Three Essays on Economic Crises, Inequality, and Political Behavior

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The President:

Prof. Dr. Thomas Bieger

In loving memory of my grandfather Gion.

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Lausanne, May 2017
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Zusammenfassung

Die akademische Literatur liefert zahlreiche Beispiele dafür, wie Konflikte bezüglich der Verteilung von Ressourcen Wahlen, politische Präferenzen sowie Handeln und somit auch das politische Alltagsgeschehen beeinflussen. Die vorliegende Dissertation untersucht solche Prozesse und illustriert anhand dreier Beispiele wie bestehende theoretische Modelle und neuere Ideen auf die Analyse politischen Verhaltens angewendet werden können. Das erste Kapitel zeigt mit Daten aus vielen Mitgliedsländern der Europäischen Union, dass schlechte ökonomische Performanz und insbesondere steigende Arbeitslosigkeit negativ mit Vertrauen in politische Institutionen korreliert. Dies basiert auf der Idee, dass mit der Wirtschaft unzufriedene Bürgerinnen und Bürger den Institutionen das Vertrauen entziehen. Weitere, tiefer gehende Analysen anhand des Beispiels von Spanien zeigen, dass ein massiver Einbruch der Wirtschaft das Vertrauen in repräsentative politische Institutionen stark unterminiert.

Das zweite Kapitel analysiert, wie der Effekt von Politikreformen auf die Einkommen verschiedener Gruppen deren Akzeptanz in der Bevölkerung beeinflusst. Anhand eines neuen, repräsentativen Conjoint-Experiments in den USA wird gezeigt, dass sehr wichtig ist, wie Reformen das eigene Einkommen beeinflussen, Bürgerinnen und Bürger aber durchaus auch darauf achten, wie sich das Durchschnittseinkommen aufgrund einer Reform ändern würde. Weitere Analysen legen nahe, dass dies wohl damit zu tun hat, dass die amerikanische Bevölkerung auch das Wohl der Armen in die Entscheidung mitbeinbezieht, ob sie eine vorgeschlagene Reform gutheisst oder nicht. Das letzte Kapitel geht der Frage auf den Grund, ob und was Individuen gegen Ungleichheit unternehmen. Daten neuer, repräsentativer Experimente in den USA und Deutschland, welche die Ungleichheit zwischen zwei Personen variieren, zeigen, dass Individuen Ungleichheit nicht komplett ausmerzen. Die aus diesen Ungleichheitsszenarien gewonnen Daten werden dann verwendet um Individuen zu klassifizieren. Diese Typologie hilft dabei vorherzusagen, welche Individuen Politikmassnahmen zur Verminderung von Ungleichheit unterstützen.

Diese Dissertation trägt somit zur wissenschaftlichen Literatur der politischen Verhaltensforschung bei und illustriert die Wichtigkeit ökonomischer Faktoren für die politische Willensbildung und Einstellungen.

Summary

The academic literature offers various examples of how conflict over the distribution of resources influences elections, political preferences, and mass political action and as a consequence also everyday politics. The present dissertation explores such processes and illustrates with three examples how established theoretical models and newer ideas can be used to analyze political behavior. The first chapter uses data from many European Union member countries and shows that bad economic performance and especially rising unemployment correlates with lower levels of trust in political institutions. The theoretical model suggests that citizens, who are disappointed with the economy, reduce trust in political institutions. Further, more thorough analyses using the example of Spain show that a massive economic downturn heavily undermines the trustworthiness of representative political institutions.

The second chapter analyzes how the income effects of policy reforms influence support for reform in the population. Data from a novel conjoint experiment in the United States shows that it matters how reforms influence one's own income. However, citizens also take into account how such reforms affect the average income. Further analyses suggest that this effect likely stems from American citizen's concern for how policy reforms influence the welfare of the poorest. The last contribution explores the question of what individuals do if they face inequality. Using data from a novel representative survey in the United States and Germany, which varies the randomly assigned inequality between two individuals, shows that they only incompletely equalize payoffs. We classify subjects based on their behavioral responses to inequality and find that the resulting typology helps predict which individuals support real-world policy interventions such as taxing the rich and welfare transfers to the poor.

This dissertation thus contributes to the academic literature on political behavior and illustrates the importance of economic factors for political decision-making processes and public opinion.

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Chapter 1

Introduction

In 2017, citizens in various important European countries such as Germany, France, and the Netherlands will go to the voting booth and cast their vote to elect the politicians who will govern their country for the years to come. These newly elected governments will undoubtedly face serious challenges as some countries in the European Union are still coping with the aftermath of the Great Recession, which was the worst economic downturn after World War II. This crisis has brought countries to the brink of a sovereign default, banks had to file for bankruptcy or be bailed out by governments, and countless people were evicted from their homes as they were not able to make their mortgage payments anymore.

According to estimates of the International Labour Office (2012), nearly 27 million people lost their jobs from 2007 to 2011 due to the crisis, which corresponds to an unprecedented growth in global unemployment. The economic woes, however, also left their mark on the political sphere as governments were voted out of office (Colomer 2012), elections produced fundamentally altered party systems (Teperoglou and Tsatsanis 2014), and citizens voiced their opinion in mass protests criticizing the increasing economic inequality and economic policies (Castañeda 2012).

These examples suggest that conflict over the distribution of resources plays an important role in politics and influences election outcomes, political preferences, and mass political action. Studying the effects of different distributional patterns

thus adds to our understanding of important phenomena we observe in day-to-day politics. The present paper-based dissertation contributes to this by analyzing to what extent established models and more recent theoretical ideas help explain political behavior. In doing so, it focuses on models that have arguments related to the distribution of economic policy benefits at their core.

In studying the determinants of political behavior, scholars have assigned special importance to the nexus between economics and politics, as a rich literature in political science highlights, which uses economic factors to explain political preferences. One of the most prominent of these strands of scholarly work argues that macroeconomic performance influences election outcomes. A prolific literature studying vote and popularity functions contends that incumbents, which provide citizens with economic benefits, have a higher probability of getting reelected and enjoy higher popularity (Kinder and Kiewiet 1981; Lewis-Beck and Stegmaier 2000; Nannestad and Paldam 1994). Academics have studied this link with data from many different countries, which are established as well as transitional democracies, and the economic vote proved to be quite robust (Lewis-Beck and Stegmaier 2013).

Analyzing the influence of economic considerations on various outcome variables in more detail, scholars have also discovered that their explanatory power extends beyond elections and applied them successfully to the analysis of political preferences more generally. Research shows that the economic consequences of political measures influence individuals' attitudes towards policies as diverse as trade (Scheve and Slaughter 2001b), monetary (Bearce and Tuxhorn 2017), immigration (Malhotra, Margalit, and Mo 2013), and welfare policy (Margalit 2013). In addition to this influence of economic factors on public opinion, decisions about the distribution of economic benefits may also trigger mass political action. As a reaction to the announcement and adoption of austerity measures and to voice general discontent with the steps governments had taken, individuals in many cities participated in large-scale protests (Kriesi 2012; van Gent, Mamadouh, and van der Wusten 2013). To understand political behavior, it is thus imperative to study how different economic considerations influence individuals in their preference formation processes.

While scholarship seems to agree that economic consequences matter, there is still an ongoing debate about the mechanism that relates economic considerations

to political behavior. Do individuals' attitudes towards politicians and policies reflect how they influence their personal economic well-being and thus follow the so-called 'pocketbook' logic (Curtis 2014; Sears and Funk 1991)? Or, rather, do voters care about larger societal groups such as the nation as a whole and hence use for instance macroeconomic indicators as an informational cue for how society is doing to form policy preferences that reflect the so-called sociotropic model (Kinder and Kiewiet 1981; Mansfield and Mutz 2009)? The latter explanation has garnered increasing interest in political science as scholars apply ideas and findings from behavioral economics (Bolton and Ockenfels 2000; Fehr and Fischbacher 2003; Fehr and Schmidt 1999), which enables them to paint a finer-grained picture of such other-regarding preferences as results suggest that social norms, such as altruism and inequity aversion, influence political behavior (Bechtel, Hainmueller, and Margalit 2014; Fowler and Kam 2007; Lü, Scheve, and Slaughter 2012; Rueda 2014).

Building on this theoretical foundation, the present dissertation explores how different economic considerations, i.e. macroeconomic outcomes, others' as well as personal well-being, affect political behavior. To this end, the dissertation not only uses, among other theoretical ideas, the well-established egoistic and sociotropic accounts of political behavior but also incorporates more recent approaches, which emphasize different varieties of other-regarding preferences. It thus aims to explore the importance of pocketbook and sociotropic concerns for individual political behavior as well as learning more about the foundations of the latter. Hence, the three contributions in this thesis all use explanations based on economic considerations and distributions of economic goods to study political behavior, thereby learning more about the mechanism that helps explain political preferences.

Chapter 2 analyzes the economic fundamentals of trust in political institutions. Confidence in political institutions contributes to the long-term stability of democratic systems and lower levels of trust may thus undermine democracy. Building on previous literature, I argue that citizens expect policymaking institutions to deliver good economic performance. Representative institutions such as the government, the parliament, and the political parties have various policy instruments at their disposal that they can use to satisfy citizens' demands. I thus expect that recessions disappoint citizens, which in turn punish policymak-

ing institutions by reducing trust in them. However, recessions alone do not hurt citizens, rather, the symptoms that come with them such as fluctuations in the inflation rate and increasing unemployment do. Hence, both those factors have the potential to influence political trust. To explore the economic fundamentals of political trust, I use Eurobarometer data from a large set of European Union member states and find that confidence in policymaking institutions is lower during recessions. The results suggest that this effect likely stems from concerns about the situation in the labor market because the unemployment rate has a consistent statistically significant negative effect on political trust. To subject this finding to another test, I use the synthetic control method to explore how the recent massive economic crisis that Spain experienced influenced confidence. I find that the downturn nearly halved Spaniards' level of trust in the government, the parliament, and the political parties. Additional explorations using public opinion data suggest that Spaniards worried to a great extent about unemployment as it was often mentioned as the most important problem the country faced during the recent crisis. To address the rival explanation of corruption, which may have played a role as some members of the Spanish government and prominent political parties were involved in corruption scandals that made headlines during the crisis, I explore to what extent this may have mattered. The results show, however, that unemployment was perceived as a far more important problem by the Spanish citizenry, thus supporting the idea that the decline in trust in Spanish political institutions was caused by the deteriorating labor market. While this article shows that macroeconomic phenomena influence political behavior, which is compatible with the idea of sociotropism, it leaves open the questions as to why this may be the case.

To address this question and contribute to the debate about which considerations underlie political preferences, Chapter 3, co-authored with Michael M. Bechtel, explores the relative importance of egocentric and sociotropic accounts of preference formation and analyzes in more detail what may explain sociotropic policy preferences. Previous literature mostly used observational data and has thus not been able to assess the relative importance of egocentric as opposed to sociotropic considerations. Furthermore, these data also did not allow to explore whether sociotropic preferences have genuinely other-regarding roots or are based on egoistic considerations about the positive impact of good macroeconomic per-

formance for future personal economic well-being. To contribute to this debate, we fielded a survey among US citizens eligible to vote and implemented a fully randomized conjoint experiment in which we detail how a policy reform influences the average and personal income as well as the income of the poor, the middle class, and the rich. The results suggest that both egoistic as well as sociotropic considerations influence individuals' support for policy reforms. However, the findings show that the magnitude of these effects differ: The effect of personal income changes is two to three times stronger than that of national average income changes. Further analyses suggest that sociotropic considerations most likely mirror concern for the poor. How a policy influences the incomes of the middle class and the rich hardly affects individuals' attitudes towards policy reform while income losses for the poor decrease support for policy, regardless of the personal income effects of a policy. Interestingly, individuals seem to exhibit loss aversion not for themselves but rather with respect to how policies influence the society at large. In addition to showing that egocentric and sociotropic accounts of preference formation matter, these results also suggest that social norms influence individual policy preferences and using these ideas consequently contributes to the literature as these offer a potential explanation as to why individuals care about the macroeconomy.

Social norms-based explanations thus seem to improve our understanding of individual political behavior. To learn more about how prevalent certain norms are in society and whether they help explain political preferences, Chapter 4, co-authored with Michael M. Bechtel and Kenneth F. Scheve, analyzes what individuals do when they are confronted with inequality. Redistribution constitutes a common way to alleviate inequality and thus a means to address a prominent challenge of modern-day politics. However, despite the possibility to implement redistributive measures, inequality has risen. We field an online survey containing an experiment in the US and in Germany in which we randomly assign different types of (in-)equality. We inform respondents that we would raffle two gift cards among all individuals completing the survey but that the value of those gift cards could vary, just like people's wealth in the real world. The initial value of the respondent's gift card was either higher, the same, or lower than that of another winner. Respondents were then given the possibility to change the value of those two gift cards by either taking away from the other winner, giving to the other

winner, or doing nothing. We find that individuals in the disadvantageous inequality condition take on average about 13% from the other and those in the advantageous inequality condition give about 12%. In addition to this experiment, we also presented respondents with different scenarios of initial gift card values and asked them how much, if at all, they would give or take. We use this information in combination with the data generated in the experiment to estimate individuals' distaste for advantageous and disadvantageous inequality. These inequality aversion parameter estimates allow us to develop a typology of d-inequality (disadvantageous) and a-inequality (advantageous) aversion where we differentiate between equalizers (parameters close to what we would expect if individuals always equalized final payoffs), non-equalizers (parameters close to what we would expect if individuals did nothing), and others. We find similar numbers of equalizers and non-equalizers and that these types are more prevalent in certain socio-demographics. Finally, the results also show that these types help explain support for redistributive measures. The distribution of different types in society may thus improve our understanding of why policymakers have not implemented policy responses to rising inequality in the past decades.

Chapter 2

Political Trust in Times of Economic Crisis

*Roman Liesch**

2.1 Introduction

The importance of trust for the functioning of democracy is widely recognized. Institutions can govern more effectively when citizens have confidence in them, as political trust increases support for policies and facilitates the implementation of policy reforms (Hetherington 1998, 2005; Jacobs and Matthews 2012). Moreover, distrust in political institutions increases the probability of abstaining from elections and casting votes for extreme and populist parties (Denemark and Bowler 2002; Hooghe, Marien, and Pauwels 2011). Despite the importance of political trust for the stability of political systems, much ambiguity still exists with respect to the relationship of economic outcomes and citizens' confidence in representative political institutions.

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Some have begun to explore the more immediate effects of the great recession on trust in selected national and international political institutions (Armingeon and Ceka 2014; Polavieja 2013; Ringlerova 2015). Countries that faced bigger economic shocks also experienced stronger fluctuations in political trust (Armingeon and Guthmann 2014). Research, furthermore, suggests that negative perceptions of the political responsiveness of political institutions, as well as corruption (Torcal 2014), and dissatisfaction with how hospitals and schools are run (Ellinas and Lamprianou 2014), negatively correlate with trust in political institutions. Yet, there is still an ongoing debate, as especially explorations of the economic determinants of institutional trust offer mixed results. A wealth of evidence suggests that subjective evaluations of the economy influence political trust (e.g., Armingeon and Guthmann 2014; Lühiste 2006; Mishler and Rose 2001; Torcal 2014). However, the results for macroeconomic indicators are less clear. Some find that economic downturns lead to lower levels of trust in national and supranational institutions (Kotzian 2011; Stevenson and Wolfers 2011) while others observe no (Hakhverdian and Mayne 2012) or mixed effects that differ across indicators (McAllister 1999; Roth, Nowak-Lehmann D., and Otter 2011; Van Erkel and Van Der Meer 2016).

I contribute to this debate by exploring how recessions influence trust in different representative institutions. Citizens expect institutions that are involved in the policymaking process to produce economic policy benefits and if they fail to do so, disappointed individuals lose trust in those institutions. I first use time-series cross-section (TSCS) analyses to test this claim with data from a large set of European Union member countries. To further explore the question of which economic issues citizens primarily condition their trust judgments on, I examine the importance of inflation and unemployment in this regression framework. Both of these factors influence citizens in their daily lives and thus have the potential to influence trust judgments. I complement these broad and general TSCS results using a case study of Spain that employs a synthetic control design to quantify the effect of the massive economic downturn on trust in political institutions. As Spain experienced the worst economic downturn since the civil war in the 1930s, it constitutes a particularly interesting case for evaluating the political cost of a severe economic crisis.[1] To arbitrate between unemployment and inflation as

[1]Pauly, Christoph. 2015. "Back from the Brink: Spain Emerges as Model for Eu-

potential explanations for the erosion of trust in times of crisis, I analyze data from "most important problems"-questions to assess the relative importance of these two issues.

In extending previous research on the topic (e.g., Roth, Nowak-Lehmann D., and Otter 2011; Van Erkel and Van Der Meer 2016), I use a combination of results from a pooled dataset and a case study to uncover the economic foundations of political trust. This allows me to investigate in greater detail the mechanism that links economic downturns and political trust. This study also contributes to our knowledge about the political costs of economic crises by separately analyzing trust in different political institutions. This more detailed analysis seems all the more relevant because previous work has pointed out that collapsing trust in different types of institutions might mask interesting heterogeneity (Rothstein and Stolle 2008, 443).

The TSCS results show that recessions correlate negatively with political trust. Moreover, this correlation reflects the evolution of unemployment and not inflation, as only the former remains significant throughout different model specifications. The case study results using the synthetic control method and focusing on Spain suggests that the Spanish economic slump reduced the share of people trusting the Spanish government by approximately 21 percentage points. This equals a decrease of about 42% compared to pre-crisis levels. Confidence in the parliament and political parties also declined during the downturn, which implies that economic crises strongly undermine trust in representative institutions more generally. Additional placebo tests show that trust in institutions not involved in the policymaking process remained stable despite the economic woes. Micro-level evidence based on individuals' perceptions of the most important policy problem indicates that the importance of concerns over unemployment exceeded that of other issues, such as inflation and corruption. These results, thus, lend additional support to the argument that poor labor market conditions explain the decline in political trust observed in times of economic crises.

rope." *Spiegel Online*, March 25. Accessed February 12, 2017. `http://www.spiegel.de/international/europe/how-spain-recovered-from-the-economic-crisis-a-1025327.html`.

2.2 Trust in Political Institutions

Rohrschneider and Schmitt-Beck (2002) place trust in institutions near the middle of a scale of regime evaluations, which ranges from constitutional ideals to democratic reality. As such, political trust combines elements from both poles of this scale. First, it "implies a broad confidence that an existing regime is a desirable regime" (Rohrschneider and Schmitt-Beck 2002, 38), i.e., citizens accept the current democratic institutional architecture. Second, it mirrors a summary judgment of a regime's capacity to provide beneficial outcomes to citizens. Drawing on this distinction and building on important previous work on the subject (Levi and Stoker 2000; Miller and Listhaug 1990; van der Meer 2010), I define political trust as a relational concept in which individuals make themselves vulnerable to political institutions that have the capacity to do them harm or betray them. Institutional trust is a summary judgment about whether institutions fulfill citizens' expectations. Therefore, citizens trust institutions to the extent that they produce desired outcomes. If expectations are met, citizens reward political institutions with higher levels of trust. If an institution fails to meet citizens' expectations, they reduce trust.

One may question why political institutions have an incentive to increase or maintain citizens' trust. The rationale seems straightforward in the case of reelection-seeking incumbents because voters with low levels of political trust in the government have a higher probability of voting for challengers (Hetherington 1999). Thus, there exists a close link between a commonly used measure of government support, i.e., vote share, and confidence in this institution.[2] However, political trust also matters in non-election periods because citizens with low levels of confidence in political institutions find it more acceptable to disobey the law (Marien and Hooghe 2011), have a higher probability of committing crimes, such as tax fraud (Scholz and Lubell 1998) and purchasing illegal products (Lindström 2008). More generally, trust serves as a resource that political institutions can use to increase citizens' willingness to support policy change (Trüdinger and Bollow 2011), contribute to the provision of public goods (Hetherington 2005; Rudolph

[2]Easton (1975) argues that support for political authorities expresses itself in trust in institutions. Empirical evidence suggesting that vote choice and trust in the government are correlated are in line with this argument.

and Evans 2005), and grant them more temporal leeway to tackle long-term social problems (Jacobs and Matthews 2012). These findings seem particularly important because recent research suggests that individuals may develop generalized, pro-social preferences that increase contributions to various types of public goods in different behavioral contexts (Bolsen et al. 2014). Overall, different institutions face strong incentives to actively try to accumulate and maintain political trust.

2.2.1 Trust in Representative Institutions

But how can political institutions satisfy citizens' expectations and thus ensure high levels of confidence? Rohrschneider and Schmitt-Beck (2002, 38) argue that citizens' assessment of whether the political actors succeeded in delivering what citizens want constitutes an important element of trust judgments. In attempting to finding an explanation of what individuals demand, a great deal of literature highlights the importance of the economy (Hetherington 1998; Levi and Stoker 2000; Lühiste 2006; Mishler and Rose 2001; Van Erkel and Van Der Meer 2016). Because the government has various policy instruments at its disposal to influence the economy and respond to adverse economic shocks, for instance, by increasing expenditure to boost demand (Alesina et al. 1993; Nordhaus 1975; Persson and Tabellini 2002), citizens expect incumbents to use these instruments to realize economic prosperity. If they succeed, this will increase citizens' confidence in the government. However, if incumbents fail to deliver good economic performance, citizens will reduce the trust they put in the government.

A similar dynamic also characterizes how economic outcomes affect trust in other representative political institutions, such as the parliament (Stevenson and Wolfers 2011; van der Meer and Dekker 2011) and the political parties. Since members of parliaments and governments belong, at least in the vast majority of the cases, to political parties, I argue that citizens condition their trust in political parties on the same factors as trust in the other two institutions. Both, parliaments and parties, have important roles in all stages of the policymaking process and therefore have the means to voice citizens' requests in the public arena and ensure the supply of policy benefits. Consequently, these institutions have a subsidiary duty to improve individuals' economic well-being and represent their interests in the policymaking process. If political institutions fail to fulfill

these expectations, citizens will lose confidence in them. Hence, in the case of a worsening economy, citizens' trust declines owing to disappointed expectations. Therefore, I expect that economic crises not only undermine trust in the government but also erode confidence in the parliament, as well as political parties and thus representative political institutions more generally.

2.2.2 Why does the Economy matter?

This raises the question of how citizens evaluate the economic performance of political institutions. While GDP growth rates may give citizens valuable insights into the general economic situation, I argue that fluctuations in unemployment and inflation, as symptoms of poor economic performance, have a more direct impact on citizens' lives and should therefore be the factors that matter for institutional trust. While increases in both reduce, for instance, reported life satisfaction, the effect of the unemployment rate is stronger (Di Tella, Macculloch, and Oswald 2001).

