of the VAT.

5.3 Data and Methods

In principle, the hypotheses sketched out above apply to the universe of democracies in the world, from the first adoption of the VAT to the present. However, since the hypotheses are generally derived from theories and empirical observations based on advanced democracies, and since adequate data on countries outside of the developed world at the early stages of VAT adoptions are not available, I focus on 22 OECD countries, commonly

grouped together as advanced democracies.[2] In what follows, I begin by discussing how I operationalize the main concepts relevant to the theories being tested. I then discuss the techniques I use to estimate the models in the main analysis.

5.3.1 Data Description

The dependent variable in the analysis is whether a country adopted the VAT in a given year. Ebrill et al. (2001) provide a detailed summary of the implementation years of the VAT from its launch in France in 1954 up until 2001. I have updated the list of countries with data from the OECD, so the data now reflect the adoption of the VAT up to and including 2014 (OECD, 2014a). Since we are interested in the decision to adopt the VAT, rather than the implementation of the VAT, I lead the variable by two years (i.e. if the VAT was adopted in 2000, it would be predicted based on the values of the independent variables in 1998). This is consistent with Ebrill et al.'s (2001) discussion of the time it takes to implement the VAT once it has been passed into law, as well as the approaches taken in recent empirical evaluations of adoption (Cizek, Lei, and Ligthart, 2012; Ufier, 2014). Due to data limitations, countries enter the sample in 1960 (which effectively drops France from the sample) or when permanent democracy was established (which affects Greece, Portugal, and Spain). In all 21 cases of adoption under study, the VAT has remained in place from its first year of adoption until the present (Ufier, 2014:1366).

I employ widely used measures the main explanatory variables. Welfare state generosity (H1) is operationalized in terms of social transfers as a share of GDP. Although the use

[2]The following countries are included in the analysis (year of VAT implementation in parentheses): Australia (2000), Austria (1973), Belgium (1971), Canada (1991), Denmark (1967), Finland (1994), Germany (1968), Greece (1987), Iceland (1990), Ireland (1972), Italy (1973), Japan (1989), Luxembourg (1970), the Netherlands (1969), New Zealand (1986), Norway (1970), Portugal (1986), Spain (1986), Sweden (1969), Switzerland (1995), the United Kingdom (1973), and the United States (not adopted).

of social spending to account for welfare state generosity is often criticized for being misguided (Esping-Andersen, 1990; Green-Pedersen, 2004), it is wholly appropriate in the present case. As the theory being tested suggests, it is the fiscal needs of the welfare state that should lead to the adoption of the VAT, and as such, high social spending directly contributes to those fiscal needs. Data for social spending is derived from the OECD and compiled by Armingeon et al. (2014).

I account for left-wing government partisanship (H2 and H3) with the yearly share of cabinet positions held by social democratic and other left-wing parties (Armingeon et al., 2014). To measure corporatism (H3 and H4), I use a measure of the degree of wage bargaining coordination at the national level, developed by Kenworthy (2003) and extended by Visser (2013). The measure takes on values from 1 ("fragmented wage bargaining") to 5 ("centralized bargaining by peak associations").[3] Finally, I focus on the type of electoral system used to capture the elements of consensus democracy (H5). The variable, derived from Armingeon et al. (2014), takes on a value of 1 for proportional representation and a value of 0 for mixed or majoritarian systems.

There are several plausible alternative theories and/or possible confounding factors that I control for. As is suggested by Keen and Lockwood (2010) and others, the VAT has spread in regional bursts. As such, it is necessary to control for the diffusion of the tax and/or any factors that might make adoptions cluster spatially. Following Keen and Lockwood (2010), I control for the effects of diffusion on policymaking in country i with a measure capturing prior adoptions by other countries, with the weight of each country determined by its geographical distance from country i. I operationalize geographical distances with

[3]The variable includes time-varying values for all countries in the sample, except for Iceland. Based on Siaroff (1999) and Jonsson (2014), I set the value for Iceland to 3. The final results are not sensitive to the value chosen.

the inverse distance between capitals of countries (Weidmann, Kuse, and Gleditsch, 2010). The resulting spatial lag is the sum of the inverse distances between capitals multiplied by an indicator variable showing whether country j has adopted the VAT in year $t-1$ or earlier, i.e.

$$\text{Spatial Lag}_i = \sum_{j=1}^{N} \text{dist}_{ij} \times \text{VAT}_j(t-1)$$

For robustness, I use an analogous measure substituting the geographic distance weight out for a weight based on trade flows between country pairs, with the logic being that countries that trade more heavily together should be more likely to become aware of policy adoptions in the other country. I operationalize the trade distance from country *i* to country *j* as the combined relative share of imports from and exports to country *j* (Barbieri and Keshk, 2012).

In addition to the spatially lagged variable, I include a measure capturing the effects of the European Union (and its predecessor, the European Economic Community). Given that implementing a VAT has been required of all member states from 1967 onward, I include a time varying indicator for EU-membership or EU application status that takes on a value of 1 from that year onward for all countries that are either members of the EU or official candidate countries for membership.

Prior empirical work suggests that the VAT might be more difficult to collect in countries dominated by agricultural production or with a federalist structure. Thus, I include measures of the share of the civilian labor force employed in agriculture (Armingeon et al., 2014) and an indicator variable for whether a country has a strong federal constitutional

structure (Armingeon et al., 2014). Furthermore, I control for economic development with the log of GDP per capita, in PPP terms and constant international dollars (Heston, Summers, and Aten, 2012).

5.3.2 Estimation

The hypotheses being tested are fundamentally about *when* the VAT was adopted in different countries, i.e. what made countries more or less likely to be early adopters of the VAT. To capture the process adequately, I use an event history framework, which models the time until a country i adopts the VAT as a function of the covariates outlined above (Box-Steffensmeier and Jones, 2004). While a number of different approaches can be employed within the event history framework, I use a logistic regression model with duration dependence accounted for by temporal splines.[4] Following Beck, Katz, and Tucker (1998), I estimate the following logistic model

$$P(y_{it} = 1 | x_{it}, y_{it-1} = 0) = \frac{1}{1 + e^{-(x_{it}\beta + H(t-t_i))}}$$

where x_{ij} is a vector of independent variables and β the vector of parameters being estimated. $H(t - t_i)$ is included in the model to account for any duration dependence in a flexible manner. The specific form of $H(.)$ was determined by a sequence of F-tests, with natural cubic splines with two knots being chosen. After the main empirical section, I include a number of robustness tests which deviate from the above model.

[4]While using the semi-parametric Cox proportional hazard model would in many ways be ideal, the specifics of the dependent variable suggest that a discrete duration framework might be more appropriate. The dependent variable is only measured yearly, there are many tied events in the dataset, and due to the limited number of countries under study, using a semi-parametric approach is unfeasible. Even so, I report the results of a Cox model in the robustness section.

5.4 Empirical Analysis

In this section, I estimate a series of models predicting VAT adoption based on varying model specifications. The final sample consists of 22 countries under "risk" of adoption for 401 country-years. Due to the small number of observations, I estimate each model with a limited number of control variables. All models include a linear duration variable ("Years without VAT") and two natural cubic splines to account for duration dependence.