Evidence suggests that it is not even necessary that individuals experience economic hardship themselves as economic crises also undermine trust among those not affected (Polavieja 2013). Recent research supports this idea suggesting that objective measures of economic performance correlate with institutional trust. The results with respect to unemployment, however, are more consistent than the ones for inflation. While unemployment has a robust negative effect and reaches statistical significance, inflation produces mixed results (Roth, Nowak-Lehmann D., and Otter 2011; Van Erkel and Van Der Meer 2016). Considering the theoretical importance of both factors, I explore the importance of unemployment and inflation as determinants of trust in political institutions as individuals that are not satisfied with the situation blame representative political institutions and lose confidence.

2.3 The Economic Foundations of Trust in Political Institutions

To explore whether economic crises influence trust in political institutions, I use Eurobarometer datasets that cover a large number of European Union member

countries.[3] The dataset covers the period from 1997 to 2013 and I measure the dependent variable as the share of respondents (in %) that expressed *Trust in the Government*, *the Parliament*, and *Political Parties* by answering "tend to trust" to the respective survey questions. From a theoretical point of view, I expect that negative economic outcomes influence confidence in all three institutions in a similar way. However, as institution-specific dynamics may exist, I explore these relationships separately.

In a first step, I use two different versions of a *Recession Indicator* as independent variables to explore whether trust in institutions differs in times of crisis. $Recession_{2\ Quarters}$ equals one if a country was in a recession that had lasted at least two quarters by the time the fieldwork took place and 0 otherwise and $Recession_{4\ Quarters}$ is one for recessions lasting at least four quarters and 0 otherwise. Both indicators are based on objective economic figures and not on stated survey measures, which offers the advantage that I avoid regressing stated preferences on subjective assessments. This helps to address the concern that subjective assessments of the economy are endogenous to vote choice (see, e.g., van der Eijk et al. 2007), which could potentially also bias results of other measures of institutional approval such as trust.

I control for a set of variables that may influence institutional trust. *Election Years* offer politicians a powerful incentive to provide citizens with policy benefits, which may positively influence political trust. To capture possible negative effects of austerity measures on trust in political institutions, I also take into consideration *Core Government Spending* to account for the potential negative effect of austerity measures on institutional confidence.[4] Finally, year and country fixed effects account for time-invariant effects, as well as a common trend. I estimate linear regression models and cluster the standard errors by country.

2.3.1 The Detrimental Effect of Recessions

Table 2.1 presents the results for all three independent variables differentiating between models that use the two different recession indicators. Both recession

[3] See Appendix 2.A for more information about the data.

[4] Following the example of Furth (2014), I construct a measure that deducts transfer payments of all kinds from general government spending so that the final measure is not influenced by social transfers of any kind, which tend to increase in crisis times.

indicators have negative signs and reach statistical significance in all regression models, thus suggesting that economic downturns undermine trust in political institutions. During a recession that lasts at least two quarters, trust in the government and the parliament decreases by more than 7 percentage points while the results suggest that such downturns reduces trust in the political parties by about 3.8 percentage points.

The corresponding figures for recessions that last at least four consecutive quarters are larger in magnitude. During long recessions, trust in the government and the parliament decreases by approximately 12 percentage points and more than 13 percentage points, respectively, while the decrease in trust in the parties amounts to about 4.9 percentage points. Overall, these results thus suggest a reduction of institutional trust in times of economic crisis. Citizens reduce the confidence they put in policymaking institutions as a response to bad economic performance. As the differences in the magnitudes of the coefficients suggest, however, this happens to a different extent. Recessions seem to hurt the trustworthiness of the government and the parliament more strongly.

Table 2.1: The Negative Effect of Recessions on Trust in Political Institutions

Dependent Variable: Trust in the...	Government		Parliament		Political Parties	
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Recession}_{2\ Quarters}$	-7.97**		-7.41**		-3.81*	
	(2.19)		(2.30)		(1.52)	
$\text{Recession}_{4\ Quarters}$		-12.19***		-13.63***		-4.89*
		(3.05)		(3.14)		(2.13)
Election Year	3.11*	2.75*	2.93*	2.55*	1.44*	1.27
	(1.25)	(1.15)	(1.11)	(1.10)	(0.63)	(0.63)
Core Government Spending	0.03	0.03	0.02	0.02	0.01	0.01
	(0.06)	(0.05)	(0.06)	(0.05)	(0.03)	(0.03)
Constant	52.86***	52.88***	55.81***	55.70***	28.04***	28.09***
	(3.19)	(3.17)	(2.84)	(2.73)	(1.36)	(1.34)
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Observations	252	252	265	265	265	265
Adjusted R^2	0.715	0.726	0.778	0.796	0.783	0.783

Note: The table reports OLS-coefficients of regressions of trust in the respective institution (0-100%) on economic, political variables, and country and year fixed effects along with robust standard errors clustered by country (in parentheses). * p <0.05, ** p <0.01, *** p <0.001.

To arbitrate between inflation and unemployment as potential explanations for the erosion of trust in times of crisis, Table 2.2 shows that by themselves, the unemployment rate, as well as inflation correlate with trust in selected representative institutions. Interestingly, inflation has a positive coefficient, suggesting that citizens value higher changes in prices.[5] However, this may stem from the fact that higher inflation cannot only be caused by bad economic stewardship but also by, e.g., increases in money supply, which stimulates economic growth and thus results in price increases (Gärtner 2009). Since citizens value economic prosperity, this would explain the positive relationship. However, once I control for unemployment, inflation does not reach statistical significance anymore. The unemployment rate is significant in all models suggesting that concerns over joblessness influence confidence in political institutions, thus producing results in line with previous research (Roth, Nowak-Lehmann D., and Otter 2011; Van Erkel and Van Der Meer 2016). For instance, if the unemployment rate increases by 1 percentage point, trust in the government decreases by about 2 percentage points.

The results again reveal different effect sizes across institutions. While the coefficients for trust in the government and parliament are similar in size, the respective coefficient for political parties is about half of the other two. These differences highlight the importance of analyzing the outcome variables separately. The indicator variable for election year has the expected sign and reaches statistical significance while core government spending only has a significant effect on trust in the parliament (Model 6) but in the theoretically unexpected direction. However, this significance vanishes once I control for the lagged dependent variable (see Table 2.I1 in the Appendix) and inflation does not reach statistical significance in any of these models. Overall, these results suggest that changes in unemployment as opposed to inflation influence political trust and the negative effect of recessions on trust is likely due to increases in unemployment.

[5]This positive sign, while surprising, is in line with some earlier research (see, e.g., Mishler and Rose 2001; van der Meer and Dekker 2011)

Table 2.2: Unemployment vs. Inflation as Explanation for Changes in Political Trust

Dependent Variable:	Government			Parliament			Political Parties		
Trust in the...	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment Rate	-2.06***		-1.98***	-2.23***		-2.21***	-1.04***		-0.99***
	(0.28)		(0.28)	(0.28)		(0.28)	(0.19)		(0.20)
Inflation Rate		2.44**	0.49		2.29*	0.15		1.28*	0.30
		(0.75)	(0.52)		(0.81)	(0.50)		(0.48)	(0.38)
Election Year	3.05*	2.62*	2.99*	2.79**	2.50*	2.77**	1.38*	1.21*	1.34*
	(1.07)	(1.21)	(1.09)	(0.91)	(1.05)	(0.92)	(0.52)	(0.54)	(0.52)
Core Government	-0.05	0.02	-0.05	-0.06*	0.01	-0.06*	-0.03	0.00	-0.03
Spending	(0.03)	(0.05)	(0.03)	(0.03)	(0.06)	(0.03)	(0.02)	(0.02)	(0.02)
Constant	65.20***	50.03***	64.10***	69.57***	53.26***	69.24***	34.29***	26.59***	33.63***
	(3.31)	(3.31)	(3.16)	(2.74)	(3.12)	(2.78)	(1.87)	(1.48)	(2.05)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	251	252	251	264	265	264	264	265	264
Adjusted R^2	0.789	0.716	0.789	0.854	0.780	0.853	0.828	0.786	0.828

Note: The table reports OLS-coefficients of regressions of trust in the respective institution (0-100%) (Government: Models 1 to 3, Parliament: Models 4 to 6, Political Parties: Models 7 to 9) on economic and political variables as well as county and year fixed effects along with robust standard errors clustered by country (in parentheses). * p <0.5, ** p <0.01, *** p <0.001.

2.4 How Politically Costly is a Massive Economic Downturn?

The results, thus far, suggest that increases in unemployment undermine political trust. To put this finding to an additional test, I conduct a case study of Spain. The fifth largest economy of the EU officially entered recession in the second quarter of 2008 (Ortega and Peñalosa 2012, 7) but even before the downturn started, unemployment began to grow until it reached almost unprecedented levels in 2013.[6] Survey data shows that the population noticed these problems, as the share of Spaniards expressing negative views about the state of the economy increased considerably in 2008 in response to the deteriorating economy (Fraile and Lewis-Beck 2014). To further strengthen my claim that poor labor market conditions undermine political trust, I then explore whether unemployment, inflation, or corruption ranked higher on the list of important problems in Spain to learn more about the mechanism.

To explore the political cost of the Spanish economic crisis, I use the same Eurobarometer waves as in the time-series cross-section analysis above. The dependent variable is the share of respondents (0-1) that expressed *Trust in the Government* (or the respective political institution), i.e., answered with "tend to trust" to the survey question. Moreover, to construct synthetic Spain I choose a set of predictors consisting of sociodemographic variables, as well as objective economic indicators based on theoretical considerations (see Mishler & Rose, 2001 for a discussion). I use the following sociodemographic variables which are part of the Eurobarometer datasets: *Unemployed*, *Male*, *Level of Education* (three categories), and *Age* (seven categories).[7] Furthermore, I use data from Eurostat on the *Yearly Unemployment Rate*, *Yearly Real GDP p.c. in €*, and *Yearly Real GDP p.c. Growth Rates*.[8]

[6] For more background information on the Spanish economic crisis see Appendix 2.B.
[7] Always as the weighted share of individuals per country and wave in the dataset.
[8] See Appendix 2.C for the variable definitions.

2.4.1 Method

The synthetic control method (SCM) aims at constructing a synthetic version of the unit of interest, in my case Spain, to use it as counterfactual (Abadie, Diamond, and Hainmueller 2010, 2015; Abadie and Gardeazabal 2003).[9] The goal is to determine a set of weights to compute a weighted average of the countries in the donor pool that approximates the evolution of trust in the pre-crisis period as closely as possible. Following Abadie, Diamond, and Hainmueller (2015), I select the synthetic control that minimizes the root mean square prediction error (RMSPE), i.e., the difference between the unit of interest and the control unit in the outcome variable in the pre-treatment period. The credibility of the counterfactual critically hinges on the RMSPE because I use this counterfactual to project how trust in the respective institutions would have evolved had Spain not experienced the economic crisis. I estimate the negative effect of the downturn on trust in political institutions as the difference between trust-levels in Spain and its synthetic counterpart following the start of the economic crisis in 2008.

One of the advantages of using the SCM is increased transparency as it provides detailed information on the composition of the synthetic control unit, i.e., the weights with which every unit of the donor pool contributes to the synthetic control group. Additionally, it also presents details about pre-intervention outcomes for the synthetic control unit, as well as the treated unit (Abadie, Diamond, and Hainmueller 2010, 494), which allows for an examination of similarities between these two units. I provide all this information in the Appendix and briefly refer to it in the discussion of the results.

The theoretical framework emphasizes the importance of the economy for trust in political institutions and the recent massive economic downturn in Spain offers a valuable opportunity to test this prediction. To this end, I define this economic crisis as treatment. The recession started in the second quarter of 2008, which precedes the fieldwork period in 2008 and I consequently define 2008 as the first treatment year. The crisis, moreover, lasted until after 2013, the final year in my dataset. Therefore, I consider the years from 2008 to 2013 as the treatment period.

[9] For a more technical discussion see Appendix 2.D.

2.4.2 Donor Pool

To construct the counterfactual, I use a donor pool that contains Austria, Belgium, Denmark, Finland, France, Germany, Luxembourg, Sweden, the Netherlands, and the United Kingdom. I selected these countries for the following reason: Abadie, Diamond, and Hainmueller (2015, 497) point out that "it is important to restrict the donor pool to units with outcomes that are thought to be driven by the same structural process as for the unit representing the case of interest" to ensure that the comparison units are suited to approximate the counterfactual of the unit of interest. Therefore, the countries included in the donor pool are selected based on similarities in the underlying data-generating process with Spain rather than on resemblance in terms of country characteristics. One argument suggests that the same structural process should drive trust in political institutions in Spain and the countries in the donor pool. All these countries joined the European Union prior to the year 1997 and thus before the beginning of the period I examine. If any shocks at the EU-level affected political trust during this period, these were common to all these countries and hence presumably influenced the structural process in Spain and the donor pool countries in a similar way. Moreover, these countries' long history of membership in the EU increases the probability that they have evolved in a similar direction and share certain commonalities and hence that a similar process drives the outcome.

Due to concerns about the similarity of the data-generating process, I exclude several other countries from the donor pool. First, Greece, Ireland, and Portugal experienced similar or even worse economic downturns than Spain and were, hence, presumably subject to a structural shock to the outcome variables of interest. Thus, these countries cannot be used to approximate the counterfactual of Spain without the economic crisis. Moreover, I exclude Italy from the donor pool because of its high levels of corruption (Del Monte and Papagni 2007). Data collected from early January to late March 2014 shows that the frequency of corruption news varies strongly across time (Rizzica and Tonello 2015, 28). Considering research suggesting that corruption correlates negatively with political trust (e.g., van der Meer and Dekker 2011), it seems likely that media coverage of these corruption scandals affected trust in political institutions, and confidence in Italy, therefore, follows an idiosyncratic pattern, which makes this country

unsuitable as comparison unit. Consequently, I drop Italy from the donor pool.[10]

Finally, I also exclude the countries that joined the European Union in 2004 and later from the donor pool. Research suggests that economic performance correlates with political trust in post-communist societies (Mishler and Rose 2001). However, these findings also show that factors such as the perceived fairness of the institutions as compared to the communist regimes and corruption significantly correlate with trust in political institutions. This suggests that the data generating process in those countries differs from Spain and they are, hence, not suited to construct synthetic Spain. This leaves me with a total of ten fellow member states in the donor pool, which like Spain joined the EU long before 2004.

Some may argue that the economic crisis also affected countries in the donor pool. This would imply that, according to the theoretical argument, these countries also experienced declining levels of political trust. Compared to the economic crisis in Spain, however, donor pool countries only experienced brief spells of economic downturns and recovered quickly. Moreover, even if the economic crisis somewhat depressed trust in the government in control countries, this would lead me to underestimate the true effect and thereby result in conservative estimates.

2.4.3 Political Trust in Spain

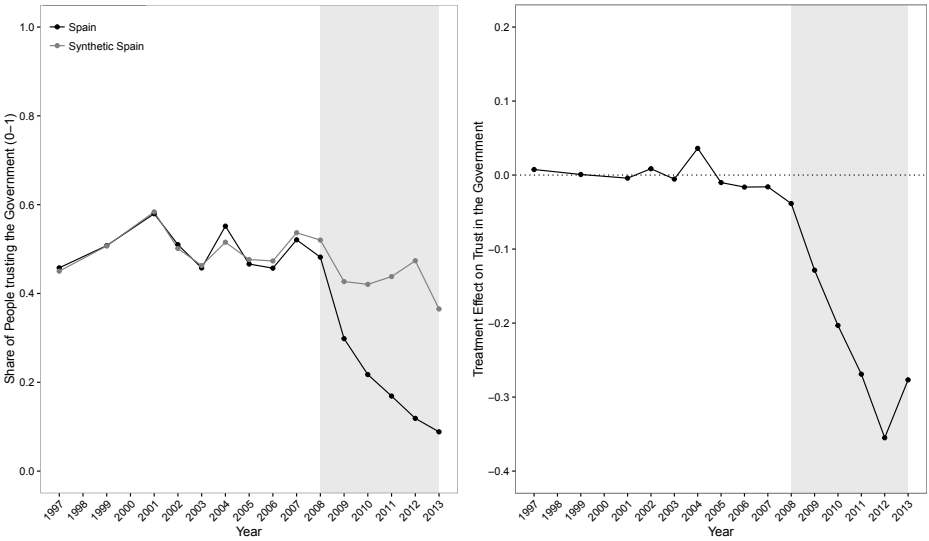
To estimate the negative effect of the economic crisis on trust in the Spanish government, I start by generating the counterfactual evolution of trust in the government in the absence of the crisis.[11] The resulting synthetic Spain is a weighted average of France, Finland, Luxembourg, Denmark, and Sweden with weights decreasing in this order. All other countries in the donor pool receive zero weights.[12] The left panel in Figure 2.1 displays the trajectory of trust in the government in Spain and synthetic Spain for the 1997 to 2013 period and shows

[10] I also conducted the same analyses including Italy in the donor pool. Overall, it yielded similar results. For some outcome variables, Italy received zero weight and thus did not influence the composition of synthetic Spain.

[11] To compute the counterfactual, I used STATA/SE 13.1 and the Synth package described in Abadie, Diamond, and Hainmueller (2015) with the options nested and allopt.

[12] Table 2.I2 shows the pre-treatment averages for synthetic Spain, Spain, and the mean of the donor pool, Table 2.I3 in the Appendix shows these weights for each country in the synthetic version of Spain for the outcome variable trust in the government, and Table 2.I4 shows the variable weights.

strong similarities before 2008. The RMSPE[13], which is only about 0.02, confirms the impression that synthetic Spain reproduces pre-crisis trust for Spain very well during the entire pre-crisis period.



Note: The left-hand panel shows the evolution of trust in the governments of Spain and synthetic Spain (constructed with the synthetic control method) for the period from 1997 to 2013. The right-hand panel plots the difference in the share of people trusting the government between Spain and synthetic Spain. Negative values illustrate the depressing effect of the economic crisis on trust in the Spanish government. The gray-shaded area is the treatment period.

Figure 2.1: The Detrimental Effect of the Economic Crisis on Trust in the Spanish Government

The right-hand panel of Figure 2.1 reports the difference in trust in the government between synthetic and observed Spain, i.e., the negative effect of the crisis. From 2008 onwards, the two lines diverge substantially. The results suggest that the economic crisis reduced trust in the government already in 2008 but that this was only the beginning of a considerable reduction in trust in the government. The difference between Spain and its synthetic counterpart grows at an almost constant pace and peaks in 2012 when the gap reaches more than 35 percentage

[13]The pre-2008 RMSPE for Spain is defined as RMSPE $= (\frac{1}{T_0}\sum_{t=1}^{T_0}(Y_{1,t} - \sum_{c=2}^{C+1} w_c * Y_{c,t})^2)^{\frac{1}{2}}$.

points. In 2013, the difference decreases to slightly below 28 percentage points, which still represents a massive loss of political capital due to the downturn. In comparison with pre-crisis levels of trust, this decrease means that the share of individuals trusting the government was more than halved in 2013. Overall, these results show a pronounced negative effect of the economic crisis on governmental trust in Spain where the difference-in-differences estimate suggest that confidence in the government was on average approximately 21 percentage points lower[14] during the crisis than before.[15] A t-test suggests that this difference is highly significant. Furthermore, in-space placebos support the idea that the Spanish economic crisis exerts a statistically significant effect on trust in the government (see Appendix 2.E for details). The documented decline in trust in the Spanish government qualitatively mirrors the results reported in Armingeon and Ceka (2014, 95) based on a before-after comparison. However, since we do not use the same Eurobarometer wave in 2011, the results are not directly comparable since for instance unemployment continued to grow in the time between those two waves. Finally, because France is assigned the highest weight generating in synthetic Spain, I conduct an additional sensitivity analysis excluding France from the donor pool. As Figure 2.H1 in the Appendix shows, excluding the country with the highest weight hardly changes the estimates which lends further credence to the conclusion that the Spanish economic crisis destroyed much political trust in the government.

The Credibility of the Counterfactual

How credible is the counterfactual used here? Some may argue that the structural process in Spain differs from the one in the countries constituting synthetic Spain, which would in turn cast doubt on the credibility of the results. However, my examinations of the data-generating process in the synthetic Spain-countries, i.e., those that receive non-zero weights, using regressions (see Appendix 2.F) reveal similarities in the determinants of the outcome variable between these two sets of countries. This increases the credibility that the countries in the donor pool

[14] See Table 2.E1 in the Appendix for more information.

[15] A simple before-after comparison using the means of the period 1997-2007 and 2008-2013 yields a minus of about 27 percentage points suggesting that it overestimates the negative impact of the Spanish economic downturn on trust in the government.

are suitable and using the resulting synthetic Spain as comparison units produces reliable estimates. Moreover, a comparison of the pre-crisis means of the predictor variables that were used to construct the counterfactual for Spain, synthetic Spain, and for reasons of comparison also the mean of all donor pool countries suggests that synthetic Spain is better suited as the comparison unit.[16] Synthetic Spain is very similar to real Spain in terms of pre-2008 share of unemployed in the sample, share of over 79 year olds in the sample, and trust in the government in 2004. These similarities are especially relevant because these variables played the most important roles in determining the outcome.[17] This suggests that using the projections of the counterfactual obtained by applying the synthetic control method should yield credible estimates.

Overall, using the synthetic control method suggests that the Spanish government lost much political capital due to the economic downturn. This is consistent with the argument that citizens, who were disappointed when the government failed to successfully cope with the crisis, lost confidence in this central representative institution. The share of citizens trusting the government remained – at least until 2013 – well below what it supposedly would have been had Spain not experienced the massive economic downturn. This strong negative effect qualitatively mirrors the magnitude of the economic crisis.

2.4.4 Negative Effects on Trust in Other Representative Institutions

I now turn to the analysis of trust in other representative institutions. Institutions, such as the parliament and the political parties, represent the citizens and are involved in the policymaking process. Therefore, they carry some responsibility for the policy response to the crisis. Consequently, I expect that the Spanish economic downturn undermined not only trust in the government but also confidence in the parliament, as well as the political parties and therefore trust in representative institutions more generally. To test this claim, I apply the synthetic control method selecting a combination of the predictors presented earlier,

[16] See Table 2.I2 in the Appendix.
[17] See Table 2.I4 in the Appendix for variable weights.

which produces the best pre-crisis fit.[18]

The upper two panels in Figure 2.2 show the results for *Trust in the National Parliament*.[19] The panel on the upper left depicts the pre-crisis fit. Synthetic Spain follows the share of people trusting the Spanish parliament quite well in the pre-crisis period (RMSPE ≈ 0.02), which lends further credence to the results in the top right panel. This panel shows a small difference between Spain and synthetic Spain in the first crisis year – 2008. Afterwards, however, the two lines diverge and the difference peaks in 2012, when the share of people that trusted the Spanish parliament was more than 33 percentage points below what it would have been without the crisis. Similar to trust in the government, the gap between Spain and synthetic Spain narrows a bit in 2013 but remains at a level that equals about half the share of individuals trusting the parliament in the last pre-crisis years. The economic crisis, therefore, reduced trust in the parliament by about 50%. These results, thus, support the idea that citizens also expect the parliament to take measures to address an economic crisis and that a strong downturn influences how citizens perceive the trustworthiness of the national assembly.