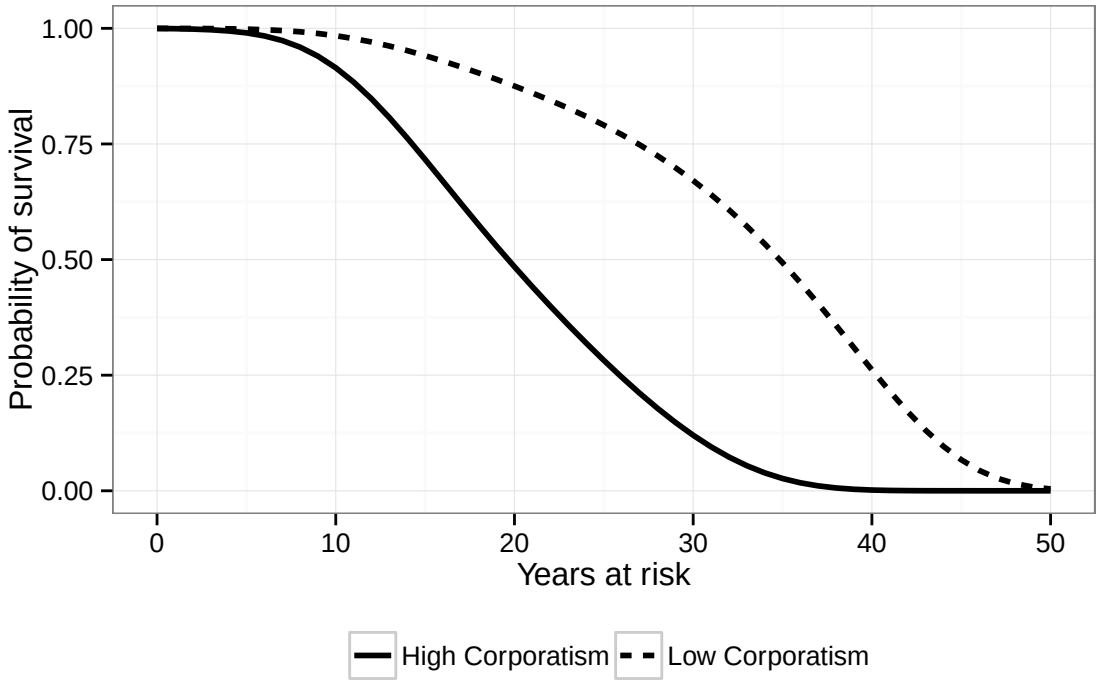
Model 1 in table 5.1 presents an initial test of four of the five hypotheses discussed above, controlling for the effects of diffusion and European Union membership (or application status). The main hypotheses find mixed support in the data. Contrary to hypotheses 1 and 2, neither welfare state generosity nor left-wing partisanship are associated with early adoption of the VAT. The former is particularly surprising, while the latter suggests that VAT adoption might not have been a partisan political issue. The latter finding is in line with recent work by Kemmerling (2015), which shows that the politics of the VAT do not follow a clear partisan logic in Germany and the UK.

As predicted by hypothesis 4, corporatism is associated with an early adoption of the VAT. Thus, Martin's (2015) emphasis on the role played by employers in the co-development of a regressive tax system and a generous welfare state is supported. The effect of corporatism is, furthermore, both statistically and substantively significant. Figure 5.2 shows the divergent survival probabilities of countries with either a low level of corporatism or high level of corporatism. In the former case, VAT adoption occurs much later than in the latter case, with the median time until adoption being 35 years versus 20 years for countries with a high level of corporatism.

	Model 1	Model 2	Model 3	Model 4	Model 5
Social Spending	0.03	0.04			
	(0.06)	(0.07)			
Left Partisanship	0.01	0.01			
	(0.01)	(0.01)			
Corporatism	0.43*	0.54*	0.36*	0.47*	0.49*
	(0.20)	(0.25)	(0.17)	(0.18)	(0.18)
Left × Corporatism		0.00			
		(0.01)			
PR System	1.31*	1.20*	1.50*	1.32*	1.74*
	(0.48)	(0.49)	(0.43)	(0.46)	(0.57)
Federalism			-0.52		
			(0.71)		
Agriculture Share			-3.99		
			(2.67)		
Ln(GDP per capita)				-0.48	
				(1.03)	
EU App/Member	3.16*	3.31*	3.12*	3.21*	3.43*
	(0.80)	(0.79)	(0.68)	(0.72)	(0.63)
Spatial Lag, Distance	-2.64+	-2.44	-1.59	-2.20	
	(1.60)	(1.55)	(1.60)	(1.44)	
Spatial Lag, Trade					-2.89+
					(1.53)
Years without VAT	0.57*	0.58*	0.49*	0.57*	0.54*
	(0.16)	(0.16)	(0.15)	(0.16)	(0.16)
Spline 1	0.19*	0.19*	0.18+	0.19*	0.20*
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Spline 2	-0.08+	-0.08+	-0.08+	-0.08+	-0.09*
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Constant	-8.18*	-8.54*	-7.18*	-6.99*	-8.29*
	(1.15)	(1.39)	(0.99)	(2.43)	(1.11)
Country-Years at Risk	401	401	401	401	401
Countries at Risk	22	22	22	22	22
BIC	184	189	185	179	174
Psuedo R2	0.25	0.25	0.24	0.24	0.24

Statistical significance: $^{+}$ $p < 0.1$; * $p < 0.05$. Robust standard errors clustered by country in parentheses.

Table 5.1 – Logit Estimates of the Probability of Adopting the VAT



Note: Low corporatism refers to a case with fragmented wage bargaining (1), while high corporatism refers to a case with centralized bargaining by peak associations (5). Other variables were set to their mean (continuous variables) or median (discrete variables) values.

Figure 5.2 – Survival Probability Plots for VAT Adoption by Corporatism

Similarly, hypothesis 5, which suggests that political institutions that foster consensus decision making, finds support in model 1. Countries with proportional representation adopt the VAT sooner than countries with majoritarian institutions, with the median time until adoption being 17 years for PR systems and 27 years for mixed and majoritarian system. This finding supports Martin's (2015) argument concerning the mutually reinforcing effects of corporatism and consensus democracy and Hays's (2003) argument highlighting the restraining effects of proportional representation on majoritarian preferences.

Model 2 in table 5.1 tests the argument made by Beramendi and Rueda (2007) concerning the constraining role of corporatist institutions on social democratic parties. Their theory does not find support in the timing of VAT adoption, with both left-wing partisanship and the interaction between the two variables remaining insignificant, while the constitutive effect of corporatism is unchanged.[5] Model 3 tests whether the effects of corporatism and proportional representation are robust to federalism and reliance on agriculture, model 4 tests whether the effects are robust to economic development, while model 5 tests whether the results change if a trade weighted measure of spatial diffusion is used, rather than a distance weighted measure. Both corporatism and proportional representation remain significant and similarly sized in all models.

Of the control variables, only the indicator variable for EU membership (or application status) remains statistically significant in all specifications. As is suggested by the literature, the European Union played a substantial role in establishing and harmonizing the VAT within the boundaries of the union. However, as the results indicate, the European Union is not the only factor influencing the decision to adopt the VAT, with domestic political factors also playing an important role.

5.4.1 Robustness Tests

The models estimated in the previous section made a number of strong assumptions about the process by which countries adopted the VAT. In this section, I present additional tests, which relax some of the main assumptions made in the primary analysis. To the extent that the results from the primary analysis remain robust, we should be more confident that they are not simply the artifact of arbitrary modeling assumptions. The baseline

[5]Note that both variables are mean centered, such that each constitutive term can be interpreted as the effect when the other constitutive term is at its mean value.

specification used for the robustness tests is identical to the one presented in model 1, although I use the trade weighted spatial lag, rather than the distance weighted lag, on the basis of the Bayesian information criterion (BIC) from model 5. Model 6 in table 5.2 shows the results of the baseline specification.

	Model 6	Model 7	Model 8	Model 9	Model 10
Social Spending	0.07	0.07	0.06	0.07	0.12
	(0.07)	(0.10)	(0.07)	(0.16)	(0.10)
Left Partisanship	0.01	0.01	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Corporatism	0.41*	0.41^{+}	0.37^{+}	0.41	0.60*
	(0.20)	(0.25)	(0.19)	(0.38)	(0.16)
PR System	1.73*	1.73*	1.50*	1.73	1.33*
	(0.60)	(0.85)	(0.58)	(2.65)	(0.61)
EU App/Member	3.19*	3.19*	2.90*	3.19^{+}	2.84*
	(0.75)	(0.88)	(0.73)	(1.81)	(0.88)
Spatial Lag, Trade	-3.68*	-3.68^{+}	-3.32^{+}	-3.68	-4.87*
	(1.79)	(2.05)	(1.75)	(3.36)	(2.25)
Years without VAT	0.55*	0.55*	0.47*	0.55	
	(0.17)	(0.23)	(0.16)	(0.48)	
Spline 1	0.20*	0.20^{+}	0.17^{+}	0.20	
	(0.10)	(0.11)	(0.09)	(0.34)	
Spline 2	-0.09^{+}	-0.09^{+}	-0.08^{+}	-0.09	
	(0.05)	(0.05)	(0.05)	(0.20)	
Constant	-8.58*	-8.58*	-7.44*	-8.58^{+}	
	(1.17)	(1.84)	(1.15)	(4.81)	
Country-Years at Risk	401	401	401	401	401
Countries at Risk	22	22	22	22	22
BIC	184	184	184	184	103
Estimation Technique	Logit	Random effects	Rare events	Logit	Cox PH
Standard Errors	RCSE	RCSE	RCSE	BCSE	RCSE

Statistical significance: $^{+}$ $p < 0.1$; * $p < 0.05$.

Table 5.2 – Logit Estimates of the Probability of Adopting the VAT: Alternative Specifications

A common concern with any event history analysis is the issue of unobserved heterogeneity, which can lead to inconsistent estimates of the parameters under study. Such an issue might arise if a relevant covariate is omitted from the model, either because it is unmeasurable, unobserved, or not known to the analyst (Box-Steffensmeier and Jones, 2004:141). Such concerns can be alleviated with the addition of a random effect which allows for the possibility that some units are more "frail" than others for unknown reasons. Model 7 in table 5.2 accounts for this possibility, with the addition of a normally distributed random effect. The results are substantively the same, for both the estimates of corporatism and electoral system type.

Another concern regarding the baseline specification is that logit estimates are biased in small sample, which is further exacerbated when events are rare. Model 8 uses King and Zeng's (2001) rare events estimator to account for this bias. Again, the results are largely unchanged. A related concern is that due to the small number of countries under study, the sampling distribution of the estimates is not normally distributed. In model 9, I address this concern, using bootstrap clustered standard errors (BCSE), rather than robust clustered standard errors (RCSE). Both of the primary variables of interest, corporatism and PR, are statistically insignificant, highlighting the potentially problematic nature of the small sample size. Finally, the parameterization of the duration dependence might be too restrictive. To account for this possibility, model 10 presents the results of a semi-parametric Cox model, which leaves the baseline hazard rate unparameterized. The results are practically unchanged from the baseline specification, with both corporatism and PR being statistically and substantively significant.

In sum, throughout the ten model specifications, corporatism, proportional representation, and EU membership (or applicant status) are generally statistically and substantively significant. In addition to providing support for the external effects of the European Union, the findings support two of the five domestic political hypotheses developed on the basis of the literature on the welfare state and regressive taxation. Both high levels of corporatism and proportional representation are associated with a considerably earlier adoption of the VAT than low levels of corporatism and majoritarianism, both collectively and on their own.[6] Conversely, welfare state generosity and left-wing government partisanship are not associated with an early adoption of the VAT, nor is the effect of corporatism conditioned by government partisanship.

5.5 Conclusion

In this paper, I apply existing theories of the relationship between welfare state generosity and regressive taxation to develop hypotheses about the adoption of the VAT. Given the important role played by the VAT in public finances and, in particular, its potential to be a "money machine" for governments, exploring the domestic political foundations of VAT adoption is a valuable addition to a literature that is largely based on exploring aggregate spending outcomes. Using an event history framework, I find that corporatism and proportional representation are both associated with early adoption of the VAT, while welfare state generosity and left-wing government partisanship are not. The findings, thus, suggest that high welfare spending do not inexorably lead to the development and

[6] As Martin (2015) notes, there is a relatively high correlation between corporatism and proportional representation (see also Hall and Soskice, 2001 and Cusack, Iversen, and Soskice, 2007). To alleviate concerns that this might bias the results, I've estimated all models with each variable separately (thus, dropping either corporatism or PR). As is to be expected, the estimate for the remaining variable is unchanged or stronger than in the models shown above.

adoption of efficient tax instruments, such as the VAT, but rather that domestic economic and political institutions play a critical role in that regard.

Given the scope conditions of existing theories and data limitations in the early stages of VAT adoptions, the current study focuses on 22 OECD countries from 1960 to 2012. However, the rise of the VAT is not isolated to the OECD countries — in 2014, over 160 countries of the world had implemented the tax. An interesting topic for further research would be to develop and apply the hypotheses of this paper to the universe of cases and explore to what extent the results are transferable to other parts of the world. While traditional corporatism might not be found to the same extend in other regions of the world, welfare state spending, left-wing government partisanship, and proportional representation should have clear implications for the adoption of the VAT in other cases.

Finally, the results of the paper support the emerging view that analyzing the welfare state without considering its revenue foundations paints a misleading picture. If generous welfare states and reliance on regressive forms of taxation are products of the same explanatory factors, such as corporatism or proportional representation, then accounts of welfare state development that focus solely on the power resources of labor unions or social democratic parties are incomplete. A more comprehensive account suggests that generous welfare states are not based solely on the ability of the poor to soak the rich, but also on economic and political institutions that foster cooperation and consensus building.

Chapter 6

Conclusion

This dissertation has presented four essays on the comparative political economy of taxation and redistribution. The first essay empirically explored the underlying dynamics of the well known empirical regularity that democracies that have proportional electoral systems spend substantively more on welfare policies than those that have majoritarian systems. The essay contributes to the literature by bringing new micro-level evidence to bear on theories seeking to explain the phenomena, and as such provides a stronger empirical foundation for evaluating the theories in question. Overall, I find robust support for more proportionality leading to increased income-based voting.