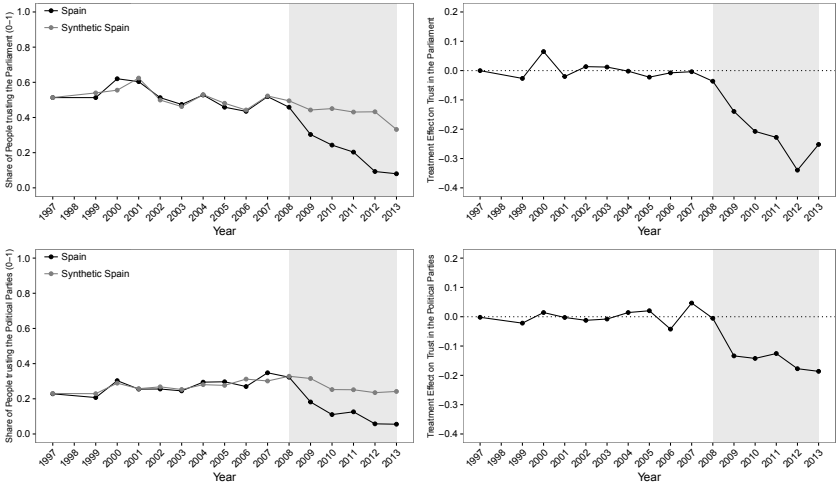
What Happened to Trust in Political Parties?

Did the crisis also affect *Trust in Political Parties*? The panel in the lower left corner of Figure 2.2 shows how confidence in parties evolved in the time before and during the crisis period in Spain and synthetic Spain.[20] Synthetic Spain tracks the observed evolution of trust in the parties well, with some deviations in 2006 and 2007 – the last two pre-crisis years (RMSPE ≈ 0.02). The lower right panel shows that economic hardship caused mistrust in the parties to grow. The results suggest that the downturn reduced the share of people trusting the Spanish parties by about 15 percentage points. This effect exceeds the differences between treated and synthetic Spain in pre-crisis times by a factor of about two. The magnitude of the decline in trust appears substantial because Spaniards already perceived political parties as rather untrustworthy before the onset of the economic crisis.

[18] I tried different combinations and subsets of the predictors listed in Appendix 2.C and chose combinations that yielded very low RMSPEs.

[19] For more information on the composition of synthetic Spain, see Tables 2.I5 and 2.I6 in the Appendix.

[20] For more information about the composition of synthetic Spain see Tables 2.I7 and 2.I8 in the Appendix.



Note: The two upper panels show the results of the synthetic control method applied to trust in the Spanish parliament, the two lower panels show the results applied to trust in the political parties. The left-hand panels show the evolution of trust in the respective institution in Spain and synthetic Spain for the period from 1997 to 2013. The right-hand panels plot the difference in the share of people trusting the parliament and the political parties respectively between Spain and synthetic Spain. Negative values illustrate the depressing effect of the economic crisis on trust in these two institutions. The gray-shaded area is the treatment period.

Figure 2.2: Negative Effect of the Crisis on Trust in the Parliament and Political Parties

Consequently, the loss in trust amounts to more than 50% over the pre-crisis average.

Overall, I find some similarities between how the Spanish economic crisis reduced trust in the national government, the parliament, and the political parties. Trust in all three representative institutions was considerably below what it would have been without the crisis. While the downturn caused trust in the government and parliament to deteriorate at an almost constant pace until 2012 and rebounded a bit in 2013, trust in the parties declined sharply from 2008 to 2009 and then remained at an almost constant level below what it would have been had the crisis not occurred. Despite these differences, these results corroborate the idea that the Spanish economic downturn eroded trust in representative institutions more generally and that citizens punish political institutions, which fulfill representative tasks directly related to the policymaking process, for an economic downturn.

Placebo tests (see Appendix 2.G) suggest that the Spanish economic woes did not affect trust in political institutions that are not involved in the policymaking process, such as the army and the legal system. On the one hand, this shows that Spain did not experience a secular decline in trust. On the other hand, this further increases our confidence in the main results, as it supports the idea that citizens reduce their trust in institutions in response to the perceived failure of these institutions to deliver what they want. Neither the legal system nor the army have a say in economic affairs and a decline in trust in these two institutions would have run counter to the theoretical argument. Thus, the findings of these additional outcomes increase the probability that the main results isolate an effect that only pertains to trust in policymaking institutions.

2.4.5 Employment, Inflation, or Corruption: Which Mechanism Explains the Decline in Trust?

To what extent does this effect mirror economic concerns, as opposed to other potentially relevant issues? The time-series cross-section results suggest that unemployment plays a more important role in influencing trust in political institutions than inflation. To further explore the mechanism I rely on survey data from different sources. The available survey evidence suggests that Spaniards blamed

the government for the economic crisis and the measures it took to cope with it. A snap poll conducted in 2008 shows that almost two-thirds of the Spaniards considered the measures the Spanish government adopted in response to the crisis to be insufficient.[21] Another survey, conducted by the Pew Research Center in the spring of 2010, 2011, and 2012,[22] shows that of the individuals that were not satisfied with the state of the economy, the share of people holding the government responsible for the economic problems was well above 50%.[23] People only accused actors, such as banks and financial institutions more often than the government, which suggests that individuals perceived these actors as the main culprits of the crisis. Most importantly, more people believed that the Spanish government was responsible for the current economic problems than the European Union. In line with the theoretical argument, this suggests that economic issues are key for our understanding of how the crisis has affected trust in representative political institutions.

Inflation vs. Unemployment

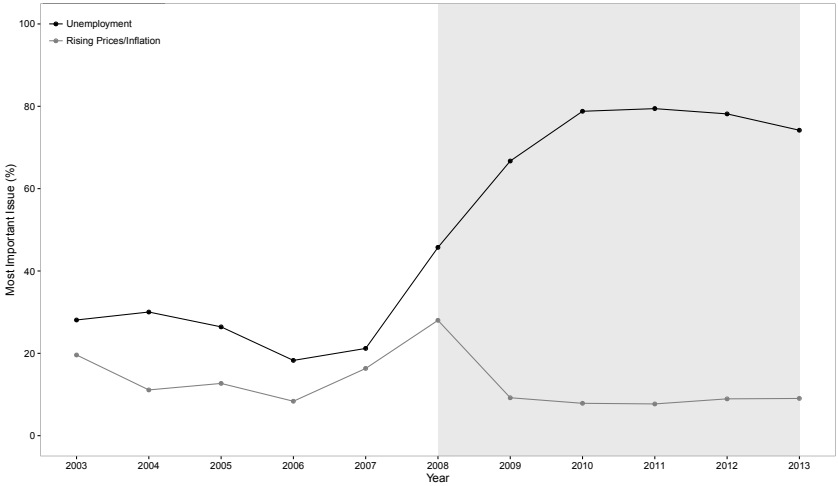
To more directly arbitrate between inflation and unemployment as two potential explanations, I explore the temporal dynamics in the relative importance of unemployment and inflation-related concerns in greater detail. During its economic crisis, Spain experienced a massive growth in unemployment (see Figure 2.B1 in the Appendix), as well as considerable fluctuations in the inflation rate (see Figure 2.B2 in the Appendix), which thus both could have influenced trust in political institutions. Figure 2.3 examines the relative importance of unemployment and inflation to Spanish interviewees using Eurobarometer data. For each survey from 2003 to 2013, I plot the fraction of respondents who perceived unemployment or rising prices/inflation to be among the two "most important issues facing Spain at the moment".[24] The gray-shaded area indicates the crisis period.

[21] Kern, Soeren. 2008. "Spain's Economy: It's A Crisis, Stupid!" *The Brussels Journal*, July 06. Accessed February 6, 2017. `https://www.brusselsjournal.com/node/3392`.

[22] Unfortunately, these questions were not asked in earlier surveys.

[23] The exact shares of people that stated the government was first or second to blame for the country's problems were 67, 65, and 60%, respectively. Based on calculations using data from Pew Research Center (2016a) and Pew Research Center (2016b).

[24] This question was asked on a regular basis from the 2003 wave I use onwards only and hence I have data of a total of eleven waves. Interviewers presented respondents with a list of issues and they could choose up to two of these, but also had the possibility to spontaneously



Note: This figure displays the share of Spaniards mentioning unemployment or rising prices/inflation as the most important issues Spain faced at the moment of the interview, respectively. Respondents can give up to two answers and totals thus may exceed 100%. The gray-shaded area represents the period in which the Spanish economy experienced the crisis. Based on Eurobarometer polling data from 2003 to 2013 since this question was only included in those waves I use.

Figure 2.3: Perceptions of Economic Concerns as Most Important Issue in Spain

The results suggest that unemployment dominated the perception of problems of Spanish citizens from 2008 onwards. While unemployment was more important to respondents than inflation throughout the whole period, it started to rapidly gain prominence in 2008, the first crisis year, when about 45% of the respondents in Spain perceived unemployment to be among the two most important current issues. In the same year, about 28% of the respondents reported that inflation belongs to the two most important issues. From the second crisis year onwards, however, the relative importance of these two issues evolved completely differently. The importance of inflation fell in the second crisis year and remained at pre-crisis lows throughout the rest of the crisis. The relative importance of unemployment, however, continued to grow until 2010 when about four out of five respondents perceived it to be among the two most important issues and it remained at roughly the same level. The relative importance of unemployment as the dominating issue is consistent with the idea that unemployment and not some other symptom of the crisis caused the decrease in trust in representative institutions.

The Issue of Corruption

In the Spanish case, a rival explanation could be that corruption was responsible for the strong decline in political trust (cf. Torcal 2014) because the country experienced several corruption scandals that made headlines during the economic crisis.[25] Plausibly, involvement in corruption also has the potential to disappoint citizens' normative expectations toward the government (Morris and Klesner 2010). Since the scandals involved prominent politicians, as well as the incumbent party, one may hypothesize that corruption perceptions contributed to the drop in trust in the policymaking institutions.

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mention others in some waves. Moreover, from the 2010 wave onwards, two lists were prepared and each was presented to one half of the sample. The two economic issues I analyze here were the same in both lists. Therefore, conditional on the assumption of the independence of irrelevant alternatives (IIA), these changes should remain inconsequential for the results.

[25] For example, the opposition party was hit by a massive scandal that surfaced in 2009 (Tremlett, Giles. 2009. "Spanish Opposition Party Rocked by Corruption Scandal." *The Guardian*, March 06. Accessed February 06, 2017. `https://www.theguardian.com/world/2009/mar/06/spain-opposition-corruption-scandal`.) and Prime Minister Mariano Rajoy's party was involved in a scandal that made headlines in 2013 ("Spain's Mariano Rajoy Corruption Scandal." 2013. *The Telegraph*, August 01. Accessed February 06, 2017, `http://www.telegraph.co.uk/news/worldnews/europe/spain/10215539/Spains-Mariano-Rajoy-corruption-scandal-I-made-a-mistake.html`).

Figure 2.4 shows that Spaniards did not perceive corruption to constitute a major problem in their country until 2009.[26] And although the time series exhibits two spikes that reflect two corruption scandals, which occurred at the end of 2009 and in 2010, the relative importance of corruption and fraud declined very quickly afterwards. This suggests that corruption scandals seem to have had relatively short-lived effects on trust. For most of the time, the share of Spanish citizens considering unemployment to be among the most important problems was at least ten times as high as the share of individuals concerned about corruption. Only in the last year I examine, the share of respondents that perceived corruption and fraud to be among the three most important problems increased. This, again, coincides with a corruption scandal that allegedly involved the governing party and the prime minister.[27] Yet, even in the presence of such a major corruption scandal, the relative importance of unemployment remained at least twice as high.[28] In combination with Mishler and Rose's (2001, 52) conclusion that "aggregate corruption does corrode political trust, but only to the extent that individuals perceive corruption and ascribe it importance relative to other concerns," this finding corroborates the idea that the crisis effect on trust can largely be attributed to unemployment concerns.

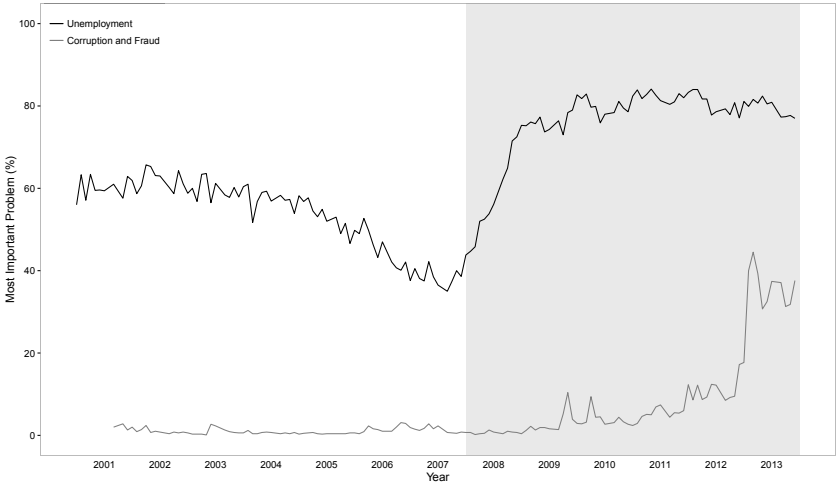
2.5 Conclusion

How does trust in different political institutions respond to economic crises? Improving our knowledge about this topic is important because political trust adds to the long-term stability of democratic systems. I argue that economic hardship erodes confidence in representative political institutions that deal with policy-making as a consequence of dissatisfaction with labor market conditions. I use a

[26] This data comes from the Spanish "Centro de Investigaciones Sociológicas", which carries out surveys containing questions about which three problems respondents consider to be the most important ones on an almost monthly basis. Data taken from Centro des Investigaciones Sociológicas (2015).

[27] Ortiz, Fiona and Iciar Reinlein. 2013. "Spain Corruption Scandal Turns Up Heat." *Reuters*, January 31. Accessed February 06, 2017. `http://www.reuters.com/article/us-spain-corruption-idUSBRE90U0DD20130131`.

[28] Figure 2.H2 in the Appendix using data from the World Bank's Worldwide Governance Indicator "Control of Corruption" furthermore suggests that problems with corruption hardly worsened during the period under consideration, as the mean of underlying indicators only shows a slight downward trend.



Note: This figure displays the share of Spaniards mentioning unemployment and corruption as the most important problems Spain faced at the moment of the interview. Respondents were allowed to mention up to three issues. The gray-shaded area represents the period in which the Spanish economy experienced the crisis. Based on Centro de Investigaciones Sociológicas polling data.

Figure 2.4: Relative Importance of Issues: Unemployment Versus Corruption and Fraud

time-series cross-section analysis of the evolution of trust in a large set of European Union countries from 1997 to 2013 to explore how recessions influence trust in political institutions. The results suggest that economic downturns negatively correlate with trust in representative institutions. Additional analyses, furthermore, indicate that this negative effect most likely stems from problems in the labor market. To evaluate the theoretical argument in a setting that allows for a more causal interpretation of the results, I use the synthetic control method to perform a case study of Spain, which experienced an economic crisis with unprecedented levels of joblessness. The results suggest that the downturn decreased trust in the Spanish government by about 21 percentage points compared to the pre-crisis period. I also find that disappointment with the incumbents' economic performance eroded trust in other representative institutions and lowered confidence in the national parliament, as well as political parties. Overall, the Spanish economic crisis had a devastating effect on confidence in the national government, parliament, and political parties. The share of citizens trusting those institutions declined up to half of the pre-crisis levels. This effect largely seems to mirror individuals' unemployment concerns and not inflation aversion or perceptions of corruption. Consistent with the theory, placebo studies indicate that trust in the legal system and the army remained rather stable during the crisis. Overall, these findings quantify how severely economic crises undermine trust in political institutions and reveal similar patterns of how downturns influence trust in the government, as well as the national parliament. Moreover, the additional analyses increase the plausibility of the claim that the observed decline in political trust stems from unemployment concerns.

These findings add to our knowledge about the effects of economic crises on public opinion and carry implications for our understanding of democratic accountability and the study of political institutions. The strong negative effect of the economic crisis on trust in the government that I document corroborates theories about the economic origins of political trust (Levi and Stoker 2000; Mishler and Rose 2001) and supports the idea that aggregate unemployment influences a multitude of outcomes (Di Tella, Macculloch, and Oswald 2001; Roth, Nowak-Lehmann D., and Otter 2011; Van Erkel and Van Der Meer 2016). However, my results also support the idea that the negative effects of an economic downturn are far-reaching, as economic hardship seems to undermine trust in representa-

tive institutions more generally, at least when one considers a massive economic crisis. Keeping this in mind, the effects estimated in the case study most likely represent an upper bound of how sizable the impact of an economic crisis can be considering the magnitude of the downturn. Overall, the results suggest that adverse economic outcomes have multi-faceted consequences on trust in a political system.

The results also speak to the literature on the empirical study of trust in political institutions. I explicitly analyze trust in several political institutions separately and while trust in all representative institutions seems to respond to unemployment, I uncover differential effects of economic crises on trust depending on the type of institution. While the extant literature often studies trust in highly aggregate form by analyzing additive indices of trust in political institutions (e.g., Catterberg and Moreno (2006) use an index combining confidence in the parliament and the civil service) my results suggest that this masks potentially important and interesting heterogeneity. In this sense, the results lend empirical support to Rothstein and Stolle's (2008) critique of the collapse of various forms of institutional trust. Because the response of trust in political institutions to economic hardship varies considerably between institutions and only seems to affect representative institutions, one may question whether political trust is truly one-dimensional (Marien and Hooghe 2011). This suggests that future work may gain deeper insight into the sources of trust by analyzing different types of institutions separately.

From a policy perspective, the result that severe economic crises undermine trust in representative institutions more generally tells a daunting tale. In some European countries, citizens continue to experience economic hardship and high unemployment on a daily basis. My findings and the accumulated evidence about the negative effects of distrust in political institutions nurtures skepticism about how quickly countries will manage to recover both economically and politically in the years to come. However, this also offers possible avenues for further research. The recent economic downturn has presumably shattered much political capital and this provides an opportunity to further explore the importance of political trust for democracy, e.g., by examining the behavioral consequences of high levels of distrust in political institutions on turnout, demonstrations, violent protest, and other types of political activism.

Finally, I would like to discuss the main limitations of my study. First, the case study only considers the negative effect of an exceptional economic crisis. This leaves open the question of whether rewards for extraordinarily positive economic performance would be of a similar magnitude and whether this would also affect trust in representative institutions more generally. Second, previous research suggests a partisan bias in blame attribution (Healy, Kuo, and Malhotra 2014; Malhotra and Margalit 2010; Tilley and Hobolt 2011). Because the data I use only rarely contains information about individuals' partisan attachments, I cannot explore the existence of a partisan bias in the erosion of political trust. However, even if such a partisan bias exists, it seems unlikely to undermine the credibility of the case study findings, especially with respect to trust in the government. First, government partisanship in Spain remained virtually constant from 2004 to 2011 and hence, partisan attachments will presumably not have played a major role in explaining the long-term decline in trust I document. My analyses suggest that unemployment concerns eroded representative institutions' trustworthiness and those concerns increased sharply in 2008, despite stability in government partisanship and small overall changes in the composition of the parliament in that period. This suggests that the severity of the crisis prevented partisanship from clouding citizens' assessment of the economy. Second, even in 2011, when the party in power changed, unemployment concerns remained at a very high level. This further supports the idea that citizens were generally disappointed with the government and the parliament as institutions that failed to effectively address increasing joblessness. Thus, the partisan bias seems unlikely to play a major role in explaining the institution-specific effects of the crisis documented here.

Appendix 2.A: Additional Information about the Data used in the Time-Series Cross-Section Analysis

The analyses primarily use data from Eurobarometer surveys, which are fielded in regular intervals in all European Union member countries. I use data from 1997 to 2013 and the full dataset contains one wave per year with the exception of 1998, where no Eurobarometer survey contained the trust questions I use as dependent variables.[29] I use the fall/winter waves of the Eurobarometer which were roughly fielded in the months September to November, depending on the year in which the survey was fielded.[30] As data for the independent variables is missing in some surveys (questionnaires differed between waves), some years are missing in the analysis. For instance, in 2000 interviewers did not ask respondents about trust in the government. Data is weighted using the post-stratification weights provided along with the datasets so as to have samples that correspond to the real population (GESIS - Leibniz-Institut für Sozialwissenschaften 2014).
Variable definitions:

- *Trust in the Government/Parliament/Political Parties:* Share (in %) of respondents that expressed trust in the respective institution by answering "tend to trust" to the survey question "I would like to ask you a question about how much trust you have in certain institutions. For each of the following institutions, please tell me if you tend to trust it or tend not to trust it." Respondents that answered "Don't know" set to missing.

- $Recession_{2\ Quarters}$*:* Equals 1 for recessions lasting at least two quarters. If a country experienced at least two consecutive quarters with negative growth

[29] I use data from a total of 16 waves. These are: Eurobarometer 48.0 (European Commission 2012a), 51.0 (European Commission 2012b), 54.1 (European Commission 2012c), 56.2 (European Commission 2012d), 57.1 (European Commission 2012e), 60.1 (European Commission 2012f), 62.0 (European Commission 2012g), 64.2 (European Commission 2012h), 66.1 (European Commission 2012i), 68.1 (European Commission 2012j), 70.1 (European Commission 2012k), 72.4 (European Commission 2012l), 74.2 (European Commission 2013a), 76.3 (European Commission and European Parliament, Brussels 2012), 78.1 (European Commission 2013b), and 80.1 (European Commission 2014).

[30] The only two exceptions are the years 1999 and 2002, where they only asked all the trust questions I use in the spring surveys for which data was collected from March to May.

rates, this recession indicator is 1 for all quarters and else 0. If a country experienced a recession during any of the quarters in which interviews for the Eurobarometer were conduced (some waves spread over multiple quarters), this country is coded as being in recession. Data on quarterly GDP growth rates over previous period (seasonally and calender adjusted) taken from Eurostat (2017a).