The second essay provided a theoretically-driven conceptualization of absolute and relative income shifts and argued that the conceptualization of income shifts has important implications for how we think about the effects of the economy on redistributive preferences. The essay presented a general theoretical framework, which accounts for empirical findings on both the effects of economic mobility and macroeconomic cycles on redistribution. Based on a novel experimental "redistribution game", the results indicate that expected shifts in absolute and relative income have opposite effects on preferences.

The third essay argued that employment insecurity is a critical and salient factor determining incumbent support and voter turnout. The theory developed goes beyond existing approaches by providing a better conceptualized measure of salient economic experiences, as well as highlighting that the economy can often serve both as a valance and positional issue, which can have important implications for the effects of the economy on voting behavior.

Finally, the fourth essay developed and empirically tested a theory of the domestic political foundations of the adoption of the value added tax, or VAT. Building on the recent literature on the relationship between regressive taxation and welfare state generosity, I hypothesize that generous welfare states, left-wing governments, corporatist labor market institutions, and consensus building political institutions, should all increase the probability of early adoption of the VAT. I find strong support for the effects of corporatism and proportional representation on early adoption of the VAT. Conversely, I find no support for the proposition that welfare state generosity or left-wing government partisanship facilitate early adoption of the VAT.

Although each one forms a stand-alone contribution to the literature, there are a number of shared themes across the essays. One theme, in particular, deserve mentioning: The emphasis on the importance of context for political action and preferences. In that vein, the first essay posits that electoral systems condition the effects of income on vote choice; the second essay maintains that the effects of income shocks on redistributive preferences are contingent on how broadly they are shared in society; the third essay argues that the effects of employment insecurity on voting behavior is conditioned by the partisanship of the incumbent government; and finally, the fourth essay asserts that early adoption of the

VAT was dependent on a specific constellation of domestic political factors. The dissertation, thus, takes seriously the admonition that "context matters", both theoretically and methodologically, and contributes to the growing literature on contextual determinants of political preferences, behavior, and outcomes in the realm of taxation and redistribution (Hall, 2003; Franzese, 2007).

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Appendix A

Appendix to Chapter 2

A.1 Descriptive Statistics

Variable	Operationalization	N	Min	Max	Mean	SD
Income-based voting	The sensitivity of vote choice to changes in income. (CSES, 2003, 2007, 2013, 2014) and own calculations.	97	6.6	26.7	14.6	3.9
Centrality of Middle Income Voters	The "centrality" of middle-income voters. Same sources as above.	97	0.7	21.4	2.1	2.7
District Magnitude	Average district magnitude for lower house elections (CSES, 2003, 2007, 2013, 2014; Bormann and Golder, 2013).	97	1.0	150.0	20.3	36.4
ln(Magnitude)	Above variable, logged.	97	0.0	5.0	2.2	1.2
Presidentialism	A dummy variable for presidentialism (Bormann and Golder, 2013).	97	0.0	1.0	0.2	0.4
Polity	Democracy score (Marshall and Jaggers, 2011).	97	7.0	10.0	9.6	0.8
ln(GDP)	Log of GDP per capita in constant dollars, PPP (Heston, Summers, and Aten, 2012).	97	1.1	3.9	3.1	0.6

Table A.1 – Descriptive Statistics (Continued on Next Page)

Table A.1 – Continued

Variable	Operationalization	N	Min	Max	Mean	SD
Ethnic Fractionalization	The probability of two randomly drawn individuals within a country to belong to different ethnic groups, mean-centered. (Alesina et al., 2003).	97	-0.3	0.4	-0.0	0.2
Religious Fractionalization	The probability of two randomly drawn individuals within a country to belong to different religious groups, mean-centered. (Alesina et al., 2003).	97	-0.4	0.3	-0.0	0.2
Unionization	Self-reported union membership in CSES-data. (CSES, 2003, 2007, 2013, 2014).	94	0.0	0.8	0.2	0.2
Gini	The GINI coefficient for disposable income inequality (Mahler and Jesuit, 2006; World Bank, 2012).	80	22.2	59.4	32.5	8.5

Table A.1 – Descriptive Statistics

A.2 Lower House Elections included in Main Analysis

Country	Year	Income-based voting	Country	Year	Income-based voting
Albania	2005	10.3	Mexico	2009	11.7
Australia	1996	9.4	Netherlands	1998	18.1
Australia	2004	10.6	Netherlands	2002	14.3
Australia	2007	15.7	Netherlands	2006	12.4
Austria	2008	14.3	Netherlands	2010	13.9
Belgium	1999	9.1	New Zealand	1996	14.7
Brazil	2002	19.3	New Zealand	2002	11.3
Brazil	2010	17.3	New Zealand	2008	15.7
Bulgaria	2001	16.5	Norway	1997	16.4
Canada	1997	8.3	Norway	2001	15.9
Canada	2004	13.2	Norway	2005	15.6
Canada	2008	11.3	Norway	2009	16.7
Chile	2005	15.9	Peru	2006	15.3
Chile	2009	23.1	Philippines	2004	15.0
Croatia	2007	15.7	Poland	1997	12.7
Czech Republic	1996	16.1	Poland	2001	15.8
Czech Republic	2002	19.5	Poland	2005	14.8
Czech Republic	2006	12.9	Poland	2007	11.5
Czech Republic	2010	17.7	Portugal	2002	17.2
Denmark	1998	15.9	Portugal	2005	12.3
Denmark	2001	13.6	Portugal	2009	23.6
Denmark	2007	21.2	Romania	1996	10.5
Estonia	2011	19.4	Romania	2004	17.6
Finland	2003	15.3	Slovak Republic	2010	26.7
Finland	2007	18.0	Slovenia	1996	17.4
Finland	2011	19.9	Slovenia	2004	13.1
France	2007	11.3	Slovenia	2008	15.4
Germany	1998	14.9	South Korea	2000	9.5
Germany	2002	11.0	South Korea	2004	12.3
Germany	2005	8.9	South Korea	2008	14.5
Germany	2009	15.3	Spain	1996	12.0
Greece	2009	8.9	Spain	2000	8.8
Hungary	1998	13.7	Spain	2004	14.0
Hungary	2002	10.9	Sweden	1998	15.3
Iceland	1999	12.8	Sweden	2002	17.0
Iceland	2003	18.1	Sweden	2006	15.8
Iceland	2007	16.3	Switzerland	1999	15.3
Iceland	2009	12.8	Switzerland	2003	16.3

Table A.2 – Lower House Elections Included (Continued on Next Page)

Table A.2 – Continued

Country	Year	Income-based voting	Country	Year	Income-based voting
Ireland	2002	10.9	Taiwan	1996	8.2
Ireland	2007	11.4	Taiwan	2001	12.0
Israel	1996	20.2	Thailand	2007	11.0
Israel	2003	25.9	Thailand	2011	15.6
Israel	2006	18.8	United Kingdom	1997	19.3
Italy	2006	22.0	United Kingdom	2005	19.0
Japan	1996	12.9	United States	1996	11.6
Mexico	1997	15.1	United States	2004	6.6
Mexico	2000	7.9	United States	2008	13.6
Mexico	2003	11.0	Uruguay	2009	8.3
Mexico	2006	13.8			

Table A.2 – Lower House Elections Included

A.3 Robustness Checks

	Model 1	Unlogged DM	PR	PR and Mixed
Constant	7.19	11.52*	16.57**	20.04**
	(6.64)	(6.60)	(6.65)	(6.61)
ln(Magnitude)	1.08**			
	(0.52)			
District Magnitude		0.03		
		(0.02)		
PR System			2.85**	2.30*
			(1.28)	(1.34)
Mixed System				−1.73*
				(0.97)
Presidentialism	0.12	−0.09	−1.39	−1.09
	(1.41)	(1.43)	(1.36)	(1.41)
Polity	0.75	0.59	−0.16	−0.48
	(0.88)	(0.86)	(0.89)	(0.87)
ln(GDP)	−0.70	−1.08	−0.33	−0.31
	(1.01)	(0.97)	(0.93)	(0.93)
Ethnic Fractionalization	−1.99	−2.83	−2.84	−3.27
	(2.30)	(2.18)	(2.08)	(2.13)
R^2	0.14	0.13	0.14	0.16
Num. obs.	97	97	97	97

**$p < 0.05$, *$p < 0.1$. Robust standard errors clustered by country in parentheses.

Table A.3 – Alternative Operationalizations of Electoral Systems

Appendix B

Appendix to Chapter 3

B.1 Descriptive Statistics

There was considerable variation in the tax rates chosen by subjects across the twenty sessions. The median tax rate chosen was 35%, with the lower and upper quartile marks at 15% and 62%, respectively. Table B.1 shows the sample size for each treatment condition, along with the median tax rate chosen. As can be seen, the median tax rate chosen differed considerably between treatments, ranging from a low of 25% in the *POUM* condition, to a high of 50% in both the *PODM* and *Upturn* conditions.

Treatment	Subjects	Rounds	Observations	Median Tax (%)
Control	70	10	700	35
Static	30	10	300	33
Upturn	30	10	300	50
POUM	20	10	200	25
Downturn	30	10	300	30
PODM	20	10	200	50

Table B.1 – Treatment Conditions

Table B.2 presents definitions and descriptive statistics for all variables used in the main analysis.

Variable	Operationalization	N	Min	Max	Mean	Median	SD	NAs
Observation-level								
Tax	Tax voted for (%)	2000	0.0	100.0	41.05	35.0	28.9	0
Ranking	Ranking from 1st task (Ordinal)	2000	1.0	5.0	3.00	3.0	1.4	0
Subject-level								
Average Tax	Average Tax Voted for Across 10 Rounds(%)	200	4.5	87.4	41.05	40.6	17.0	0
Average Ranking	Average Ranking Across 10 Rounds (%)	200	1.3	5.0	3.00	3.0	0.9	0
Female	Female? (Yes/No)	200	0.0	1.0	0.47	0.0	0.5	0
Family Income	Survey question: What is your family's annual income? (Ordinal)	200	1.0	9.0	7.27	8.0	2.3	11
Republican	Survey Question: Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what? (Ordinal)	200	-3.0	3.0	0.04	0.0	2.0	0
Conservatism	Survey question: Here is a 7-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place yourself on this scale? (Ordinal)	200	-3.0	3.0	-0.23	-1.0	1.8	0

Table B.2 – Detailed Descriptive Statistics

B.2 Handout from Experiment for Two Period Treatments

Thank you all for coming and agreeing to participate in our study. This study entails two parts: a small game and a short survey. Together, your participation should take no longer than 15-20 minutes. In return for your participation, beyond receiving extra credit, you will also all be provided with a $5 show up fee, and you will keep your monetary payoff from one of the rounds of the game, which will on average add up to an extra $5 reward. The extra reward will be no lower than $1 and no higher than $15, so if you follow the instructions carefully and make good decisions you may earn considerably more than the average reward.

In front of you, on the computer screen, you should see the welcome screen to the game. Let me know if that is not up on your screen right now. Each one of you has been assigned a number. This is your identification for this game, created in order to facilitate our processing of the data and provide you with your correct payoff. While there are 10 of you here, you will be playing in games of 5 participants. However, each round of the game, we are going to randomly select the two groups, and you will not be told with whom you are playing. This means that for each round, you may be playing with a different set of 4 players. But as far as each of you is concerned, the game will proceed in the same fashion, only that the people you are playing with might change in each new round – though there is no way for you to know that, since you will not know exactly who you are playing with. Is that part understood?

When playing the game, I also ask that you please refrain from talking to each other, as this game is supposed to be played alone. A failure to do may result in your monetary payment for the session being reduced.

The game will proceed as follows. There are 10 rounds in the game itself. You will perform a task (twice) in each round for 30 seconds (each).[1] After you perform it (once), you will find out what your score was and what your payoff in dollars is for that task. Now, the payoffs are fixed. So the best performer of the task will receive the highest payoff, and the 2nd best performer will receive the 2nd highest payoff, and so forth. You are essentially competing for a higher position. This will become clear as you play the game. The reverse side of this handout has a screenshot of the task that you play – the so called "slider task".[2] The purpose is to move as many of these sliders to the middle of the line, to the 50 mark, to gain points for that task. The points get added up for each task and then determine your monetary payoff for that task.

After you performed the task (once, and before you perform it again), you will be asked to vote for the share of payoffs that should be put in a group fund and allocated equally to all five participants, irrespective of their performance in the task. The chosen group contribution rate will be applied to each one's payoffs in (both of) the task(s) for that round – meaning that you vote for the contribution rate after knowing your payoff from the task (, but before knowing it for the second task). All the money that is collected by this method will then be allocated equally among the 5 players in the group. When you vote on the contribution, you will select your preferred contribution rate. When you do, you will see what the distribution of payoffs (meaning what everyone in the group will make) if that rate is selected for the entire group (meaning if it "wins"). So you can play around with different proposals, and then when you find one that you like, you press

[1]Because the *Static* condition only involves one task per round, the handout was slightly modified for that treatment. All text shown in parentheses in the handout was not included for the *Static* condition, but was included for all other treatment conditions.

[2]The screenshot shown was identical to figure B.1 in section B.5 of the supporting information.

continue, in the bottom right of the screen, and you wait for everyone else to finish before proceeding (to perform that task a second time). The contribution rate that is ultimately selected is the median contribution rate for the group for that group – meaning that the rate that falls in the middle, where 2 proposals are higher and 2 lower.

Any questions so far?

Once you have voted (and performed the task a second time), a new round will start. You do this 10 times, basically. It's important to point out that you will not find out the results of the vote (or of the second task performance) until the end of the experiment. We do this in order to ensure that each round of the game is independent of the other rounds, since reporting the outcomes might affect how you play the subsequent rounds. And your payoff for this experiment, beyond the $5 and the extra credit that each of you will receive, will be based on your payoff, after the group share has been imposed and allocated equally, from 1 randomly selected task from 1 randomly selected round from the entire game. This means that from the tasks that you end up performing, only one of them will determine part of your payoff for participating in this game. Since you do not know beforehand which task will be the important one, we advise you to do your best in each task, and pay attention to vote in each round, since one of those votes will matter for how much money you leave here with.