- *$Recession_{4\ Quarters}$:* Equals 1 for recessions lasting at least four quarters. If a country experienced at least four consecutive quarters with negative growth rates, this recession indicator is 1 for all quarters and else 0. If a country experienced a recession during any of the quarters in which interviews for the Eurobarometer were conduced (some waves spread over multiple quarters), this country is coded as being in recession. Data on quarterly GDP growth rates over previous period (seasonally and calender adjusted) taken from Eurostat (2017a).

- *Unemployment Rate:* Weighted average (by number of respondents in the respective month of the field period) of the seasonally adjusted monthly unemployment rate (% of active population). Respondents with missing information on interview date ignored in these analyses. Data taken from Eurostat (2016c).

- *Inflation:* Weighted average (by number of respondents in the respective month of the field period) of the annual rate of change of the harmonized index of consumer prices (HICP). Data taken from Eurostat (2017b).

- *Election Year:* Dummy equaling 1 in years when elections for the government or the parliament that then formed the government were held. Sources: Data on presidential vs. parliamentary systems taken from Beck et al. (2001) and data on the timing of elections taken from the ECPR's Political Data Yearbook (European Consortium for Political Research 2015).

- *Core Government Spending:* Measure that deducts social benefits other than social transfers in kind and social transfers in kind, capital transfers, adjustments for the change in pension entitlements, other current transfers, and interest payments from total yearly general government spending. Data

taken from Eurostat (2016a) with the following formula using the codes in Eurostat's government revenue, expenditure and main aggregates table is as follows: TE - D62_D632PAY - D9PAY - D8 - D7PAY - D41PAY (in €1'000).[31]

Appendix 2.B: A Brief Overview of the Spanish Economic Crisis

This section briefly describes the Spanish economic downturn which started in 2008 when Spain went into recession. To this end, I discuss some peculiarities of the Spanish economic crisis and focus on four elements: the surge in unemployment, the crash of the housing bubble, the recession, and the request for financial assistance. I will briefly discuss those in turn.

After years of good news from the Spanish labor market, the trend reversed in 2007 (see Figure 2.B1). Unemployment grew only slightly at first but growth in joblessness accelerated enormously in 2008. The steepest increase took place between late 2007 and early 2009. Afterwards, growth in unemployment slowed down but only after joblessness had more than doubled within less than two years. One of the reasons for this surge in unemployment was the collapse of the Spanish real estate bubble in 2008. House prices started to fall in the second quarter of 2008 and investment in construction decreased for more than 15 consecutive quarters. Spain moreover went into recession in the second quarter of 2008 and this recession, which lasted until the first quarter of 2010, was much sharper than previous ones as investment in housing had been very important in Spain prior to the crisis and this is a "demand component that tends to generate the most pronounced cyclical oscillations" (Ortega and Peñalosa 2012, 26). Additionally, during this crisis, Spain's inflation rate also fluctuated quite markedly (see Figure 2.B2).

In 2008 and 2009, the Spanish government tried to address the economic woes by introducing a package of expansionary policies but the crisis continued to

[31] Due to lack of data about several components used to calculate core government spending the following observations are excluded from the analyses: Bulgaria (2007-2013), Finland (1997-2013), Greece (2001-2005), Hungary (2004-2013), Latvia (2004-2013), Lithuania (2004-2013), Poland (2004-2013), Slovakia (2004-2013), Sweden (1997), and United Kingdom (1997-2013).

worsen and unemployment increased further. Consequently, since their strategy to pursue an expansive fiscal policy had failed (Conde-Ruiz and Marín 2013, 22), Spanish authorities changed their approach from 2010 onwards and adopted budget cuts as well as tax increases to cope with the downturn. In June 2012, the Spanish government finally had to request financial assistance to recapitalize its financial institutions.[32] The collapse of the housing market left the banks vulnerable which necessitated this step. To comply with the conditions for the banking bailout set by the Eurozone countries, the Spanish government announced substantial budget cuts, raised taxes for 2013[33] and finally passed a budget with a total of €39 billion in budget cuts later in 2012.[34] The IMF estimates that the Spanish economy will take until 2017 to return to the size it was in 2008 before the bubble burst. As of 2015, the economy shows signs of recovery but many people still experience economic hardship on a daily basis.[35]

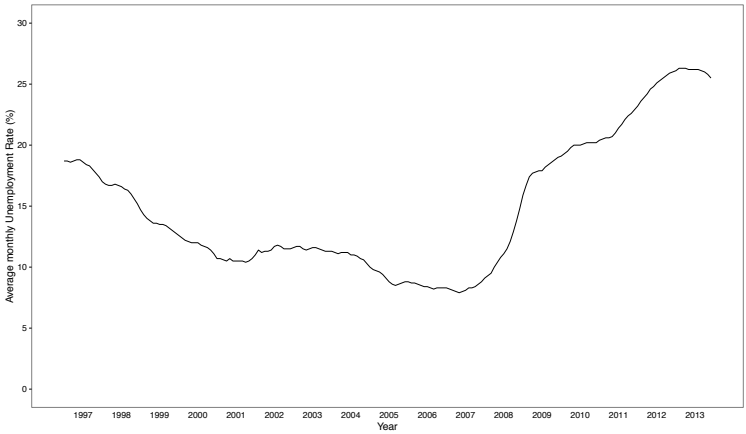
It is important to note that the aforementioned austerity measures the Spanish government resorted to started to take effect in May 2010. Such budget cuts may negatively impact trust. Because the economic downturn was the prime reason in forcing the Spanish government to adopt adjustment measures, however, the possible negative effect of these measures on political trust can also be attributed to the economic crisis.

[32]Teevs, Christian. 2012. "Many Questions Remain: Spain Officially Requests Aid for its Ailing Banks," *Spiegel Online*, June 25. Accessed February 06, 2017. `http://www.spiegel.de/international/europe/spain-issues-official-request-for-eu-bailout-of-its-banks-a-840873.html`.

[33]Tremlett, Giles. 2012. "Mariano Rajoy Announces €65bn in Austerity Measures for Spain." *The Guardian*, July 11. Accessed February 06, 2017. `https://www.theguardian.com/business/2012/jul/11/mariano-rajoy-spain-65bn-cuts`.

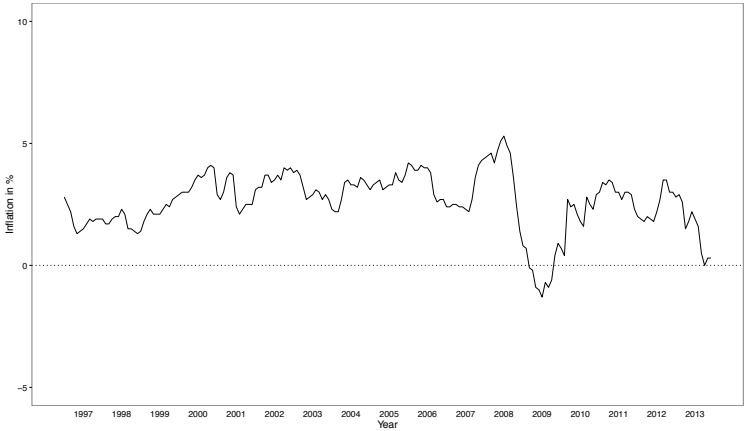
[34]"Spain Passes Massive Austerity Cutbacks." 2012. *thejournal.ie*, December 20. Accessed February 06, 2017. `http://www.thejournal.ie/spain-passes-cutbacks-725680-Dec2012/`.

[35]"Spain's Recovery: Not Doing the Job." 2016. *The Economist*, April 24. ccessed February 06, 2017, `http://www.economist.com/news/europe/21649660-spanish-unemployment-ticks-up-again-many-workers-are-sinking-poverty-not-doing-job`.



Note: This figure shows the seasonally adjusted monthly unemployment rate for Spain (percent unemployed of active population). Source: Eurostat (2016c).

Figure 2.B1: Unemployment in Spain



Note: This graph plots the monthly data for the annual rate of change in inflation in Spain (m/(m-12)). Source: Eurostat (2017b).

Figure 2.B2: Inflation in Spain (Annual Rate of Change)

Appendix 2.C: Additional Information about the Data used in the Synthetic Control Method Analysis

The analyses using the synthetic control method are based on the same waves of the Eurobarometer surveys as the previous analyses but also use additional data, which I describe in this section. Since this is a data-driven procedure, the usage of these variables may vary to achieve the best fit.
Variable definitions:

- *Trust in the Government/Parliament/Political Parties/Legal Sytem/Army:* Share of respondents that expressed trust in the respective institution by answering "tend to trust" to the survey question "I would like to ask you a question about how much trust you have in certain institutions. For each of the following institutions, please tell me if you tend to trust it or tend not to trust it." Respondents that answered "Don't know" set to missing.
- *Age:* Share of respondents falling into the following age categories: 18–29, 30–39, 40–49, 50–59, 60–69, 70–79, and over 79.
- *Education:* Share of respondents falling into the following categories: Stopped education when they were up to 15 years old or have no full-time education, stopped education when they were 16 to 19, stopped education when they were over 20 or are still studying.
- *Male:* Share of male respondents in sample.
- *Unemployed:* Share of unemployed respondents in sample.
- *Real GDP p.c.:* GDP per capita in €, chain linked volumes. Source: Eurostat (2016b).
- *Real GDP p.c. Growth:* Yearly GDP p.c. growth rates, chain linked volumes (in %). Source: Eurostat (2016b).
- *Unemployment Rate:* Average yearly unemployment rate (in % of active population). Source: Eurostat (2016d).

Appendix 2.D: Technical Details about the Synthetic Control Method

In this section, I discuss in some more detail how synthetic control method (SCM) works. To illustrate the basic idea, I use the potential outcomes notation (Gerber and Green 2012, 21ff.). Let $Y_{c,t}^{I}$ denote the share of people trusting the government during an economic crisis, the intervention, in countries $c = 1, \ldots, C + 1$ at time $t = 1,2,\ldots,T$. $Y_{c,t}^{N}$ is trust in non-crisis times.[36] Let $c = 1$ denote Spain, the treated unit, and countries $c = 2$ to $c = \mathrm{C} + 1$ constitute the donor pool. I am interested in $a_{1,t} = Y_{1,t}^{I}$ - $Y_{1,t}^{N}$, which denotes the difference in the share of people trusting the national government in Spain exposed to the intervention (superscript I) and under control conditions (superscript N), i.e., not exposed to the intervention. It is, however, not possible to observe both those outcomes at the same time, i.e., trust shares in Spain during an economic crisis and without economic crisis. This is known as the so-called "fundamental problem of causal inference" (Holland 1968). To impute the missing counterfactual, I employ the SCM (Abadie, Diamond, and Hainmueller 2010, 2015; Abadie and Gardeazabal 2003). The following discussion closely follows Abadie, Diamond, and Hainmueller (2015, 497).

To impute the counterfactual for Spain, I use a weighted average of control countries $c = 2,\ldots,C + 1$ in my donor pool. This synthetic control can be represented by a $(\mathrm{C} \times 1)$ vector containing weights $\mathbf{W} = (\mathrm{w}_2,\ldots,\mathrm{w}_{\mathrm{C}+1})'$, with $0 \leq \mathrm{w_c} \leq 1$ for $c = 2, \ldots, C + 1$ and $\mathrm{w}_2 +, \ldots, + \mathrm{w}_{\mathrm{C}+1} = 1$. Each $\mathbf{W}$ is one possible synthetic control group. I choose the combination of weights W^* that minimizes the difference between the pre-crisis characteristics of Spain and the synthetic control given by the vector X_1 - $X_0 W$.

The estimator of the effect of the intervention, i.e., in my case the Spanish economic crisis, on trust in the government is given by the difference between the level of trust in Spain and in the synthetic control:

$$\hat{a}_{1,t} = Y_{1,t} - \sum_{c=2}^{C+1} w_c^* Y_{c,t}.$$

[36] To facilitate reading of this section, I only refer to the outcome variable trust in the government. The same logic, however, also applies to the other outcome variables.

This allows me to estimate how the Spanish economic downturn affects trust in political institutions.

The credibility of the counterfactual critically hinges on the pre-treatment fit and hence reducing the deviations in the outcome variable between observed Spain and synthetic Spain in pre-crisis times. I assess the credibility of the counterfactual in two ways in the main body of the text: First, I check whether Spain and the control follow similar trajectories in the outcome variable. Second, I examine the root mean square prediction error (RMSPE) which measures the lack of fit between the evolution of the outcome variable in Spain and its synthetic counterpart.

Data limitations restrict the number of pre-treatment periods to nine/ten.[37] This, however, should not pose a problem and theoretically it may make sense to focus on a limited period prior to the intervention. Going further back in time, the data-generating process may have been different than shortly before the crisis. This would consequently also influence the calculation of the variable and country weights. In this case, the synthetic control method may produce predictor weights that do not accurately depict which factors drive trust in the respective political institution in the time prior and during the Spanish economic crisis. Thus, it seems reasonable to focus on the years in the new millennium and a shorter period to avoid using data which was characterized by a different data-generating process.

[37] For trust in the government, the dataset contains nine pre-treatment periods, for the other outcome variables ten.

Appendix 2.E: Significance of the Treatment Effect for Trust in the Government

To gauge the statistical significance of this effect, I calculate the difference-in-differences estimate comparing the pre-crisis period with the crisis period (see Table 2.E1). The mean share of people trusting the government in the pre-treatment period is virtually identical in Spain and synthetic Spain with a t-value of the difference-in-means test of 0. Trust levels in synthetic Spain are lower in the post-2007 period than before but the decline in Spain is much stronger. The difference-in-differences estimate[38] suggests that during the crisis, the share of people trusting the government was on average about 21 percentage points (t-value: -4.5) lower than in the pre-crisis period. Thus, while there is no significant difference in people trusting the government between Spain and synthetic Spain before 2008, the difference-in-differences estimate for the difference between pre-crisis and crisis period is highly significant and substantially relevant.

[38] I calculate the estimate using the means in the share of people trusting the government in the following way: (Spain$_{crisis}$ - Spain$_{pre\ crisis}$) - (Synthetic Spain$_{crisis}$ - Synthetic Spain$_{pre\ crisis}$).

Table 2.E1: Difference-In-Differences Estimates of the Negative Effect of the Economic Crisis on Trust in the Government

		Spain		Synthetic Spain					
	Period	Mean	SD	Mean	SD	N	DID	t-Value After-Before	t-Value Treated-Synthetic
Government	Pre	0.50	0.04	0.50	0.04	9			0.0
	Post	0.23	0.14	0.44	0.05	6	-0.21	5.4	-4.5

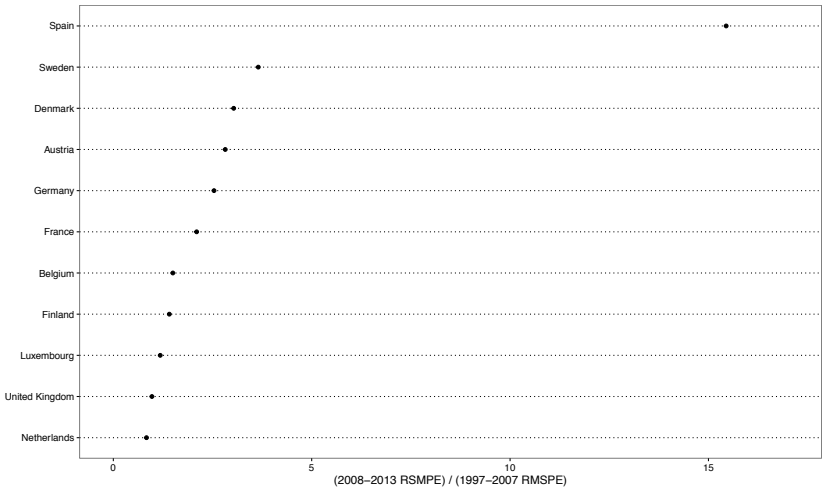
Note: This table present the difference-in-differences estimate along for synthetic Spain along with a t-test. Mean is the share of respondents trusting the government (0-1), SD is the standard deviation, N is the number of observations, DID is the difference-in-differences estimate, t-value is the value of the test statistic for a difference-in-means test.

Another way to assess the statistical significance of the results is to conduct placebo studies wherein I reassign the treatment to control units when excluding Spain ("in-space placebos")[39] (Abadie, Diamond, and Hainmueller 2010, 497). To this end, I use the same predictors and search for configurations that minimize the RMSPE for every single country in the donor pool.[40] Some of these placebo studies produce quite a good fit while others hardly approached the placebo-treated unit. In order to nevertheless take advantage of these results, I follow the example of Abadie, Diamond, and Hainmueller (2015, 505) by dividing treatment period RMSPE by pre-treatment period RMSPE for all placebo studies and Spain, respectively. A large crisis RMSPE by itself does not indicate a large effect of the intervention if the pre-crisis fit is poor. Calculating the ratio of crisis RMSPE to pre-crisis RMSPE across all possible configurations, i.e., Spain as well as the placebo studies, allows me to assess if the RMSPE-ratio of the Spanish case is unusually large in comparison with the others.[41] As Figure 2.E1 shows, Spain clearly stands out as the country with the highest RMSPE-ratio. The crisis-gap is about 15 times as large as the pre-crisis gap, followed by Sweden where the ratio is well below 5. Thus, if I was to pick one country at random from the sample, the probability of obtaining a RMPSE-ratio as high as this one would be $1/11 \simeq 0.09$. This strongly increases the credibility of these findings.

[39] Because I use predictors averaged over the entire pre-treatment period as well as from certain years, I do not conduct any in-time placebos.

[40] To produce counterfactuals that closely follow the trend in the placebo treated unit, I use a more detailed categorization of the education variable for some countries that consists of five categories instead of three.

[41] By doing so, I avoid having to take the decision which of the pre-crisis fits I deem too poor to be used as credible counterfactual.



Note: This figure displays the ratio of crisis RMSPE (period 2008-2013) to pre-crisis RMSPE (1997-2007) for the countries listed on the y-axis. The higher this ratio, the bigger the difference between a country and its synthetic counterpart during the crisis period compared to pre-crisis times. Or to put it differently, the smaller this ratio, the smaller the difference in the mean deviations between a country and its synthetic counterpart in crisis compared to pre-crisis times.

Figure 2.E1: Ratio of Crisis to Pre-Crisis RMSPE: Spain and Control Countries

Appendix 2.F: Analyzing Similarities in the Data-Generating Process

The plausibility of causal identification in synthetic control studies also hinges on whether the same structural process drives the outcome variable in the donor units and the treated unit (Abadie, Diamond, and Hainmueller 2015, 497). To explore to what extent this is the case, I analyze the data-generating process in the donor pool countries that form synthetic Spain (Denmark, Finland, France, Luxembourg, and Sweden) and observed Spain, respectively. I estimate regressions which model trust in the government using the same set of independent variables as predictors that I also use to construct the counterfactual.

Due to the small number of observations I have for Spain (n = 15), I test the individual-level variables with individual data and the country-level variables with aggregate data. This is not optimal but it does at least allow for providing some informative evidence on the similarities (or dissimilarities) in the data-generating processes. It is important to note, however, that the aggregate-level results are more informative here because these allow me to test the data-generating process at the level of analysis used for the main estimation. If the coefficient on any given variable has the same sign and/or significance in both models, it is more likely that the data generating process is largely comparable in both sets of countries.

Individual Level

Table 2.F1 presents the results of the individual-level regressions. Model 1 shows the estimates for Spain while Model 2 reports the results for the countries (pooled) used to create synthetic Spain. To account for differences in the general levels of trust between countries, Model 2 includes country fixed effects. First, none of the variables that are significant in both models differ with respect to their sign although the exact magnitude varies. This suggests that the direction of the correlations between individual-level trust in the government and these categories/variables are similar in Spain and the countries I use to construct synthetic Spain. I also find that only two coefficients (highest level of education and dummy for 70-79 year old individuals) differ with respect to their sign when comparing Model 1 and Model 2. Thus, these two categories differ with respect to how

they correlate with the outcome variable compared to the base categories of these variables and this hints at some dissimilarities between Spain and the countries that form synthetic Spain. Furthermore, some coefficients are significant only in one of the two models which is also a sign of differences. Overall, these results only partly support the claim that the data-generating process in Spain and the countries I used to construct synthetic Spain is similar at the individual level. However, I find some evidence of similar dynamics with respect to important variables/categories. Moreover, since the main analysis presented above relies on aggregate-level data, it appears more important to examine the correlations at the macro level.

Aggregate Level

Table 2.F2 shows the results of the aggregate-level regressions that model the share of people trusting the government as a function of economic indicators: the unemployment rate, GDP per capita, and GDP per capita growth. Model 1 reports the results for Spain and Model 2 shows the results for the pooled set of countries used to generate synthetic Spain. To take into account country-level differences in the level of trust, Model 2 includes country fixed effects. Most importantly, I find that the coefficient of the predictor unemployment rate has the exact same magnitude and significance level in both models. This strongly supports the assumption that the unemployment rate exerts the same effect on aggregate-level trust in Spain as in the pooled set of donor countries, which I use to construct synthetic Spain. There seem to exist some minor differences with respect to the other variables. For example, GDP per capita has a negative impact on trust in the Spanish government while the variable is not significant in Model 2. GDP per capita growth does not reach statistical significance in either of the two models.

To answer the question if the coefficients of these variables are statistically significantly different from each other in the two models, I estimated an additional regression model containing a dummy indicator for Spain and the interaction of this dummy indicator with all the independent variables in the model. If the interaction terms are statistically significant, it means that the effect of the respective variable differs in the two subsamples. Table 2.F3 presents the results.

Table 2.F1: Individual-Level Determinants of Trust in the Government in Spain and the Countries Forming Synthetic Spain

	(1) Spain	(2) Donor Pool Countries
Level of Education		
Middle	0.00	0.01*
	(0.01)	(0.01)
High/Still studying	-0.01	0.09***
	(0.01)	(0.01)
Age		
18-29	-0.09***	-0.13***
	(0.03)	(0.01)
30-39	-0.08***	-0.14***
	(0.03)	(0.01)
40-49	-0.08***	-0.14***
	(0.03)	(0.01)
50-59	-0.02	-0.14***
	(0.03)	(0.01)
60-69	-0.01	-0.10***
	(0.03)	(0.01)
70-79	0.01	-0.06***
	(0.03)	(0.01)
Unemployed	-0.20***	-0.06***
	(0.01)	(0.01)
Country FE		✓
Constant	0.47***	0.65***
	(0.02)	(0.01)
Observations	13410	62718
Adjusted R^2	0.030	0.069

Note: This table reports OLS coefficients from a linear regression of trust in the government (0-1) on sociodemographic variables along with robust standard errors (in parentheses). Model 1 presents the regression results for Spanish individuals, Model 2 those for a pooled sample of individuals from Denmark, Finland, France, Luxembourg, and Sweden, the countries that form synthetic Spain. Reference categories: Low level of education, over 79 years old. * p <0.10, ** p <0.05, *** p <0.01.

Table 2.F2: Aggregate-Level Determinants of Trust in the Government in Spain and the Countries Forming Synthetic Spain

	(1) Spain	(2) Donor Pool Countries
Unemployment Rate	-0.03***	-0.03***
	(0.00)	(0.01)
Real GDP p.c.	-0.00***	0.00
	(0.00)	(0.00)
Real GDP p.c. Growth	-0.01	0.00
	(0.00)	(0.00)
Country FE		✓
Constant	2.06***	0.72***
	(0.18)	(0.09)
Observations	15	75
Adjusted R^2	0.943	0.737

Note: This table reports OLS coefficients OLS coefficients from a linear regression of trust in the government (share of people trusting the government from 0-1) on economic indicators and country fixed effects along with robust standard errors (in parentheses). Model 1 presents the regression results for Spain, Model 2 those for a pooled sample of Denmark, Finland, France, Luxembourg, and Sweden, the countries that form synthetic Spain. * p <0.10, ** p <0.05, *** p <0.01.

As expected, the coefficient of the interaction of Spain and the unemployment rate is not significant and thus I find no evidence that this variable exerts a statistically significantly different effect on trust-shares in the two sets of countries. The same also applies to GDP p.c. growth, which also fails to reach statistical significance. The interaction of Spain with GDP per capita, however, is significant at the 1% level which suggests that this variable has a different effect on trust in Spain and the pooled countries used to construct synthetic Spain. A brief look at Table 2.I4 on page 63, which displays the variable weights that result from applying the synthetic control method, shows that this variable has a small weight which means that it only played a marginal role in fitting synthetic Spain. Hence, the significant difference with respect to the influence of this variable in the two sets of countries can be safely ignored. Overall, these similarities increase my confidence in that the countries in the donor pool can be plausibly used to construct synthetic Spain.

To sum up, these additional tests evidence several similarities in the data-generating processes underlying trust in Spain and the donor countries. Although examinations at the individual and aggregate level reveal some differences, the correlational strucutre with respect to the important variables in terms of variable weights are largely comparable. Specifically, the results show similarities in how the share of unemployed in the sample influences trust levels in the Spanish population and the donor countries. Moreover, since I carry out the main analysis at the aggregate level and I find similarities, the results of this section provide some support for using the donor pool countries (Denmark, Finland, France, Luxembourg, and Sweden) to construct a counterfactual for Spain, thereby adding to the credibility of the causal estimate.

Table 2.F3: Aggregate-Level Determinants of Trust in the Government With Interaction Effects

	(1)
Unemployment Rate	-0.03***
	(0.01)
Real GDP p.c.	0.00
	(0.00)
Real GDP p.c. Growth	0.00
	(0.00)
Spain	1.46***
	(0.22)
Spain * UR	-0.01
	(0.01)
Spain * Real GDP p.c.	-0.00***
	(0.00)
Spain * Real GDP p.c. Growth	-0.01
	(0.01)
Country FE	✓
Constant	0.60***
	(0.15)
Observations	90
Adjusted R^2	0.791

Note: This table reports OLS coefficients from a linear regression of trust in the government (share of people trusting the government from 0-1) on economic indicators, an indicator variable for Spain, country fixed effects, and interactions between these variables along with robust standard errors (in parentheses). * p <0.10, ** p <0.05, *** p <0.01.

Appendix 2.G: Placebo Tests: Trust in Other Institutions

The findings in the main part of this chapter show the extent to which the Spanish economic crisis eroded trust in representative institutions. I argued that this is primarily a consequence of the responsibility these institutions have for steering the economy and providing citizens with economic benefits. To what extent do these results reflect the theoretical mechanism laid out above according to which individuals lose trust in institutions involved in the policymaking process when they are dissatisfied with the economy? Applying the same logic to institutions that do not participate in the policymaking process predicts that trust in such institutions remains unaffected by an economic downturn as they do not have the means to influence the economy. Evaluating this prediction resembles a placebo test which helps to address the concern that the results obtained so far may capture a general trend of declining confidence in political institutions that occurs due to reasons other than the theoretical mechanism outlined above. To explore whether economic hardship also reduced trust in political institutions that are not involved in the policymaking process, I analyze how trust in the legal system and the army evolved before and during the crisis in Spain.[42]

The upper two panels in Figure 2.G1 show the results of the synthetic control method applied to *Trust in the Legal System*.[43] In the pre-crisis period, synthetic Spain replicates the evolution of trust in the legal system very well (RMSPE $\approx$ .02), which increases its credibility as a counterfactual. The difference between Spain and synthetic Spain from 2008 until 2010 reaches a maximum of only about six percentage points. This suggests that trust in the Spanish legal system was largely similar to what it would have been in the absence of the crisis. Moreover, the estimates suggest that while the downturn had a pronounced negative effect on trust in representative political institutions already in 2009 and 2010, trust in the national legal system seems to have remained rather stable in this period.

[42] Unfortunately, questions about trust in these two institutions were included less frequently in the Eurobarometer surveys. For trust in the national legal system, I have data for the same waves as for the other outcome variables but only from 1997 until 2010, and for trust in the army from 1997 to 2010 without 2008.

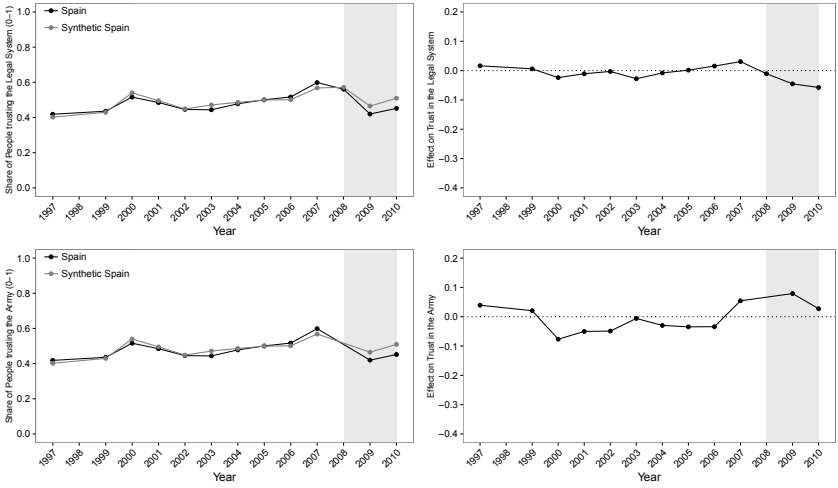
[43] See Tables 2.I9 and 2.I10 for country weights and a comparison of the predictor means before the crisis for trust in the legal system.

This corroborates the idea that the Spanish economic crisis did not negatively influence trust in the legal system.

To explore trust in another institutions that does not participate in the policymaking process, the lower two panels in Figure 2.G1 show the effect of the economic crisis on *Trust in the Army*.[44] A visual inspection of the left-hand panel and the RMSPE of .04 indicate that in pre-crisis times, synthetic Spain approximates the share of people trusting the army in Spain well. The lower right panel in Figure 2.G1 confirms this impression. The deviations during the crisis are positive. i.e. a higher share of individuals trusts the army than without the crisis, but seem small and comparable in magnitude to the negative deviations in the pre-crisis period. These results thus suggest that the crisis did not reduce citizens' trust in the army.

Taken together, the findings of this chapter provide evidence that the economic downturn in Spain strongly and durably decreased trust in representative institutions. The results furthermore suggest that citizen's confidence in the legal system and the army remained at a level very similar to what it had been had the crisis not occurred. Overall, these results thus support the idea that the Spanish economic downturn destroyed much political capital and strongly reduced trust in representative political institutions. However, despite the severity of the crisis, the negative effect of the downturn did not spread to other institutions and it seems likely that the results capture the detrimental effect of the economic downturn rather than a general decline in confidence in political institutions.

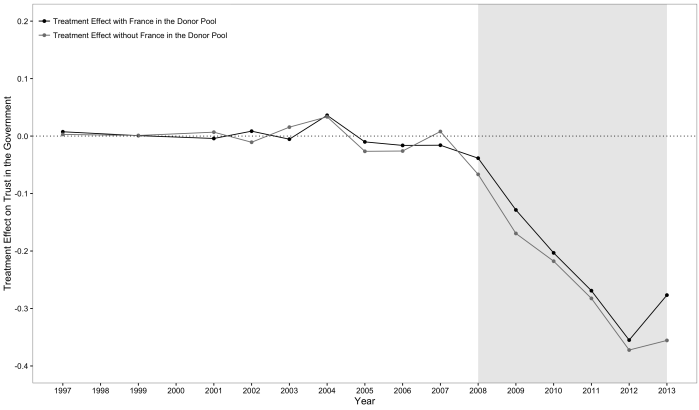
[44]See Tables 2.I11 and 2.I12 for country weights and a comparison of the predictor means before the crisis for trust in the army.



Note: The two upper panels show the results of the synthetic control method applied to trust in the legal system in Spain, the lower two panels those of trust in the Spanish army. The left-hand panels show the evolution of trust in the respective institution in Spain and synthetic Spain for the period from 1997 to 2010. The right-hand panels plot the difference in the share of people trusting the legal system and the army respectively between Spain and synthetic Spain. Negative values illustrate the depressing effect of the economic crisis on trust in these two institutions. The gray-shaded area is the treatment period.

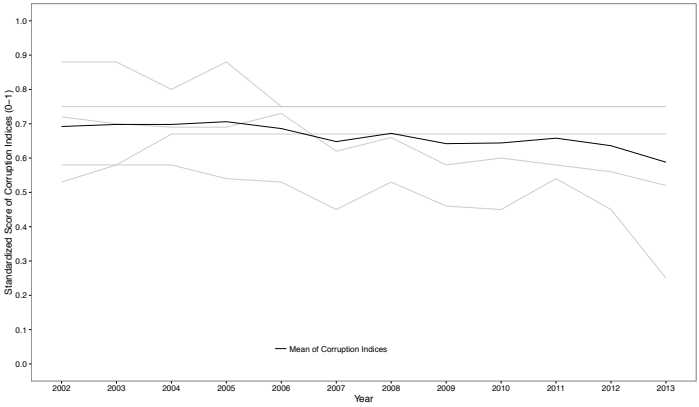
Figure 2.G1: Trust in the Legal System and the Army Before and During the Spanish Economic Crisis

Appendix 2.H: Appendix Figures



Note: The figure plots the difference in the share of people trusting the government between Spain and synthetic Spain with and without France in the donor pool. Negative values illustrate the depressing effect of the economic crisis on trust in the government. The gray-shared area is the treatment period.

Figure 2.H1: Sensitivity Analysis: The Detrimental Effect of the Economic Crisis with and without France



Note: The figure plots the corruption indicators used to construct the World Bank's WGI "Control of Corruption" index for (Kaufmann et al. 2010) which data from 2002 to 2013 is available. The dark line is the average of the individual indicators depicted in light gray.

Figure 2.H2: Corruption Indicators for Spain, 2002 to 2013

Table 2.I1: The Negative Effect of Economic Crises on Political Trust: Controlling for Past Level of Trust

Dependent Variable: Trust in the...	Government			Parliament			Political Parties		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment Rate	-1.33***		-1.34***	-1.28***		-1.31***	-0.71***		-0.71**
	(0.22)		(0.22)	(0.22)		(0.21)	(0.18)		(0.19)
Inflation Rate		0.77	-0.12		0.62	-0.17		0.53	0.03
		(0.47)	(0.38)		(0.55)	(0.45)		(0.35)	(0.35)
Trust in the Governemnt$_{t-1}$	0.33*	0.53***	0.33*						
	(0.12)	(0.11)	(0.12)						
Trust in the Parliament$_{t-1}$				0.43***	0.66***	0.43***			
				(0.11)	(0.08)	(0.11)			
Trust in the Political Parties$_{t-1}$							0.36**	0.50***	0.36**
							(0.10)	(0.10)	(0.10)
Election Year	3.86*	4.11*	3.88*	3.78**	4.09**	3.81**	2.04**	2.09**	2.03**
	(1.35)	(1.53)	(1.37)	(1.01)	(1.13)	(1.02)	(0.64)	(0.66)	(0.63)
Core Government Spending	-0.00	0.04	-0.00	-0.02	0.02	-0.02	-0.02	0.01	-0.02
	(0.03)	(0.05)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Constant	41.34***	20.28**	41.63***	38.50***	18.88***	39.00***	23.33***	15.03***	23.26***
	(8.33)	(6.42)	(8.36)	(7.29)	(4.54)	(7.20)	(4.11)	(3.64)	(4.49)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	207	207	207	233	233	233	233	233	233
Adjusted R^2	0.837	0.808	0.836	0.899	0.881	0.899	0.858	0.839	0.857

Note: The table reports OLS-coefficients of regressions of trust in the respective institution (0-100%) on economic and political variables along with robust standard errors clustered by country (in parentheses). * p <0.5, ** p <0.01, *** p <0.001.

Table 2.I2: Trust in the Government Predictor Means Before the Economic Crisis in Spain

Variable	Year (If Empty: Averaged over Entire Pre-Treatment Period)	Treated	Synthetic	Mean of Donor Pool
Unemployed (Share in Sample)		0.05	0.05	0.05
Education: Share in Sample That Stopped Education When They Were...				
No Full-Time Education, up to 15		0.45	0.17	0.20
16 to 19		0.29	0.35	0.39
Over 20 or Still Studying		0.26	0.48	0.41
Share of Male Respondents in Sample		0.49	0.48	0.48
Age: Share in Sample That is...				
18 to 29		0.23	0.21	0.20
30 to 39		0.20	0.19	0.20
40 to 49		0.17	0.18	0.18
50 to 59		0.14	0.16	0.16
60 to 69		0.13	0.13	0.14
70 to 79		0.10	0.09	0.10
Over 79		0.03	0.03	0.03
Unemployment Rate		11.39	7.68	6.57
Unemployment Rate	2003	11.50	7.59	6.58
Unemployment Rate	2004	11.00	7.91	7.03
Unemployment Rate	2005	9.20	7.68	7.04
Unemployment Rate	2007	8.20	6.61	5.94
Real GDP p.c.		22411.11	37816.81	36900.00
Real GDP p.c.	2004	23100.00	38834.00	37750.00
Real GDP p.c.	2005	23500.00	39514.80	38410.00
Real GDP p.c. Growth	2004	1.60	2.78	2.52
Trust in the Government	2004	0.55	0.52	0.47

Table 2.I2 (continued)

Note: The left-most column lists the variables that were used to construct synthetic Spain and the second column the period over which the data was averaged. The third column shows the corresponding figures for Spain, the fourth column those for synthetic Spain, and the last one those for the unweighted average of the donor pool countries Austria, Belgium, Denmark, Finland, France, Germany, Luxembourg, Sweden, the Netherlands, and the United Kingdom in the period 1997 to 2007 for all years for which I have data for the dependent variable.

Table 2.I3: Synthetic Weights for Spain, Outcome: Trust in the Government

Country	Weight
Austria	0
Belgium	0
Denmark	.10
Finland	.31
France	.40
Germany	0
Luxembourg	.14
Netherlands	0
Sweden	.06
United Kingdom	0
RMSPE	0.02

Note: This table lists the weights assigned to the different countries from the donor pool which resulted from using the Synth package in Stata for the outcome Trust in the Government.

Table 2.I4: Trust in the Government: Predictor Weights of Synthetic Spain

Predictor	Year (If Empty: Averaged over Entire Pre-Treatment Period)	Weight
Unemployed (Share in Sample)		0.76
Education: Share in Sample That Stopped Education When They Were...		
No Full-Time Education, up to 15		0.00
16 to 19		0.04
Over 20 or Still Studying		0.00
Share of Male Respondents in Sample		0.00
Age: Share in Sample That is...		
18 to 29		0.00
30 to 39		0.00
40 to 49		0.01
50 to 59		0.01
60 to 69		0.00
70 to 79		0.00
Over 79		0.09
Unemployment Rate		0.00
Unemployment Rate	2003	0.00
Unemployment Rate	2004	0.00
Unemployment Rate	2005	0.00
Unemployment Rate	2007	0.01
Real GDP p.c.		0.00
Real GDP p.c.	2004	0.00
Real GDP p.c.	2005	0.00
Real GDP p.c. Growth	2004	0.01
Trust in the Government	2004	0.06

Table 2.I4 (continued)

Note: This table shows weights of each predictor that was used to construct the synthetic control group. Lower weights mean that the predictor played a minor role in determining the country weights and their importance increases with the weights.

Table 2.I5: Synthetic Weights for Spain, Outcome: Trust in the Parliament

Country	Weight
Austria	0
Belgium	0
Denmark	0
Finland	0
France	.59
Germany	0
Luxembourg	.35
Netherlands	.04
Sweden	.02
United Kingdom	0
RMSPE	0.02

Note: This table lists the weights assigned to the different countries from the donor pool which resulted from using the Synth package in Stata for the outcome Trust in the Parliament.

Table 2.I6: Trust in the Parliament Predictor Means Before the Economic Crisis in Spain

Variable	Year (If Empty: Averaged over Entire Pre-Treatment Period)	Treated	Synthetic
Unemployed (Share in Sample)		0.05	0.04
Education: Share in Sample That Stopped Education When They Were...			
No Full-Time Education, up to 15		0.45	0.20
16 to 19		0.29	0.41
Over 20 or Still Studying		0.26	0.39
Share of Male Respondents in Sample		0.49	0.48
Age: Share in Sample That is...			
18 to 29		0.24	0.21
30 to 39		0.20	0.21
40 to 49		0.17	0.18
50 to 59		0.14	0.15
60 to 69		0.13	0.13
70 to 79		0.10	0.10
Over 79		0.03	0.03
Unemployment Rate		11.44	6.71
Unemployment Rate	2003	11.50	6.67
Unemployment Rate	2004	11.00	7.38
Unemployment Rate	2006	8.50	7.14
Real GDP p.c.		22310.00	44494.59
Real GDP p.c.	2004	23100.00	45901.80
Real GDP p.c.	2005	23500.00	46542.90
Real GDP p.c. Growth	2004	1.60	2.38
Real GDP p.c. Growth		2.59	2.47
Trust in the Parliament	2004	0.53	0.53

Table 2.I6 (continued)

Note: The left-most column lists the variables that were used to construct synthetic Spain and the second column the period over which the data was averaged. The third column shows the corresponding figures for Spain, and the fourth column those for synthetic Spain.

Table 2.I7: Synthetic Weights for Spain, Outcome: Trust in the Parties

Country	Weight
Austria	.10
Belgium	.26
Denmark	.13
Finland	0
France	.23
Germany	0
Luxembourg	.27
Netherlands	0
Sweden	0
United Kingdom	0
RMSPE	0.02

Note: This table lists the weights assigned to the different countries from the donor pool which resulted from using the Synth package in Stata for the outcome Trust in the Parties.

Table 2.I8: Trust in the Parties Predictor Means Before the Economic Crisis in Spain

Variable	Year (If Empty: Averaged over Entire Pre-Treatment Period)	Treated	Synthetic
Unemployed (Share in Sample)		0.05	0.05
Education: Share in Sample That Stopped Education When They Were...			
No Full-Time Education, up to 15		0.45	0.20
16 to 19		0.29	0.39
Over 20 or Still Studying		0.26	0.41
Share of Male Respondents in Sample		0.49	0.48
Age: Share in Sample That is...			
18 to 29		0.24	0.20
30 to 39		0.20	0.20
40 to 49		0.17	0.18
50 to 59		0.14	0.15
60 to 69		0.13	0.13
70 to 79		0.10	0.10
Over 79		0.03	0.03
Unemployment Rate		11.44	6.17
Unemployment Rate	2003	11.50	6.36
Unemployment Rate	2004	11.00	6.92
Unemployment Rate	2005	9.20	6.76
Unemployment Rate	2007	8.20	5.98
Real GDP p.c.		22310.00	43382.63
Real GDP p.c.	2004	23100.00	44652.90
Real GDP p.c.	2005	23500.00	45362.40
Real GDP p.c. Growth	2004	1.60	2.65
Real GDP p.c. Growth	2006	2.50	2.56

Table 2.I8 (continued)

Trust in the Parties	2004	0.29	0.28

Note: The left-most column lists the variables that were used to construct synthetic Spain and the second column the period over which the data was averaged. The third column shows the corresponding figures for Spain, and the fourth column those for synthetic Spain.

Table 2.I9: Synthetic Weights for Spain, Outcome: Trust in the Legal System

Country	Weight
Austria	.24
Belgium	.20
Denmark	0
Finland	0
France	.56
Germany	0
Luxembourg	0
Netherlands	0
Sweden	0
United Kingdom	0
RMSPE	0.02

Note: This table lists the weights assigned to the different countries from the donor pool which resulted from using the Synth package in Stata for the outcome Trust in the Legal System.

Table 2.I10: Trust in the Legal System Predictor Means Before the Economic Crisis in Spain

Variable	Year (If Empty: Averaged over Entire Pre-Treatment Period)	Treated	Synthetic
Unemployed (Share in Sample)		0.05	0.05
Education: Share in Sample That Stopped Education When They Were...			
No Full-Time Education, up to 15		0.45	0.21
16 to 19		0.29	0.44
Over 20 or Still Studying		0.26	0.35
Share of Male Respondents in Sample		0.49	0.48
Age: Share in Sample That is...			
18 to 29		0.24	0.22
30 to 39		0.20	0.20
40 to 49		0.17	0.18
50 to 59		0.14	0.15
60 to 69		0.13	0.13
70 to 79		0.10	0.09
Over 79		0.03	0.03
Real GDP p.c. Growth		2.59	1.83
Trust in the Legal System	2007	0.60	0.57
Trust in the Legal System	1999	0.44	0.43
Trust in the Legal System	2003	0.44	0.47
Unemployment Rate	2003	11.50	7.54

Note: The left-most column lists the variables that were used to construct synthetic Spain and the second column the period over which the data was averaged. The third column shows the corresponding figures for Spain, and the fourth column those for synthetic Spain.

Table 2.I11: Synthetic Weights for Spain, Outcome: Trust in the Army

Country	Weight
Austria	0
Belgium	.33
Denmark	0
Finland	0
France	.04
Germany	.18
Luxembourg	0
Netherlands	0
Sweden	.45
United Kingdom	0
RMSPE	0.04

Note: This table lists the weights assigned to the different countries from the donor pool which resulted from using the Synth package in Stata for the outcome Trust in the Army.