One final word, as well, about what happens after you finish the 10 rounds. You will then be asked to answer a short survey, with some questions about your background and some of your views. This is just so we have a sense of who participated in the survey, though, don't worry, we never take down your name or anything, so your answers and your participation in the game is perfectly anonymous. We only take down your name in

the beginning, with the consent form you signed when you came in, in order to notify your professors so that you get your course credit as well. When you finish the questionnaire, make sure to click continue all the way to the end — since the last screen tells you how much your final payment will be and what your ID number is, which you'll need to give to me so that I know how much money to give you before you leave.

That's it. Does anyone have any questions before we get started?

B.3 Experimental Treatments

The following are the text prompts subjects receive after the first effort-task, but before the voting stage in each round. This is the only manipulation that differs across the treatments.

Static (No 2nd task, thus no text displayed)

Control

For the 2nd task, you will continue to earn 1 point for each correct movement of the slider, and your possible payoffs will remain the same at $10, $6, $4, $3, and $2.

Upturn

For the 2nd task, you will continue to earn 1 point for each correct movement of the slider but your possible payoffs will now possibly change.

There is now a 70% probability that the possible payoffs for this task will be $15, $8, $6, $4.5, and $3, and a 30% probability that the possible payoffs for this task will remain the same as before.

Downturn

For the 2nd task, you will continue to earn 1 point for each correct movement of the slider but your possible payoffs will now possibly change.

There is now a 70% probability that the possible payoffs for this task will be $5, $3, $2, $1.5, and $1, and a 30% probability that the possible payoffs for this task will remain the same as before.

POUM (Half of group, other half get Control)

For the 2nd task, your possible payoffs will remain the same at $10, $6, $4, $3, and $2, but your earnings for each correct movement of the slider will now change.

One half of the players today, including yourself, have been randomly selected to earn 2 points for each correct movement of the slider in the 2nd task. Thus, you will now earn 2 points for each correct movement. The other half of the players today will continue to earn 1 point for each correct movement of the slider.

PODM (Half of group, other half get Control)

For the 2nd task, your possible payoffs will remain the same at $10, $6, $4, $3, and $2, but your earnings for each correct movement of the slider will now change.

One half of the players today, including yourself, have been randomly selected to earn 0.5 points for each correct movement of the slider in the 2nd task. Thus, you will now earn 0.5 points for each correct movement. The other half of the players today will continue to earn 1 point for each correct movement of the slider.

B.4 Survey Questions

The following questions were included in the post-experiment questionnaire. Participants were informed that they did not need to answer any question they did not wish to answer. Titles in brackets were not included in the survey and are only shown for clarification.

Screen 1 [Basic Information]

- What is your family's annual income? (9 ordered choices)
- When you think about your ethnic background, which category do you fall into? (7 nominal choices)
- What is your sex? (2 nominal choices)
- What is your major? (free text)
- Thinking back on the game you just played for 10 rounds, how would you say that it made you feel? (7 point scale)
- Would you say that playing the game made you feel upset? (4 point scale)
- Have you participated in a research experiment at UNIVERSITY before? (Yes/No)

Screen 2 [Politics]

- Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what? (7 point scale)
- Here is a 7-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place yourself on this scale? (7 point scale)

Screen 3 [Risk]

- Suppose in a lottery game, the possibility of winning $1,000 is 10%. How much would you pay at most to buy a lottery ticket? (free text)
- Imagine the conditions change. Suppose you are offered $100 in cash. However, you can instead choose a lottery ticket, which has a prize of $2,000, but the probability of winning is not yet determined. How high should this probability be, at a minimum, for you to take the lottery ticket instead of the $100 in cash? (free text)
- How do you see yourself: are you generally a person that is fully prepared to take risks, or do you try to avoid taking risks? Please select on the scale below, where the value 1 means "unwilling to take risks" and the value 10 means "fully prepared to take risks" (10 point scale)
- Which of the two alternatives would you chose?
  - To receive $2,400 with certainty
  - A 25% chance of winning $10,000
- Which of the two alternatives would you chose?
  - To lose $2,400 with certainty
  - A 25% chance of losing $10,000

Screen 4 [Egalitarian Attitudes]

- Please state how strongly you agree or disagree with the following statements: (7 point scale for each)

  - If incomes were more equal, nothing would motivate people to work hard
  - Under a free market system, people tend to get the outcomes they deserve
  - Making incomes more equal means socialism, and that deprives people of individual freedoms
  - Equal distribution of resources is unnatural
  - They way the free market system operates in the United States is fair

Screen 5 [Role of Government]

- Please state how strongly you agree or disagree that it should be the government's responsibility to: (7 point scale for each)
  - Provide for the elderly
  - Provide jobs
  - Reduce income differences
  - Provide health care
  - Provide education

Screen 6 [Manipulation Checks]

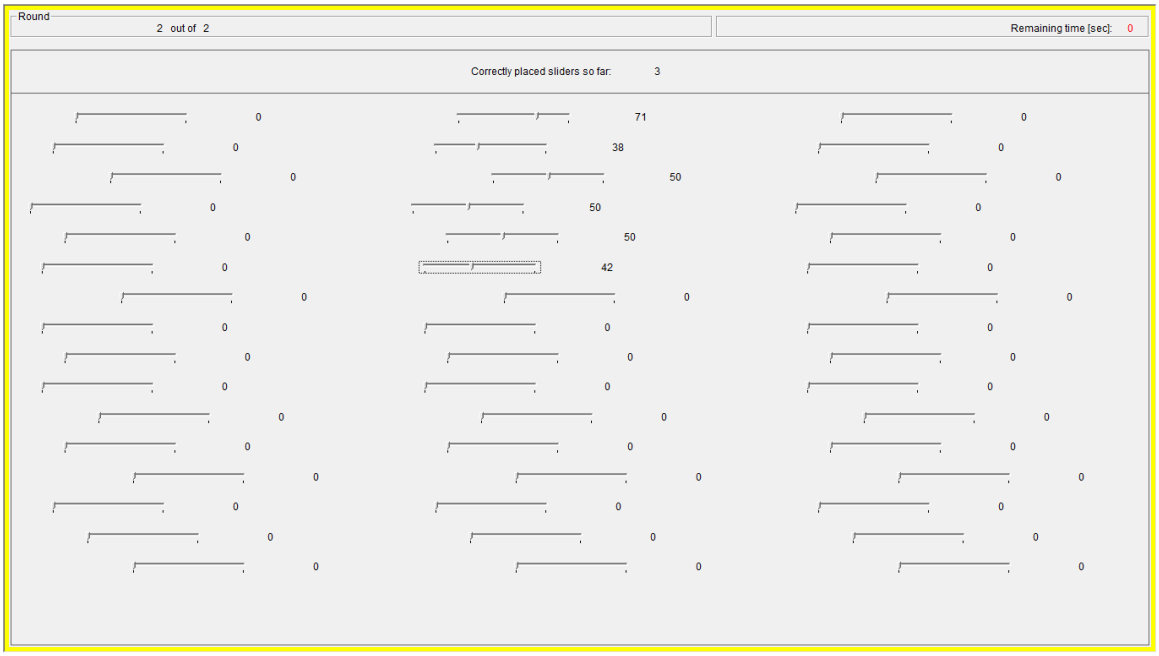
- In your opinion, how fair was the game you just played for 10 rounds? (5 point scale)

- How much do you agree with the following statement: "If players focused and tried really hard to do well on the slider task, they were able to end up with a higher monetary reward in the game." (7 point scale)
- How much do you agree with the following statement: "The slider task was based on luck more than effort." (7 point scale)
- In each round, your position after the second task was never reported. If you had to guess, how would you say that your position compared between the first task and the second task, in general over the 10 rounds you played? (5 ordered choices)
- In each round, your payoff after the second task was never reported. If you had to guess, how would you say that your payoff compared between the first task and the second task, in general over the 10 rounds you played? (5 ordered choices)
- Thinking back on the voting phase of each round of the game, would you say that voting for the share rate was confusing and difficult? (5 point scale)

Screen 7 [Thank you]

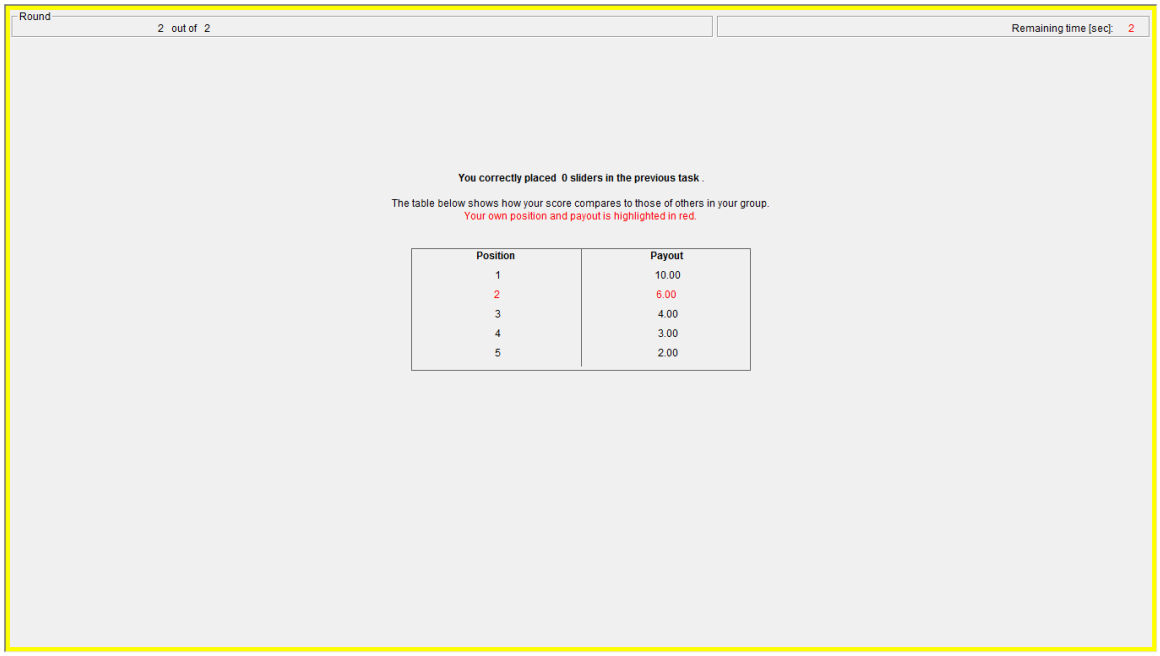
Information on earnings given.

B.5 Screenshots from Program



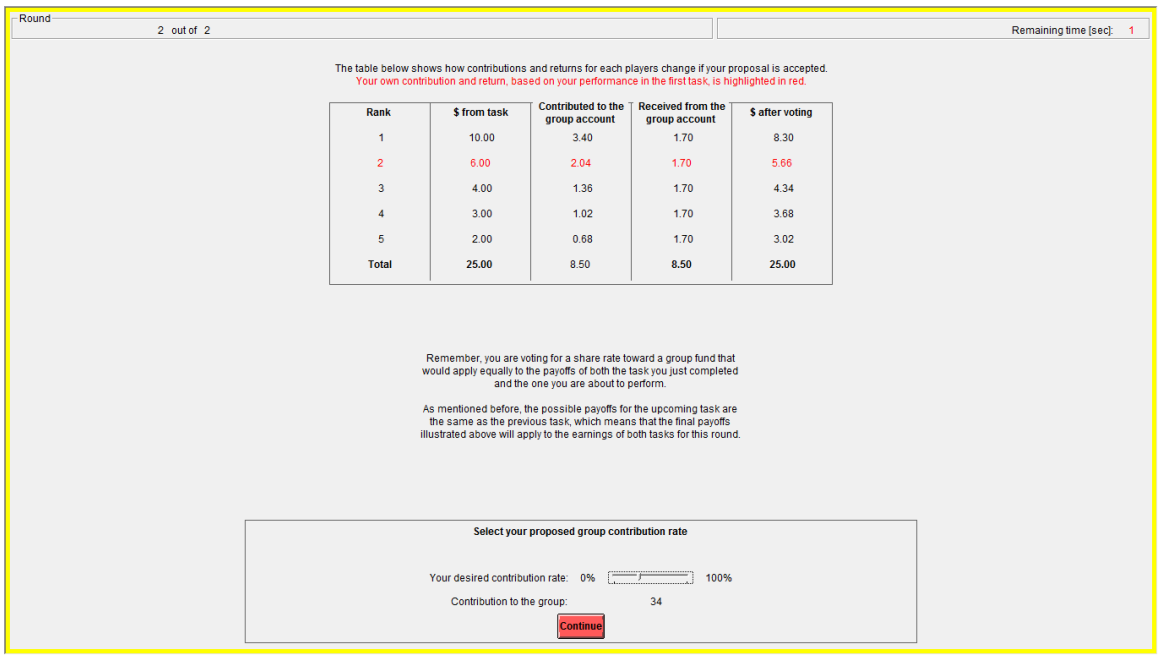
The effort task involves moving sliders with a range from 0 to 100 exactly to the value 50.

Figure B.1 – The Effort Task



This is the screen subjects see after completing the first task in each round. Notice that their relative rank is highlighted (in red).