Table 2.I12: Trust in the Army Predictor Means Before the Economic Crisis in Spain

Variable	Year (If Empty: Averaged over Entire Pre-Treatment Period)	Treated	Synthetic
Unemployed (Share in Sample)		0.05	0.06
Education: Share in Sample That Stopped Education When They Were...			
No Full-Time Education, up to 15		0.45	0.19
16 to 19		0.29	0.38
Over 20 or Still Studying		0.26	0.43
Share of Male Respondents in Sample		0.49	0.49
Age: Share in Sample That is...			
18 to 29		0.24	0.20
30 to 39		0.20	0.19
40 to 49		0.17	0.17
50 to 59		0.14	0.16
60 to 69		0.13	0.15
70 to 79		0.10	0.11
Over 79		0.03	0.03
Unemployment Rate		11.44	7.75
Unemployment Rate	2003	11.50	7.77
Unemployment Rate	2004	11.00	8.34
Unemployment Rate	2005	9.20	8.65
Unemployment Rate	2006	8.50	8.11
Real GDP p.c.		22310.00	32777.63
Real GDP p.c.	2004	23100.00	33743.50
Real GDP p.c.	2005	23500.00	34362.70
Real GDP p.c. Growth	2004	1.60	3.13
Trust in the Army	2004	0.66	0.69

Table 2.I12 (continued)

Note: The left-most column lists the variables that were used to construct synthetic Spain and the second column the period over which the data was averaged. The third column shows the corresponding figures for Spain, and the fourth column those for synthetic Spain.

Chapter 3

Egoistic and Sociotropic Policy Preferences

*Roman Liesch and Michael M. Bechtel**

3.1 Introduction

What explains why citizens support a specific party, candidate, or policy? One of the most commonly held explanations states that individuals prefer the political option that provides them with the greatest economic benefits. Consistent with this idea, a wealth of evidence suggests that economic factors help explain electoral choice (Kinder and Kiewiet 1981; Nannestad and Paldam 1994), as well as support for reforms in various policy fields including foreign trade (Scheve and Slaughter 2001b), financial openness (Quinn and Toyoda 2007), tax policies (Lü and Scheve 2016), and immigration (Mayda 2006). The ability of citizens to form economic assessments of policies therefore greatly conditions citizens' evaluations of incumbent performance. If voters electorally reward economically competent governments, incumbents seeking re-election face a strong incentive to provide high levels of economic prosperity.

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Any errors resulting from typesetting the document are my own.

While the impact of economic factors in shaping electoral preferences has been widely studied (Healy and Lenz 2014; Kayser and Peress 2012; Malhotra and Kuo 2008; Tavits and Potter 2015), our ability to disentangle the egoistic and sociotropic sources of these effects has been limited. After all, the empirical patterns consistent with sociotropic or pro-social preferences could ultimately reflect egoistic concerns (Funk 2000; Kiewiet and Lewis-Beck 2011). Citizens may, for example, interpret improvements in national economic performance as a positive signal that increases their expectations about harnessing personal benefits such as wage increases or improved job security.

The relative importance of self-interest and pro-social concerns for how voters' assess candidates, parties, or specific policy choices has important implications for the functioning of democracy. Purely self-interested voters create a set of incentives for politicians that differ fundamentally from those generated by voters that favor the provision of overall welfare gains. While both types of individuals may, for example, be sensitive to national income growth, egoistic voters will more quickly become dissatisfied if the expected personal gains fail to materialize and this dissatisfaction with the distribution of collective economic gains may have profound consequences for electoral outcomes. Therefore, the weight voters attach to improvements in their personal well-being and the extent to which they also care about the welfare of others promises to offer an important explanation for the (in-)action of governments to counter the rise in economic inequality (Alt and Iversen 2017). Recent work has made important progress by providing causal estimates of how factors related to self-interest (Bearce and Tuxhorn 2017) and sociotropic considerations (Hainmueller and Hopkins 2015) affect individual attitudes toward policy. At the same time, these causal effects remain open to both egoistic and sociotropic interpretations because the policies' precise redistributive impact on an individual's own financial standing and the welfare of others has not been fully specified. Therefore, individuals could be sensitive to the economic effects of a policy, possibly because of self-interest, sociotropic concerns, or a mixture of the two.

We argue that individuals are sensitive to a country's overall economic performance because of altruistic concerns for the welfare of those living close to or below the poverty line. We derive several empirical implications from this argument and develop a design that isolates egoistic and sociotropic concerns in a

randomized income conjoint experiment. This allows us to fully specify how a policy affects one's personal income, the national average, as well as earnings in three income groups (the poor, the middle class, and top earners). The strength of this research design is that we can begin to simultaneously explore the egoistic and sociotropic origins of policy preferences because it describes in greater detail the personal and societal income effects of a policy. We evaluate our predictions using data from an online survey conducted on a representative sample of about 2,700 American citizens.

Our results suggest that both, personal as well as national-level (average) income changes matter. Voters are about two times more sensitive to personal income gains and three times more sensitive to personal income losses than to similar changes in the average income in the US. However, the overall economic impact of a policy on the society remains a significant driver of individual policy preferences, a sensitivity that seems to reflect pro-social concerns about the welfare of those that are worst off. In contrast, neither large gains nor losses have discernible effects on support for a reform if they affect the middle class and high earners. We establish these results against complementary and rival explanations including social comparisons, group identification (nationalism, partisanship), and Luke's principle of affordable giving ("Anyone who has two shirts should share with the one who has none [...]." Luke 3:11). These findings expand our understanding of the egoistic and non-egoistic roots of political preferences and contribute to the debate about which types of redistribution-related cleavages may explain public support for different reforms, candidates, and political parties.

3.2 The Sources of Egoistic and Sociotropic Policy Preferences

Understanding which policies incumbents pursue requires knowledge about the origins of voter preferences over policy. The dominant view in the literature asserts that individuals rationally support policies from which they expect to personally benefit (e.g., Curtis et al. 2014; Fiorina 1978). Egoistic motivations have been shown to affect electoral behavior (Downs 1957; Duch and Stevenson 2010; Kinder and Kiewiet 1981; Lewis-Beck and Stegmaier 2013), as well as support for

trade policy (Scheve and Slaughter 2001b), European integration (Gabel 1998), and immigration (Scheve and Slaughter 2001a). Initially, scholars believed that an egoistic, rational model of policy preferences would imply relatively low demands in terms of its assumptions about individuals' intellectual and informational capabilities (Kinder and Kiewiet 1981, 130). More recent work shows, however, that the formation of egoistic preferences requires on the availability of specialized knowledge, especially in the context of more complex policy questions such as monetary policy (Bearce and Tuxhorn 2017).

Although rational, self-interested individuals may never be fully informed about the income effects of a reform proposal, political elites face strong incentives to identify and communicate the economic effects of a policy. Some of these efforts have been powerful enough to result in effective political collective action. For example, when the United States imposed sanctions on agricultural products from the European Union in 1999, French farmers successfully lobbied for subsidies (Sobel 2006, 146). Further, the German government in 2015 proposed a climate tax to cut greenhouse gas emissions. Unions reacted by organizing large-scale protests of coal miners and workers in coal-fired plants who feared declining wages and job losses.[1] We theorize how these different types of personal and societal income effects of a policy may influence individual preferences.

3.2.1 Self-Interest and Support for Policy

Self-interest has long been appreciated as an important explanation for individual preferences over policy which are assumed to reflect the utility differences between the available options. A self-interested assessment of policy proposals that affect personal incomes seems important for explaining policy preferences because of rationality, materialism, and individualism (Sears and Funk 1991). Rationality assumes that individuals perform a cost-benefit analyses when having to choose between different options, materialism describes the desire to acquire material goods, and individualism denotes the strong tendency to focus on how a choice affects oneself, a trait that Sears and Funk (1991) characterize as "the most fundamental of American values" (77). Therefore, when assessing the desirability

[1] "Thousands of coal workers march in Berlin in protest against climate tax." 2015. *Reuters*, April 25. Accessed February 1, 2017. `http://www.reuters.com/article/us-germany-coal-protests-idUSKBN0NG0Q220150425`.

of different political reforms, individuals will maximize personal wealth as this allows them to be self-reliant by removing dependence on material support from others. The availability of monetary resources affects an individual's ability to insure against adverse events, such as, unemployment or long-term sickness, and to engage in intertemporal investment choices. Empirically, one's own income strongly predicts individual happiness (Easterlin 2001) and affects voters' evaluations of incumbent performance (Healy and Lenz 2014). Previous work on the importance of self-interest in explaining individuals' policy views suggests that factors related to self interest, for example, home ownership, predicts opposition to a policy that would eliminate the tax deduction for mortgage interest payments (Chong, Citrin, and Conley 2001). Thus, policies that provide individuals with personal income gains should receive higher support while reforms that are associated with income losses will be less popular among voters.

3.2.2 Sociotropic Preferences

Although preferences over policy may strongly depend on the costs and benefits it imposes on an individual, we argue that voters also care about how a reform changes a nation's average income and the distribution of wealth within a country. This argument relies on two classes of mechanisms: Egoistic and pro-socioal motivations. An important challenge lies in theoretically and empirically discriminating between those two explanations. As Kinder and Kiewiet (1981) put it, "the distinction between pocketbook and sociotropic politics is *not* equivalent to the distinction between a self-interested and an altruistic politics" (132). This is because the sensitivity to the seemingly sociotropic features of a policy may ultimately reflect self-interested concerns. Therefore, we distinguish between two different types of egoistic and sociotropic explanations for preference formation that we subsequently subject to an empirical test.

A widely held belief is that the sensitivity of individual preferences to aggregate measures of economic performance ultimately reflects egoistic concerns because individuals interpret this information as indicative of their personal economic future (Sears and Funk 1991). A complementary explanation focuses on the net effects of changes in a country's level of wealth and the associated tax

burden.[2] This argument contends that individuals may favor economic upturns because this implies a lower number of welfare recipients. Under the assumption of constant per capita transfers, a lower number of individuals below the poverty line may be associated with lower tax rates, which may be felt particularly among richer individuals due to progressive tax schemes. In the context of support for economic reforms, we would therefore expect that an increase in average incomes in a country cause higher levels of support, while reforms that decrease average incomes will reduce individuals' willingness to back a reform. However, the sensitivity to the income effects of a reform may also originate from sociotropic motivations that reflect other-regarding preferences. We distinguish between two pro-social explanations related to altruism and nationalism.

Altruism: Individuals often care about the well-being of others that are worse off and are consequently willing to incur personal losses to improve their welfare (Elster 2006, 186). Such altruistic behavior gives rise to two sources of satisfaction. The first source has been called "warm glow" (Andreoni 1990), which originates from the act of giving while the second results from having improved the well-being of those that are worse off. A large literature has documented the existence of altruistic giving (Fehr and Schmidt 2006) suggesting that individuals derive some consumption benefit from helping those that have less even if their contributions have been made involuntarily (Harbaugh, Mayr, and Burghart 2007; Smeets, Bauer, and Gneezy 2015). We therefore expect that individuals will oppose policies that entail income losses for the poor while they will support reforms that increase incomes among low earners.

A stronger version of altruism would predict that individuals oppose income losses inflicted on the poor independent of whether they themselves benefit financially. In contrast, a weaker form consistent with the principle of affordable giving (Luke 3:11) would predict that one's willingness to care for the poor depends on whether one personally enjoys income gains. Consequently, individuals should be more opposed to income losses among the poor if they personally experience income gains. We will assess the empirical validity of these expectations further below.

Nationalism: Voters may also care about the broader impact of economic policy

[2]See Facchini and Mayda (2009) for a related argument in the study of support for immigration.

because of their nationalist identities, which consist of emotional and cognitive attachments to the country (Brown 2000; Huddy 2013; Tajfel 1981). The sense of national belonging is strongly related to feelings of pride that give rise to concerns about the nation's policy choices and its economic status (Dimitrova-Grajzl, Eastwood, and Grajzl 2016). Previous work suggests that national identity helps predict support for foreign economic policy such as trade liberalization (Mayda and Rodrik 2005) and domestic redistribution (Shayo 2009). Therefore, national attachments could also increase concerns about the nation's overall economic performance. In the context of support for policy reforms that have broader income effects, individuals would then be sensitive to average income changes because of their national identity. Specifically, policies that entail losses in the nations' average income would reduce reform support out of nationalist sentiment. This notion also underlies the policies outlined in U.S. President Trump's inauguration speech: "We are one nation and their pain is our pain. Their dreams are our dreams. And their success will be our success."[3] Therefore, even if an individual may not personally benefit from better economic conditions, identification with the nation may increase one's willingness to support policies that provide the country with economic benefits.

3.2.3 Loss Aversion and Effect Asymmetry

So far, our theory has focused on why individuals care about the personal and societal impact of a policy on incomes. These arguments, and the existing previous literature, have treated economic losses and gains as comparable in that the effects are assumed to be symmetric. An economic gain g is supposed to increase support by d and a loss in the order of g is assumed to decrease support by d. Yet, experiencing welfare increases as opposed to decreases may not necessarily have symmetric effects on support for a reform since individuals have been shown to be generally more concerned about losses than about benefits. The existence of loss aversion has been widely documented in the lab-experimental literature. For example, already Kahneman and Tversky (1983) report results from several studies

[3]Blake, Aaron. 2017. "Trump's full inauguration speech transcript, annotated." *Washington Post*, January 20. Accessed January 22, 2017. `https://www.washingtonpost.com/news/the-fix/wp/2017/01/20/donald-trumps-full-inauguration-speech-transcript-annotated/?utm_term=.64ceb34bf838`.

in which subjects preferred an option if it framed a decision problem in terms of the associated potential gains instead of losses even though the expected values of the options were actually identical (Tversky and Kahneman 1986). Following this reasoning, we argue that individuals should be more sensitive to income losses in comparison to increases that are equal in absolute terms.

3.2.4 Social Comparisons and Fairness

Assuming that the effects of a reform on an individual's own financial standing are perfectly known, purely self-interested voters should assess the attractiveness of this proposal exclusively based on this information. However, previous work suggests that individuals often assess distributive effects relative to a specific benchmark (Kahneman, Knetsch, and Thaler 1991; Tversky and Kahneman 1974). Whether an income change is perceived as acceptable or not may therefore depend on whether others experience similar income effects. Asymmetries in the distribution of economic benefits within a society can conflict with conceptions of fairness which may trigger emotional reactions such as envy (Fehr and Schmidt 1999; Henrich et al. 2001). The existence of relative income assessments may also explain the well-known Easterlin paradox, which contends that aggregate levels of happiness have remained largely unchanged despite strong increases in per capita income (Clark, Frijters, and Shields 2008). This suggests that the effect of personal income changes on support for a policy depends on whether others gain or lose.

3.3 Sample and Experimental Design

To estimate the sensitivity of individual preferences to the multidimensional income effects of a policy, we design a survey instrument that includes a fully randomized income conjoint experiment (Hainmueller, Hopkins, and Yamamoto 2014). We conduct the survey on a nationally representative sample of American citizens (N=2,723).[4] The conjoint experiment asks respondents to choose between

[4]We programmed the questionnaire in Qualtrics and worked with Respondi, an international survey firm, to recruit respondents. The field period was September/October 2016. See Table 3.D1 in the Appendix for more information about the composition of the distribution of sociodemographics in the raw sample, the voter population, and the weighted sample.

two policy options that randomly vary along several features to explicitly model the multidimensional nature of policies. This enables us to explore both the relative importance of different income effects and the trade-offs that individuals make when facing asymmetric income effects, i.e, some individuals or groups experience gains while others suffer losses. Employing a conjoint design to explore the formation of policy preferences is preferable over a vignette design for at least three reasons. First, it mirrors the fact that a meaningful policy decision requires making a choice between at least two alternatives. Second, many reforms affect the well-being of several groups simultaneously. While these effects could in principle be uniform, they will often be heterogeneous, as some groups will gain more than others. Our design enables us to explore those multi-dimensional effects and how they affect reform support. Third, the effects estimated based on data from a conjoint design replicate behavioral benchmarks better than vignette-based designs (Hainmueller, Hangartner, and Yamamoto 2015). Although the choices respondents make in the conjoint experiment are hypothetical, this research strategy allows us to test the competing theoretical implications and assess the relative importance of egoistic as well as sociotropic factors.

As Table 3.1 shows, we implement a fully-randomized conjoint that explicitly specifies the effects of a policy on different incomes: Average national income, one's own personal income, as well as low (about $10,000 per year), medium (about $85,000 per year), and high income individuals (about $375,000 per year). Those categories reflect the observable distribution of incomes in the United States (Lü and Scheve 2016; Lü, Scheve, and Slaughter 2012). The low-income group comprises individuals below the official poverty line as defined by the Census Bureau and accounts for about 24% of the American population.[5] The second group accounts for the middle class as about 88% of the population has a total annual income less or equal to $85,000 per year. The third group includes high-income individuals earning about $375,000 per year.[6] We follow previous work and randomize the order of the dimension across respondents, but not within subjects to avoid confusing respondents.

[5]This figure also includes individuals without income. For information about the definition of the poverty thresholds see Proctor, Semega, and Kollar (2016, 43) and for information about the incomes by total work experience taken from the CPS 2016 Annual Social and Economic Supplement see U.S. Census Bureau (2017).

[6]See U.S. Census Bureau (2017).

Table 3.1: Policy Dimensions and Values for the Sociotropic Preferences Conjoint Experiment

Dimension	Values
Income Changes	
Average income in the United States	+$5,000 +$2,500 $0 -$2,500 -$5,000
Your personal income	+$5,000 +$2,500 $0 -$2,500 -$5,000
Income Changes in Different Income Categories	
Change in average income of individuals that earn about $10,000 per year	+$5,000 +$2,500 $0 -$2,500 -$5,000
Change in average income of individuals that earn about $85,000 per year	+$5,000 +$2,500 $0 -$2,500 -$5,000
Change in average income of individuals that earn about $375,000 per year	+$5,000 +$2,500 $0 -$2,500 -$5,000

Note: The table shows the dimensions and corresponding values used in the conjoint experiment.

The values for each dimension are randomly drawn from the following set of income changes: +$5,000, +$2,500, $0, -$2,500, -$5,000. These values correspond to empirically meaningful shifts of income changes in the United States over the past 50 years. As a recent example, average household income has decreased by about $4,000 from the time when Bill Clinton left office until the end of Barack Obama's presidency.[7] We communicate the economic effects of policies in terms of absolute income changes instead of relative (percentage) changes. This quantity states the economic impact most directly and should be easier to understand for respondents. This way we intend to maximize individuals' knowledge about the policies' precise income effects (Bearce and Tuxhorn 2017). We assess the sensitivity of our results to this choice in the robustness section. Overall, our design intends to capture a large set of income effects ranging from broad patterns

[7]Sorkin, Andrew Ross. 2016. "President Obama Weighs His Economic Legacy." *The New York Times Magazine*, April 26. Accessed February 20, 2017. `https://www.nytimes.com/2016/05/01/magazine/president-obama-weighs-his-economic-legacy.html?_r=0`.

of economic fluctuations (that affect salaries throughout the country and among specific income groups) to the specific effects of the policy on one's own income.

3.3.1 Outcome Variable: Reform Support

To measure support for different economic policies that randomly vary in terms of their effects on incomes, we asked respondents to choose between two policies and indicate which one they prefer.[8] This is equivalent to a ranking of the policies in a binary contest. We use this binary choice as our dependent variable (*Reform Support*), which equals one if a respondent chose this policy in the binary comparison and is 0 otherwise. In the robustness section we evidence that our results remain unchanged if we use an individual rating of each policy on a scale from 1 to 10 as a measure of support.

3.3.2 Measuring Potential Moderators: Social Norms and Nationalism

Exploring the sources of sociotropic preferences necessitates individual-level measures of social norms as specified by the theory. To avoid priming a specific sensitivity among respondents, these measures came after the income conjoint experiment.

Altruism: Our measure of an individual's level of altruism is based on the well-known dictator game that we transformed into a charity-dictator game to make it suitable for inclusion in large-scale survey. Following previous work (Bechtel, Hainmueller, and Margalit 2014; Bechtel and Scheve ming), we inform respondents that we will raffle a gift card worth $100 among all participants completing the questionnaire and that the winner can decide to donate a share of the gift card to charity.[9] If respondents indicate that they want to donate, we offer a long list of charities from which individuals can choose or write in a charity of their choice. We then ask them the amount they would like to allocate to their chosen charity and use this transfer as a proxy for altruism.

Nationalism: We measure nationalism using a standard survey item which asks respondents to specify their level of identification with the nation. Our

[8] See Section 3.B in the Appendix for more information about the conjoint experiment.
[9] See Section 3.A in the Appendix for more information about the instructions.

coding is based on the question: "Many consider themselves to be part of one or more groups. Some are more important to them than others. In general, how important are the following groups in describing who you are?" We measure an individual's national attachment on a scale from not at all important (1) to very important (5) and then convert this measure into a binary indicator that equals 1 for respondents that report the nation to be a fairly important or very important group and is 0 otherwise. This coding is equivalent to a median split, i.e. the indicator splits the sample into two equally large groups (Appendix Table 3.D2 reports detailed descriptive statistics for all covariates).

3.4 Empirical Results

3.4.1 Egoistic and Sociotropic Preferences

We estimate the effects of various types of income changes by regressing our binary measure of support for policy on a set of indicator variables that distinguish the different treatment groups. We define the no income change condition ($0) as the reference group for each dimension. Figure 3.1 reports the causal effects graphically along with 95% robust confidence intervals. How strongly do individuals care about the direct impact of a policy on their own earnings? According to the estimates reported in Figure 3.1, a $5,000 personal income loss reduces support by about 12.5 percentage points, while a $5,000 salary gain increases support by 11 percentage points.[10] We find large and significant causal effects also when examining smaller personal income changes of $2,500.

We now evaluate whether and to what extent individuals respond to societal income changes. Since our experimental design fully informs respondents about the effects of each reform on their own disposable income, purely self-interested individuals should not be sensitive to how a reform affects the national average or other income groups. Yet, and consistent with the theory, we find that strong decreases (-$5,000) in average national income significantly reduce support for a policy by about 6.5 percentage points. Similarly, a decline in the national average income by $2,500 causes policy support to shrink by about 3.5 percentage points.

[10]We use survey weights for all estimations. The results remain almost unchanged when re-estimating the effects without weights (see Figure 3.C5 in the Appendix).

Compared to the baseline level of public support (which is 0.5), these estimates are equivalent to a 13% and 7% decrease, respectively.

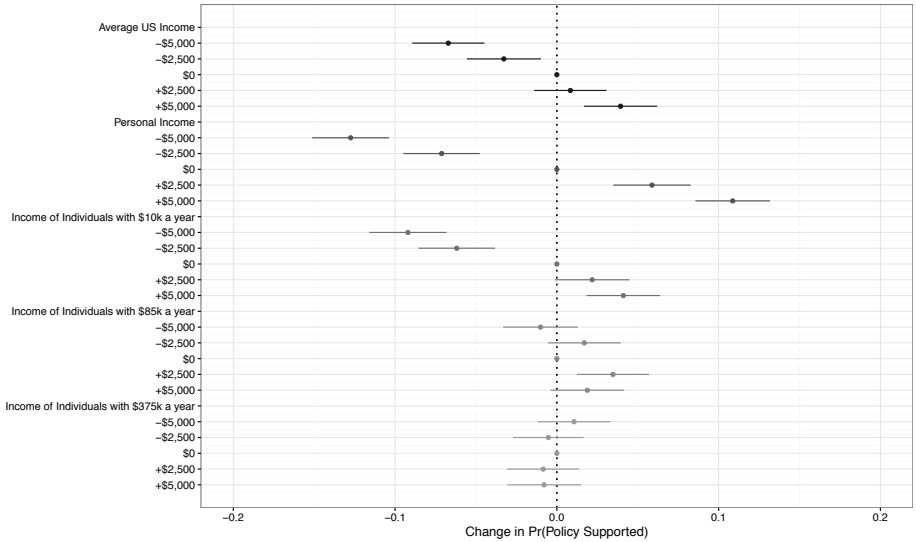
Figure 3.1 directly assesses the relative importance of national (sociotropic) and personal (egoistic) income effects.[11] When focusing on salary cuts, we find that personal income losses are about twice as important as national income losses. This asymmetry is even more pronounced when examining the causal effects of salary gains as opposed to losses. For example, a \$5,000 increase in average US incomes causes support for a policy to raise by about 4 percentage points. However, the same nominal (\$5,000) increase in one's own income increases support by about 11 percentage points. This means that the sensitivity to large personal income gains is about three times higher than to national income increases of the same magnitude.

3.4.2 Loss Aversion

We argue that individuals should exhibit an asymmetry in how strongly they respond to a reform that entails income losses as opposed to gains. Specifically, voters should be less sensitive to personal income gains than to losses of the same absolute magnitude. The results in Figure 3.1 suggest that a \$5,000 (\$2,500) personal income loss decreases policy support by 12.5 (7) percentage points on average. In contrast, a personal income gain of \$5,000 (\$2,500) causes policy support to increase by 11 (6) percentage points. To assess in more detail whether the hypothesized effect asymmetry exists, Figure 3.2 plots the absolute magnitude of the estimated causal effects and the difference between the estimates of absolute positive and negative income changes (Δ) for each amount and dimension. For example, when considering a nominal income change of \$5,000, we compute their absolute effects and plot the difference along with the corresponding 95% confidence intervals.

As indicated by the negative and significant estimate of Δ in Figure 3.2, we find that individuals are significantly less sensitive to large (\$5,000) and medium (\$2,500) average income gains than to income losses. The results also indicate that a somewhat more pronounced asymmetry exists in the extent to which individuals

[11] Figure 3.C1 in the Appendix shows very similar results when using the rating mechanism as dependent variable although the magnitude of the effects differs a bit.



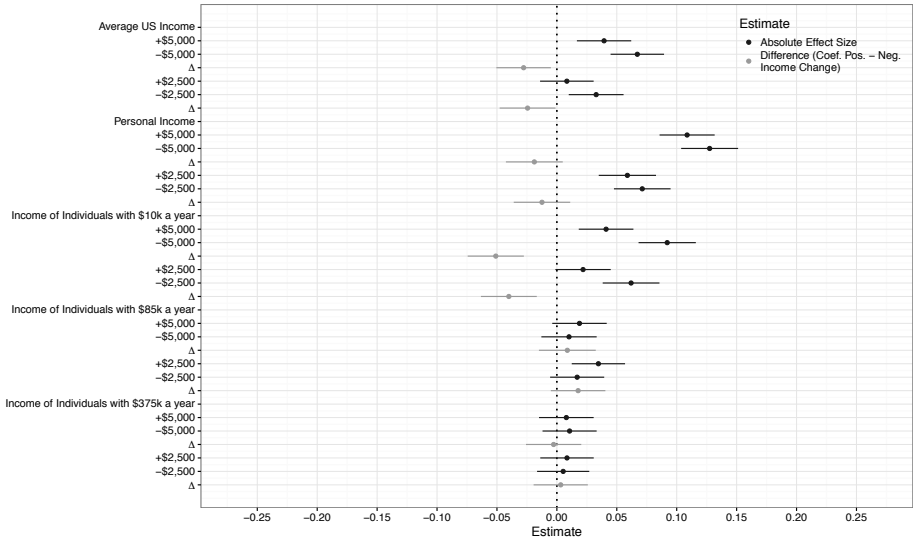
Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: .52. N (respondents) = 2,723, N (rated policies) = 18,918.

Figure 3.1: The Causal Effects of Income Changes on Reform Support

respond to large ($5,000) and medium-sized ($2,500) income changes among the poor. For the personal income effects, the point estimates of Δ have the expected negative sign, although they only reach statistical significance at the 10% level. Overall, these results suggest that the sensitivity to a reform's impact on average incomes, and incomes among the poor, tends to be asymmetric. Individuals are more sensitive to the losses associated with a policy proposal than to its potential gains.

3.4.3 Income Effects on the Poor, the Middle Class, and the Rich

Our experimental results suggest that preferences for political reform depend on both, the proposal's personal income effects and its impact on income averages



Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Black points and lines represent absolute effect sizes relative to no income change for the respective dimension, grey points and lines represent the difference between the absolute effects of positive and negative income changes for the respective amounts and dimensions. *Example:* The effect of an average income increase by $5,000 is about 3 percentage point smaller than the effect of an average income loss of the same absolute size. Baseline level of support: .52. N (respondents): 2,723, N (rated policies): 18,918.

Figure 3.2: The Causal Effects of Income Changes on Reform Support: Asymmetry

in the country. This finding is important since if individuals were purely self-interested, we would expect that the availability of detailed information about the exact personal economic consequences of a reform would remove any sensitivity to changes in national income averages. At the same time, this finding begs the question of why respondents continue to care about average income changes.

By definition, the income average is a function of incomes among the poor, the middle class, and the rich. According to our argument, respondents should be most concerned about the impact of a policy on the well-being of those that are worst off, while the impact on the rich should not matter much. The results shown in the lower part of Figure 3.1 suggest that this is the case. We find that

individuals are quite averse to policies that worsen the living conditions of the poor ($10,000). For example, according to our estimates, a policy that reduced the income of low earners by $2,500 decreases support by 6 percentage points. Such income losses may well be an empirical reality for low earners becoming unemployed or having to accept part-time positions. It seems noteworthy that this effect is almost exactly as large as that of a corresponding $2,500 personal income gain, which suggests that responses to income gains and losses can be close to symmetric. Individuals are also more likely to support policies that entail income gains for low earners. Policies that increase the income of the poor by $5,000 per year receive four percentage points more support than proposals that leave the incomes of the poor unchanged.

When examining how voters respond to policies that affect medium and high-income individuals, we find much smaller or even null effects. Income losses to individuals making about $85,000 per year does not make a policy significantly more or less attractive. If anything, voters would like this income category to experience moderate gains in the order of $2,500. Strikingly, Americans neither oppose income gains nor support income losses when considering the rich. As we show in the robustness section, this result is not an artifact of conceptualizing the income effects in absolute terms as this finding replicates when communicating the income changes in percentages. Overall, our findings suggest that individuals care about a policy's redistributive income effects on the poor as they oppose policies that entail losses and support reforms that entail income gains. In contrast, a reform's distributive effects on the middle-class and high-earners do not have a notable impact on individual policy preferences, at least in a setting that also details the reform's effects on other income groups and one's own financial standing.

3.4.4 Egoism and Affordable Pro-social Concerns

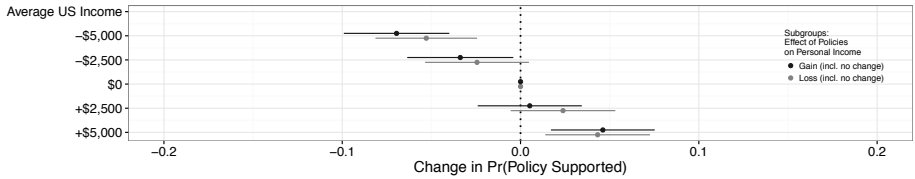
These results are based on average treatment effects, i.e., they rely on a comparison between average policy support in the treatment condition of interest and the benchmark scenario of no income change. Given the strong effects of personal income changes on policy support, one may question whether the sensitivity to average income effects and the concerns for the poor are truly independent of

whether a policy provides personal benefits. After all, the sensitivity to average income effects could still reflect some degree of self-interested concerns. This possibility is not only of theoretical interest because empirically some policies may in fact have opposite effects on one's own income, the average national income, and the distribution of income between different income groups. Such a case may arise, for example, as a consequence of trade liberalization that improves overall welfare, but causes temporary income losses among specific groups that have to undergo a transition period in which the factors of production are re-allocated.

We first evaluate to what extent the sensitivity to average income effects can be explained by whether an individual personally gains or loses. In particular, we study the theoretically interesting scenarios in which there exists a tension between personal and societal income changes. This would be the case, for example, if an individual personally loses while average incomes increase. We re-estimate the causal effects for these two groups of personal winners and losers, which is equivalent to examining an interaction between two features of our conjoint, average income changes and personal income effects. Figure 3.3 shows that the effects of national-level income changes are virtually identical for both groups. Irrespective of whether individuals experience personal income gains or losses, they remain sensitive to changes in national economic conditions. This evidence is again consistent with the idea that the sensitivity of policy preferences to national-level income effects reflects some type of pro-social concerns.

Yet, average income changes still constitute a compositional quantity that could mask an infinite number of redistributive effects and a uniform increase in all income groups will be a rare special case. Historically, average incomes tend to increase mostly because a relatively small number of top earners receive substantially higher incomes, while earnings among the poor remain largely constant. This gives rise to interesting combinations that seem important if individuals engage in social comparisons when assessing the desirability of a reform. Specifically, the preference for shielding the poor from income losses could depend on whether an individual personally gains or loses from a policy.

To explore the robustness of the main results, we estimate the income category effects by whether a respondent will personally gain or lose. Figure 3.4 shows that the overall pattern remains largely unchanged. Policies that inflict losses on the poor are significantly less popular and reforms that provide income gains

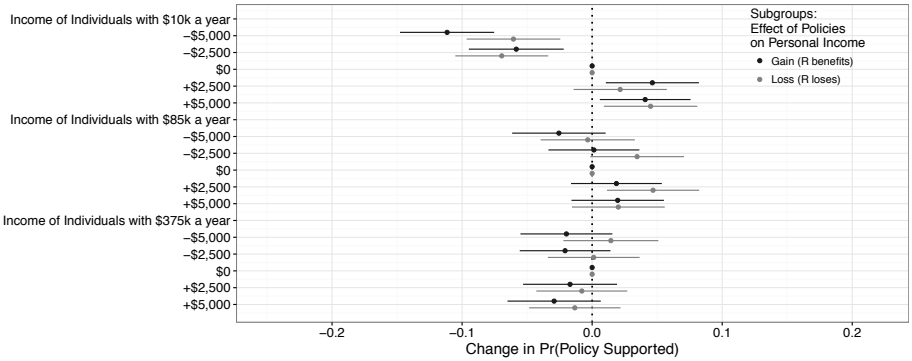


Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: Gain: Includes positive personal income changes as well as no change, Loss: Includes negative personal income changes as well as no change. Baseline level of support: Gain: .53; Loss: .50. Gain: N (respondents) = 2,694, N (rated policies) = 11,351. Loss: N (respondents) = 2,701, N (rated policies) = 11,342.

Figure 3.3: The Causal Effects of Average Income Changes by Personal Income Changes (Gains vs. Losses)

for the poor receive somewhat higher support. Also, irrespective of the personal income effects a policy has, individuals exhibit little sensitivity to the income effects on the middle class and top earners. This suggests that voters care about how policies affect the poor when forming policy preferences and irrespective of whether they win or lose, income reductions for the poor generally reduce support for a reform. Voters also prefer policies that strongly improve the income of the poor independent of the impact of a policy on their personal income.

To what extent do the elasticities we have estimated still reflect egoistic concerns that originate from one's own position in the real-world income distribution? For example, low-income individuals could be more averse to reforms that impose income losses on the poor than to policies that reduce incomes of high earners. To explore this question, Figure 3.5 breaks down the income effects by respondents' own reported incomes. We find that citizens generally dislike policies that cause income losses among the poor, and support reforms that provide low earners sizeable benefits irrespective of their own income. When examining how respondents assess the impact on individuals with medium or high incomes, we find little to no significant effects. Apparently, the less and the more wealthy both share the wish to avoid reforms that inflict income losses on the poor, and both groups care relatively less about how policies affect those with medium and high incomes.

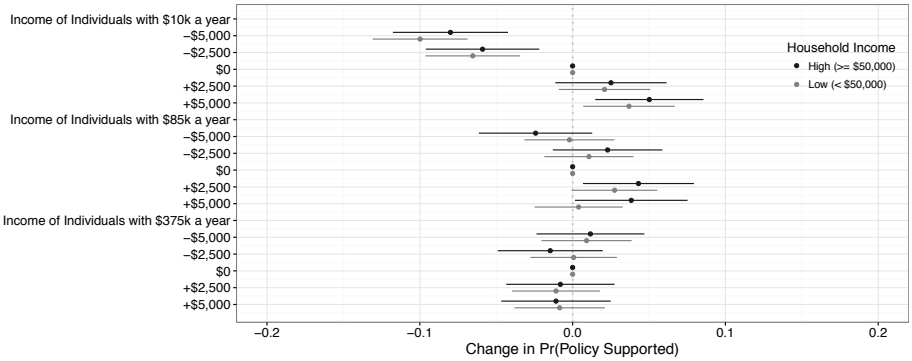


Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: Gain: Positive income changes for respondent, Loss: Negative income changes for respondent. Baseline levels of support: Gain: .67; Loss: .44. Gain: N (respondents) = 2,601, N (rated policies) = 7,576. Loss: N (respondents) = 2,594, N (rated policies) = 7,567.

Figure 3.4: The Causal Effects of Income Changes among Different Income Groups on Reform Support by Personal Income Changes (Gains vs. Losses)

3.5 Evaluating Explanations for Sociotropic Preferences

Our conjoint experiment clearly specifies how a policy affects one's own personal income and average earnings in the country. From a purely self-interested perspective, we would expect that once the personal economic consequences of a reform are fully known, the effects of changes in income averages would disappear. However, our results suggest that even if individuals receive detailed information about the impact of a reform on their personal financial standing, they remain sensitive to average income changes. The previous section suggests that this sensitivity reflects a concern for the welfare of the poor. In this section we continue evaluating the empirical validity of this interpretation by estimating the causal effects separately for theoretically meaningful subgroups that relate to the mechanisms laid out above. The empirical patterns we find will assist in learning about the



Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: High: $50,000 and more (above median category), Low: below $50,000 (below and including median category). Figure 3.C8 in the Appendix shows the distribution of reported household income in the sample. Baseline level of support: High: .52; Low: .53. High: N (respondents) = 1,107, N (rated policies) = 7,628. Low: N (respondents) = 1,610, N (rated policies) = 11,244.

Figure 3.5: The Causal Effects of Income Changes among Different Income Groups by Respondent's Own Income

validity of our theoretical explanations.

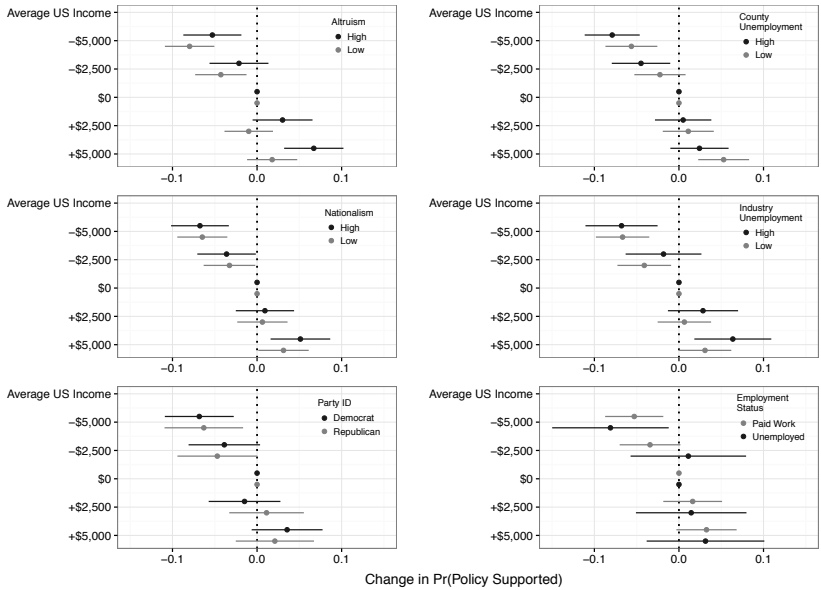
3.5.1 Pro-social Preferences

We first assess whether the sensitivity to national-level changes in average incomes reflects altruistic motivations. Our prediction is that individuals identified as being more altruistic based on their behavior in the charity-dictator game should be more averse to average income losses and more supportive of average income gains than less altruistic individuals. The first panel in the left column of Figure 3.6 shows the causal effects of changes in the nation's average income estimated separately for more and less altruistic respondents. Altruists are coded as those individuals that made a strictly positive contribution in the quasi-behavioral charity-dictator game. We find that the causal effects of average income gains are close to zero and insignificant among non-altruistic respondents. For more al-

truistic individuals, however, average income increases can have a significantly positive impact on reform support. For example, a $5,000 income gain causes policy support to increase by about 7 percentage points among altruists. In contrast, the effect for less altruistic respondents is insignificant. More importantly, additional tests show that the difference between those two effects is statistically significant (Table 3.D4 in the Appendix reports those results in detail). Although the heterogeneity in the effects of average income losses is not significant, the pattern underlying the differences in the effects of income gains seem consistent with an altruistic interpretation.

Another mechanism that could explain sensitivity of support for policy to national income averages relies on individuals' identification with their nation. Voters that more strongly think of themselves as belonging to a specific country should be more sensitive to a reform's impact on a country's overall economic performance. The second panel in the first column of Figure 3.6 shows the results. When we break down the treatment effects by national identity, we find that the treatment effects are very similar. A statistical test of the difference in the estimated effects confirms this impression (see Table 3.D5 in the Appendix). Apparently, citizens are about equally sensitive to the country's overall economic performance irrespective of their nationalist identities. This is inconsistent with a nationalist interpretation of why individuals continue to care about average income changes when evaluating economic reforms even if the effects on their own earnings is perfectly known.

We also estimate the treatment effects by respondents' self-reported partisanship. As Figure 3.6 (first column, third panel) shows, both Democrats and Republicans share similar views when it comes to assessing the desirability of a policy based on how it affects the mean income (Table 3.D6 in the Appendix reports the numerical results and corresponding tests in detail).



Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: Altruism: High: donation > 0, Low: donation $= 0$; Nationalism (measured on five point scale): High: National Identity > 3 (median category), Low: National Identity $<= 3$. County Unemployment: High: Unemployment in County of Residence $>$ Median, Low: Unemployment in County $<=$ Median; Industry Unemployment: High: Industry Unemployment $>$ Median; Low: Industry Unemployment $<=$ Median; Baseline level of support: Altruism (High): .52; Altruism (Low): .53; Nationalism (High): .52; Nationalism (Low): .53; Democrat: .52; Republican: .57; County Unemployment (High): .54; County Unemployment (Low): .51; Industry Unemployment (High): .52; Industry Unemployment (Low): .53; Employment Status (Paid Work): .50; Employment Status (Unemployed): .50. Altruism (High): N (respondents) = 1,175, N (rated policies) = 8,078. Altruism (Low): N (respondents) = 1,548, N (rated policies) = 10,840. Nationalism (High): N (respondents) = 1,153, N (rated policies) = 7,872. Nationalism (Low): N (respondents) = 1,551, N (rated policies) = 10,928. Democrat: N (respondents) = 800, N (rated policies) = 5,608; Republican: N (respondents) = 624, N (rated policies) = 4,298. County Unemployment (High): N (respondents) = 1,308, N (rated policies)= 9,076; County Unemployment (Low): N (respondents) = 1,415, N (rated policies)= 9,842; Industry Unemployment (High): N (respondents) = 751, N (rated policies) = 5,230; Industry Unemployment (Low): N (respondents) = 1,338, N (rated policies) = 9,468; Employment Status (Paid Work): N (respondents) = 1,128, N (rated policies) = 7,752; Employment Status (Unemployed): N (respondents) = 289, N (rated policies) = 2,010.

Figure 3.6: The Causal Effects of Average Income Changes on Reform Support by Altruism, Nationalism, Partisanship, and Measures of Respondents' Employment Status

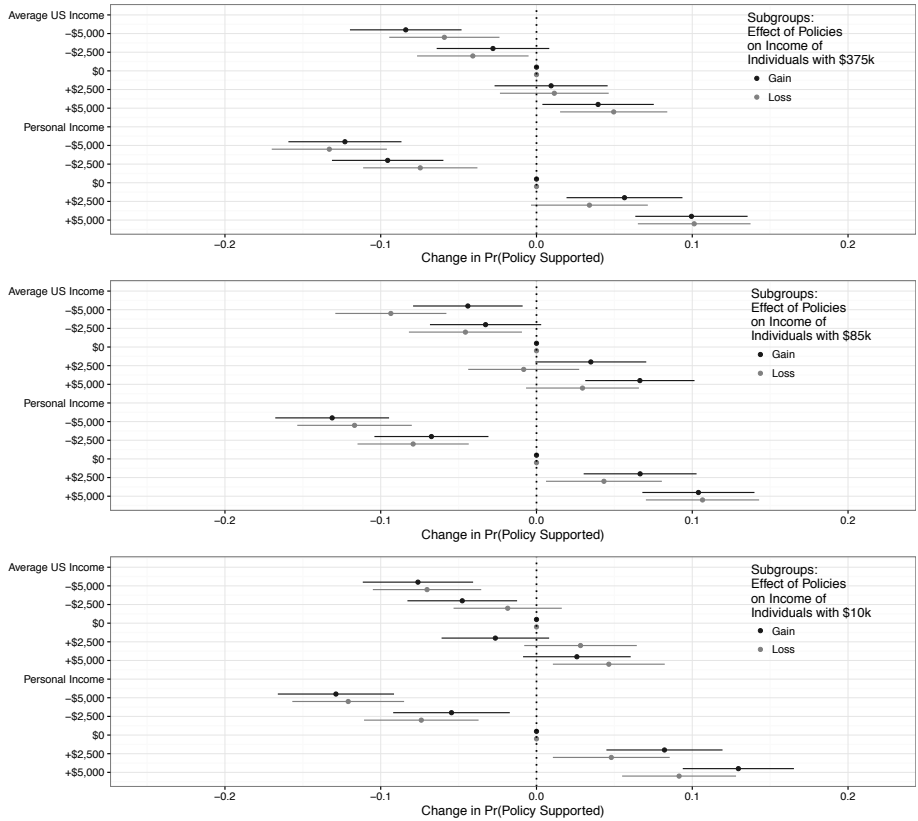
Finally, we re-visit an egoistic reading of the sensitivity to average income changes by exploring heterogeneity in our treatment effects by several measures capturing a respondent's financial standing. The second column in Figure 3.6 shows the results. We first explore whether our treatment effects are larger if we focus on respondents that live in a county with high levels of unemployment. However, compared to individuals from counties with low levels of unemployment, the treatment effects appear to quite similar and the statistical tests for the differences in these effects fail to reject the null hypothesis of no difference (see Table 3.D8 in the Appendix). We repeat this test using alternative measures of respondent's employment and occupational status. Again, we do not find any statistical differences between those subgroups.