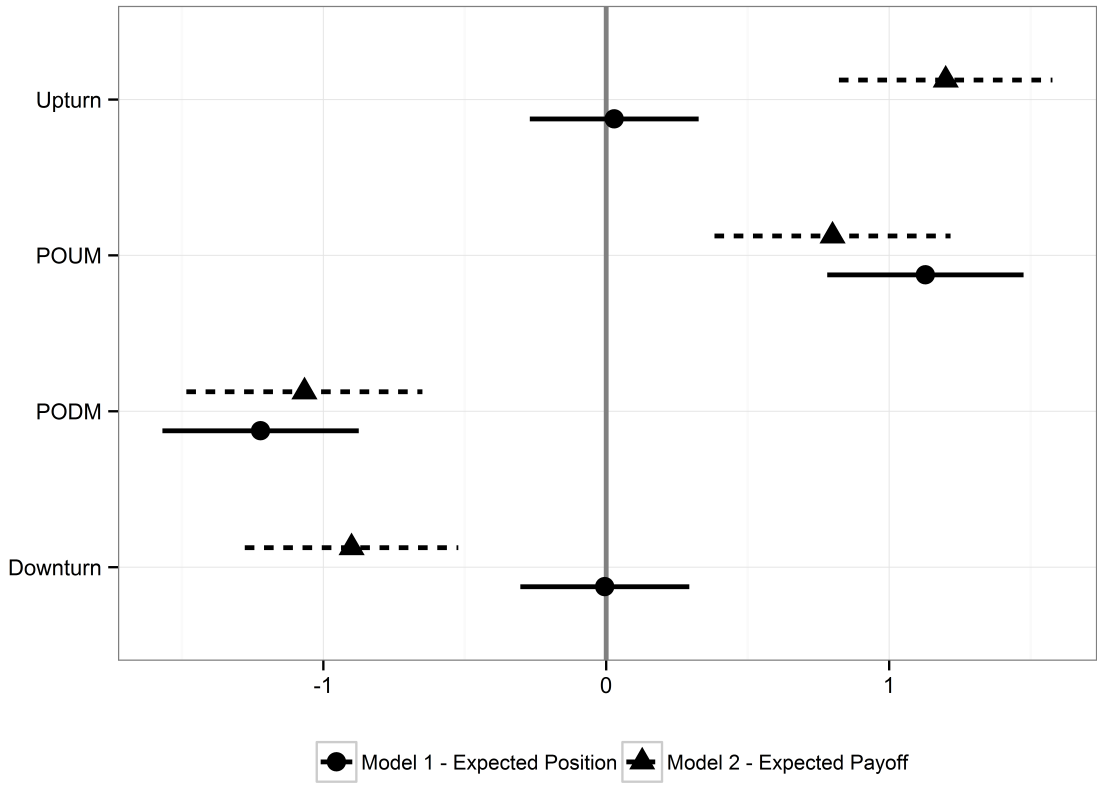
Figure B.2 – Ranking after 1st Task



This is the voting stage. Subjects select the tax rate using the slider near the bottom of the screen. When they move the slider the values in the table update to reflect how the chosen tax rate affects the payout of every player.

Figure B.3 – The Voting Stage

B.6 Manipulation Checks



Note: Point estimate and 95% confidence intervals shown.

Figure B.4 – Manipulations Checks

Notes: The dependent variable in model 1 are answers (on a five point scale) to the question *"In each round, your position after the second task was never reported. If you had to guess, how would you say that your position compared between the first task and the second task, in general over the 10 rounds you played?"*.

The dependent variable in model 2 are answers (on a five point scale) to the question *"In each round, your payoff after the second task was never reported. If you had to guess,*

how would you say that your payoff compared between the first task and the second task, in general over the 10 rounds you played?"

Indicator variables for the four forward looking treatment conditions were the only independent variables in both models — the *Control* condition was the omitted category. As a consequence, the coefficients should be interpreted as the difference between each treatment condition and the *Control*.

The results show that the manipulations worked as expected, compared to the *Control* condition. The results for model 1 show that the *POUM* and *PODM* conditions affected the expected rank of subjects, while the *Upturn* and *Downturn* conditions did not. The results for model 2 show that all conditions affected the expected payout of subjects in the correct direction. Thus, subjects in the *PODM* and *Downturn* conditions expected to receive a lower payout than those in the *Control* condition, while the opposite held true for the *POUM* and *Upturn* conditions.

Appendix C

Appendix to Chapter 4

C.1 Descriptive Statistics

Variable	Operationalization	N	Min	Max	Mean	SD
Respondent-level						
Inc. Vote	Voted for incumbent (%)	28299	0.00	1.00	0.36	0.48
Opp. Vote	Voted for opposition (%)	28299	0.00	1.00	0.48	0.50
Abstained	Abstained (%)	28299	0.00	1.00	0.16	0.37
Age	Age in years	28299	-26.87	43.67	0.29	11.44
Female?	Gender indicator	28299	-0.59	0.68	-0.02	0.50
Education	Education in years	28299	-4.98	3.82	0.06	1.57
Income	Income quintile of household	28299	-2.92	2.26	0.05	1.29
Left-Right Ideology	11-point scale	28299	-6.13	6.17	-0.02	2.21
Union Member?	Union indicator	28299	-0.88	0.92	0.01	0.42
Unemployed?	Unemployed indicator	28299	-0.30	1.00	-0.01	0.24
OUR	Occupational unemployment rate (OUR) (Rehm, 2009, 2011b).	28299	-18.15	24.18	-0.08	4.29
OUR Δ	Change in OUR from previous year	28299	-4.86	5.18	0.00	0.77
Country/Election-level						
Unemp. Rate	National unemployment rate (World Bank, 2012).	43	2.30	22.00	7.50	4.20
GDP Growth	Economic growth (Heston, Summers, and Aten, 2012).	43	-0.07	0.08	0.02	0.03
Inc. Partisanship	Partisanship of incumbent government (Left $\Uparrow$)	43	0.00	1.00	0.53	0.36
Inc. Last Vote	Vote share of current incumbent government in last election	43	0.27	0.82	0.48	0.13
Last Turnout	Turnout in last election	43	0.41	0.91	0.69	0.13

Note: Unless otherwise noted, all data come from CSES (2003; 2007; 2013; 2014).

Table C.1 – Descriptive Statistics for Main Analysis

Variable	Operationalization	N	Min	Max	Mean	SD
Subj. Econ. Insecurity	"Using this card, please tell me how likely it is that during the next 12 months you will be unemployed and looking for work for at least four consecutive weeks?" (4-point scale)	13909	1.00	5.00	1.94	0.94
Unempl. Perceptions	"Of every 100 people of working age in [country] how many would you say are unemployed and looking for work?" (11-point scale)	13909	1.00	11.00	4.36	2.81
Acc. Perceptions	Reversed absolute difference between above variable and actual unemployment rate. Unemployment data from World Bank (2012). Higher values mean more accuracy.	13909	0.00	10.00	2.40	2.51
Age	Age in years	13909	-28.81	80.65	0.28	12.02
Female?	Gender indicator	13909	-0.58	0.80	-0.00	0.50
Education	Education in years	13909	-14.26	20.68	0.17	3.53
Income	Income decile of household	13909	-7.37	6.03	0.10	2.30
Left-Right Ideology	11-point scale	13909	-8.37	6.03	-1.25	2.31
Union Member?	Union indicator	13909	-0.88	0.90	0.02	0.45
Political Interest	Unemployed indicator	13909	-6.13	6.40	0.14	2.15
OUR	Occupational unemployment rate (OUR) (Rehm, 2009, 2011b).	13909	-11.05	22.06	-0.12	4.34
OUR Δ	Change in OUR from previous year (Rehm, 2009, 2011b).	13909	-5.16	8.29	-0.03	1.41

Note: Unless otherwise noted, all data come from ESS 2008.

Table C.2 – Descriptive Statistics for Auxiliary Analysis