3.5.2 Social Benchmarking

When evaluating the distributive consequences of a policy, voters may engage in social comparisons to assess the desirability of personal (and average) income changes. This type of benchmarking could constitute a way to determine the fairness of a reform. For example, the value of changes in one's personal income may appear more or less fair depending on how the policy affects the economic situation of other groups (low, medium, high-income individuals).

The results shown in Figure 3.7 suggest that voters perceive personal income gains as desirable and income losses as undesirable irrespective of whether those with high or medium incomes also gain or lose (i.e., a reform that implies personal income changes is not significantly more or less attractive if it provides the top earners with income gains or imposes losses on them).

However, and in line with our altruism-related findings reported above, we find that individuals are more in favor of policies that make themselves better off if these reforms also benefit low earners. In contrast, if the poor experience income losses, a policy that provides personal benefits is less attractive. While this speaks against a purely egoistic version of social benchmarking, the finding is again consistent with the argument that individuals value personal income gains at least partly because they expect those policies to also make the poorest somewhat better off.

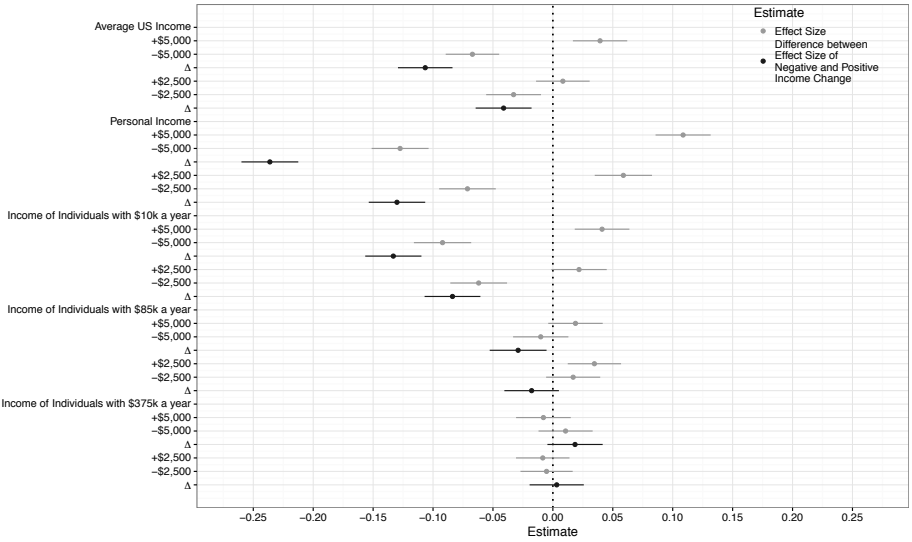


Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: Gain: Positive income changes for the income class stated in the legend; Loss: Negative income changes for the income class stated in the legend. Baseline level of support: High Income (Gain): .54; High Income (Loss): .52; Middle Income (Gain): .54; Middle Income (Loss): .55; Low Income (Gain): .55; Low Income (Loss): .48. High Income (Gain): N (respondents) = 2,580, N (rated policies) = 7,479. High Income (Loss): N (respondents) = 2,589, N (rated policies) = 7,595. Middle Income (Gain): N (respondents) = 2,606, N (rated policies) = 7,637. Middle Income (Loss): N (respondents) = 2,599, N (rated policies) = 7,535. Low Income (Gain): N (respondents) = 2,607, N (rated policies) = 7,698. Low Income (Loss): N (respondents) = 2,586, N (rated policies) = 7,501.

Figure 3.7: The Causal Effects of Average and Personal Income Changes by Gains/Losses for Low, Middle, and High-income Individuals

3.6 Effect Size

To explore the relative magnitude of the treatment effects documented above in more detail, we compute the differences in the effects of a reform's redistributive consequences along with the corresponding 95% confidence intervals. These confidence intervals have to be computed from the variance-covariance matrix of the effect estimates since the significance of the differences cannot be assessed by visually inspecting overlap between the confidence intervals of the two constitutive effects (Cumming and Finch 2005). As the results in Figure 3.8 show, compared to a policy that implies a personal income gain of \$5,000, a proposal that entails a \$5,000 loss reduces reform support by about 23.5 percentage points on average. This is equivalent to a 45% shift relative to the baseline level of support and could on its own bring about pivotal changes in the political viability of a reform. When comparing how individuals respond to personal income effects with their sensitivity to changes in incomes of individuals at the poverty line, we find that reducing one's personal income change from +\$2,500 to -\$2,500 causes a decrease in reform support by 13 percentage points, an effect size that also results from a proposal that entails income changes among the poor from +\$5,000 to -\$5,000. These quantities help to illustrate the relative importance of various types of income changes for the formation of individual policy preferences.



Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Grey points represent the estimated treatment effect relative to the reference group of no change. Black points indicate the difference between a negative income change and a positive income change for the following value combinations: -$5,000,$5,000; -$2,500,$2,500. *Example:* If a policy lowers the average national income by $5,000 as opposed to increasing it by the same amount, this reduces reform support by about 10.5 percentage points. Baseline level of support: .52. N (respondents): 2,723, N (rated policies): 18,918.

Figure 3.8: The Causal Effects of Income Changes on Reform Support: Comparing Positive and Negative Income Changes

3.7 Robustness

We subject our main results to a large set of robustness tests. These include whether our results depend on the measurement of our outcome variable and estimation strategy, how we communicate a reform's income effects, the use of sampling weights, and individuals' attentiveness.

3.7.1 Alternative Measure of Support: Individual Rating of Policy Reforms

Our dependent variable relies on a design in which respondents always select one of the two reform proposals they are shown in a conjoint comparison. We now explore whether our results also hold if we use the rating component of our conjoint design to measure policy support. The rating component asked respondents: "If you could vote on each of these policies in a referendum, how likely is it that you would vote in favor or against each of the proposals? Please give your answer on the following scale from definitely against (1) to definitely in favor (10)". We re-estimate our main results using this rating measure as the dependent variable. Figure 3.C1 in the Appendix shows that the results from this analysis are very similar to those reported above. We again find that both the personal and societal income effects cause significant changes in support for a policy with personal income effects being between two and three times stronger than average income changes. Since the rating measure of reform support is bounded (1-10 scale), we also re-estimate the results using a Tobit model that accounts for the censoring of the dependent variable at 1 and 10. Figure 3.C2 in the Appendix reports the results. Our findings remain unchanged. To safeguard against measurement error, we also analyze a binary support indicator based on the original rating variable. This dummy variable is 1 if a reform proposal received a rating of 6 or higher and is zero otherwise. Figure 3.C3 shows the results. The effects are virtually identical to our main estimates reported above.

3.7.2 Relative versus Absolute Income Effects

Our results suggest that citizens are more sensitive to income changes, especially losses, if they concern low earners. One potential explanation for this finding could be that our conjoint design specifies the income changes in absolute monetary terms, e.g., +$2,500. Respondents could assess the magnitude of those income shifts relative to a person's total annual income. If this was the case, the same nominal income gain or loss would appear much larger if the person had only a very low annual income. To explore whether our results can be explained by our decision to conceptualize income changes in monetary terms, we fielded an

additional income conjoint survey that communicated a policy's income effects in relative terms, e.g., -2%. Figure 3.C4 in the Appendix reports the full results. We find that the sensitivity patterns remain unchanged.

3.7.3 Weighting, Response Time, and Attention

We continue to explore the robustness of our results by analyzing their sensitivity to sampling weights. Figure 3.C5 shows the causal effects of income changes on reform support estimated with and without sampling weights. The results are almost identical. Next, we evaluate the extent to which the findings depend on how much time individuals spent on the binary conjoint comparisons. Figure 3.C6 in the Appendix breaks down the results for choices that were made faster or as fast as the median decision (which was about 30 seconds, see Table 3.D2 in the Appendix) and those that were slower. The results are consistent with our main results reported above. As one would expect, the effects are slightly more precisely estimated and more sizeable for individuals that took more time in answering the conjoint questions. Thus, the more carefully individuals process information about the redistributive consequences of a policy, the clearer their political views on the issue. Finally, we re-estimate the results separately for more and less attentive respondents according to whether subjects passed a screener question. As Figure 3.C7 in the Appendix indicates, the results are reassuringly similar.

3.8 Conclusion and Discussion

In trying to better understand the conditions under which voters support some policies, candidates, and parties, scholars have developed an important distinction between egoistic and sociotropic preferences. We explore the relative importance of those factors and the sources of sociotropic considerations. We argue that voters – while being sensitive to personal income effects – care about the societal impact of policy choices on those that are worst off. In evaluating our theory, we address the problem that has prevented previous work from being able to directly explore these questions. By devising a randomized conjoint experiment that details the impacts of a policy on a person's own income and the incomes of other members of society, we can isolate and directly compare the magnitude of

personal and societal income effects.

The results suggest that both personal and average income effects matter for policy preferences. Moreover, the causal effects of changes in one's own financial situation are about two to three times greater than those of changes in national income averages. We also find that the effects are asymmetric in that income gains have significant, but weaker effects on policy support than income losses of the same absolute magnitude, a pattern which is consistent with theories of loss aversion. Finally, our results suggest that individuals seek to avoid reforms that inflict income losses on the poor, a preference that remains robust irrespective of whether a policy entails positive or negative personal income shocks. Thus, voters seem to engage in social comparisons when assessing the attractiveness of policy options in ways consistent with altruistic motivations.

These findings have several implications for research and policy. First, real-world reforms can affect the distribution of wealth in many ways. Our results generate detailed knowledge about the likely impact of personal and societal income effects on the political feasibility of a reform. These estimates allow policymakers to better gauge the personal economic sacrifices individuals are willing to accept in exchange for more equitable economic outcomes by increasing overall welfare and shielding the poor from negative income shocks. As a consequence, public officials interested in designing reforms can better anticipate the political prospects of different options. Second, since our design details the monetary impact of policy choices on public support, the results also provide the information needed to juxtapose the net economic benefits of a policy and the political support this proposal receives. Thereby, policymakers can determine both the political and economic efficiency of policy choices. Third, our results speak to ongoing debates about the impact of economic inequality on elections. The evidence suggests that while egoistic assessments of incumbent performance will be the most powerful drivers of electoral choice, individuals will also punish governments that offer a poor macro-economic record and that have inflicted income losses on the poor. These sociotropic types of performance voting seem particularly important since the median income has remained very stable since the early 1970s.[12] Absent changes in one's own personal income, voters have to rely relatively more strongly

[12] See Stone et al. (2001) for information based on U.S. Census Bureau data.

on how incumbents or policies have affected the poor and the nation as a whole when forming electoral preferences. Consistent with this idea, President Obama has recently attributed negative assessments of his presidency and the outcome of the 2016 election to two factors: Rising income inequality and the widespread perception of economic growth having stagnated or worsened in the past eight years.[13]

Our study attempts to address a long-standing question about the egoistic and sociotropic roots of policy preferences by specifying in detail the income effects of a reform on different parts of society. This approach allows us to directly explore the relative importance of different income effects for the formation of individual policy preferences. While we believe that this strategy is useful, it comes with several limitations, all of which relate to informational asymmetries and the ecological validity of our study.

First, most citizens typically have very little issue-specific information about policy choices and their consequences. Although reforms that potentially affect an individual's own economic standing constitute one of the strongest incentives to become better informed, knowledge about how that policy affects other parts of the citizenry will likely be limited. Viewed against this background, our results can be seen as providing some suggestive evidence about how policy preferences would change if individuals were fully informed about the income effects of a policy on specific societal subgroup. Second, we have abstracted away from the deliberative (Baccaro, Bächtiger, and Deville 2016) and framing (Druckman and Lupia 2000) dynamics that typically surround major reforms. Subsequent research may begin to explore the extent to which the redistributive repercussions of a policy can condition the impact of deliberation and framing on individual policy preferences in these informationally more complex environments. Third, we fully acknowledge that the individual-level, redistributive effects of policy choices can sometimes remain highly uncertain and this uncertainty could explain why some reforms fail to find majority support even if their overall economic impact is positive and certain (Fernandez and Rodrik 1991). Our study extends this line of research by examining how the prospects of policy change depend on knowledge

[13]Sorkin, Andrew Ross. 2016. "President Obama Weighs His Economic Legacy." *The New York Times Magazine*, April 26. Accessed February 20, 2017. `https://www.nytimes.com/2016/05/01/magazine/president-obama-weighs-his-economic-legacy.html?_r=0`.

about a reform's multidimensional income effects. We hope that subsequent work will be able to combine these two approaches in an effort to generate insights into how uncertainty and redistribution jointly affect individual preferences and the ability of policymakers to implement welfare-enhancing reforms.

Appendix 3.A: Variable Definitions

Dependent Variable:

- *Reform Support:* Binary indicator that is 1 if a respondent has chosen the policy proposal in the conjoint comparison and zero otherwise. Question wording: "Which policy do you prefer?"

Covariates:

- *Personal Income Gains/Losses*: Binary indicator identifying policies which would entail personal income gains or losses for the respondent.
- *Altruism*: Respondent's degree of altruism as measured by the willingness to donate a share of a $100 voucher raffled among all survey participants. Question wording: "We will raffle one $100 Amazon voucher among all respondents that have completed the survey. Alternatively, you can donate parts or all of the gift card to a charity of your choice. The amount you decide to donate will be deducted from your voucher." Individuals that wanted to donate could then choose from a menu of 11 charities or identify a specific charity of their own choice. Subsequently they could enter the amount they wanted to donate (allowing for any integer value between 0 and 100). We convert this raw measure into a binary indicator that equals 1 if the donation was greater than zero (high) and is 0 otherwise (low).
- *Nationalism*: Based on the question: "Many consider themselves to be part of one or more groups. Some are more important to them than others. In general, how important are the following groups in describing who you are?" We measured respondents attachment to their nation on a scale from not at all important (1) to very important (5). The measure was converted into binary indicator that distinguishes between strong national identity (fairly important and very important) and weak national identity (all other categories).
- *Party Identification*: Based on the question: "Is there a particular party you feel closer to than all the other parties?" Respondents that chose yes where

then asked a follow-up question "Which one?" and could chose either Republican, Democratic or other and specify. Converted into binary indicators for self-identifying democrats and republicans.

- *County Unemployment*: Based on respondents' indication of county of residence. We matched this with information about the county unemployment rate (not seasonally adjusted) in the month respondents started to fill out the survey. Data source: Bureau of Labor Statistics Local Unemployment Statistics (Bureau of Labor Statistics 2017). Converted into binary indicator that identifies counties with unemployment rates above (high) vs. below and including the median (low).

- *Industry Unemployment*: Based on respondents' indication of sector and occupation. Coded according to the NAICS classification scheme and matched with data from the Bureau of Labor Statistics for October 2016 (not seasonally adjusted) from the "The Employment Situation - October 2016" (Bureau of Labor Statistics 2016). Converted into binary indicator that identifies respondents employed in sectors with unemployment rates above (high) vs. below and including the median (low).

- *Employment Status*: Based on respondents' indication of status of employment. Unemployed contains individuals that indicated that they are unemployed irrespective of whether they are looking for employment or not.

- *Household Income*: Self-reported household income. Converted into dummy variable that identifies respondents with household income above the median (high) vs below and including the median category (low).

Appendix 3.B: Conjoint Instructions

The directions for the conjoint experiment appeared before the respondent began choosing between policies. Respondents were given the following instructions immediately followed by a screenshot example with further instructions:

> As you may know, policies affect individuals and societal groups in various ways. We are interested in what you think about such policies and the United States's possible adoption of them.
>
> We will now provide you with several examples of what income effects various policies could have. We will always show you two possible policies in comparison. You may like both alternatives similarly or may not like either of them at all. Regardless of your overall evaluation, please indicate which alternative you prefer over the other.
>
> In total, we will show you four comparisons. People have different opinions about this issue and there are no right or wrong answers. Please take your time when reading the potential policies. In addition to deciding which policy you would prefer, we also ask you how likely you would be to vote for or against adopting each policy in a referendum.
>
> The figure below shows the features of the two proposals that you will be choosing between. Note that the order of the features may vary.

Respondents saw the introductory screen for 15 seconds before they were allowed to proceed to the next screen that presented the first binary comparison.

Binary Contests

After the conjoint instructions (Figure 3.B1), respondents were given four conjoint tasks. Each task detailed two policy reforms and asked respondents to rate and rank the proposals, i.e., they were asked to chose the proposal that they prefer and subsequently indicated how likely they would vote in favor or against each of the proposals in a referendum. To ensure that respondents carefully read the two proposals, answers could only be submitted after ten seconds had elapsed.[14]

[14] Figure 3.C6 presents the results by response time differentiating by how long it took respondents to submit their choice.

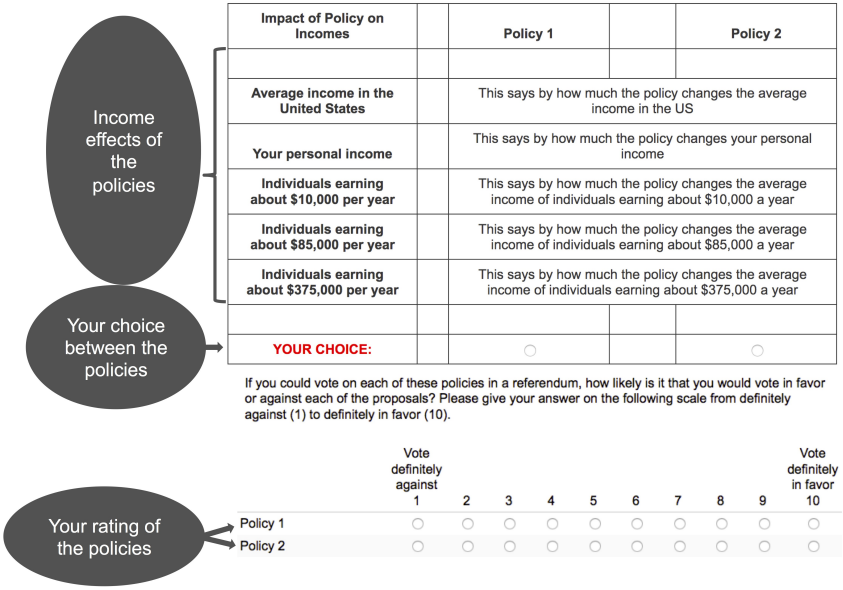
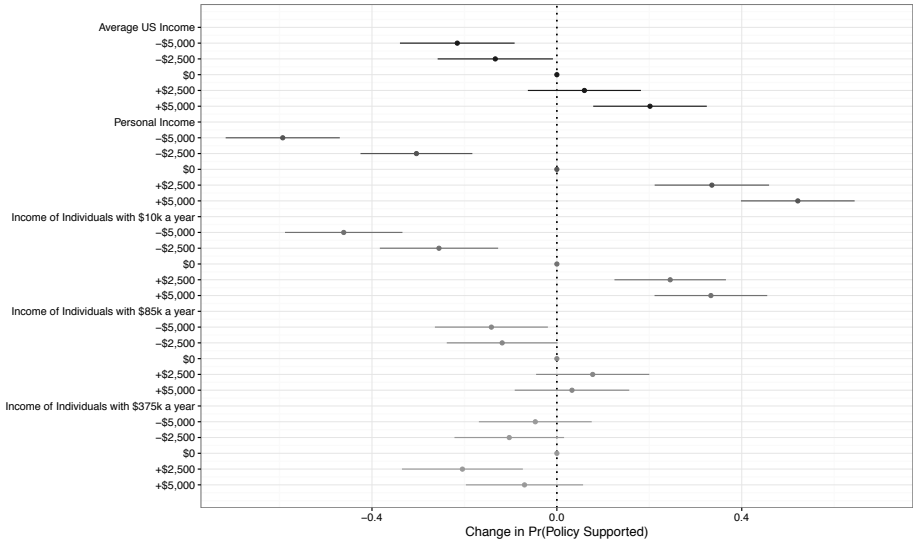


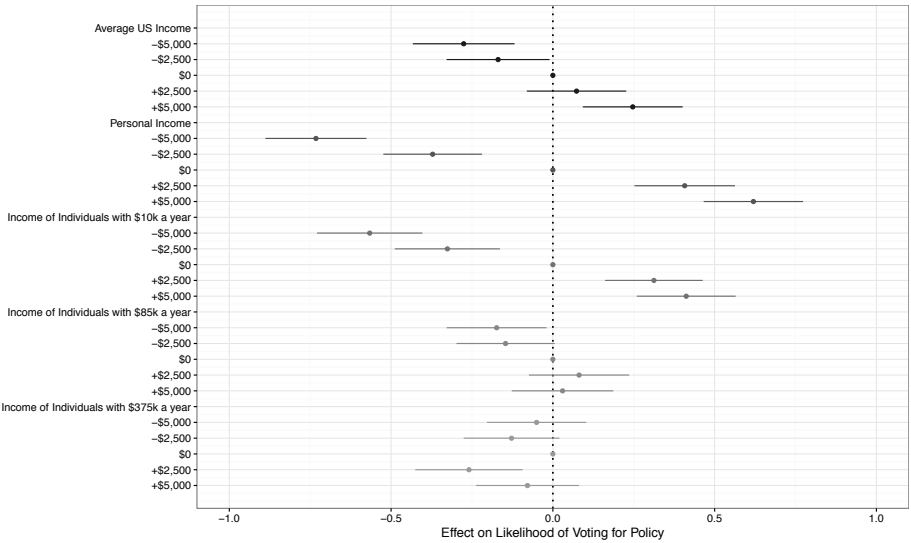
Figure 3.B1: Conjoint Instructions

Appendix 3.C: Appendix Figures



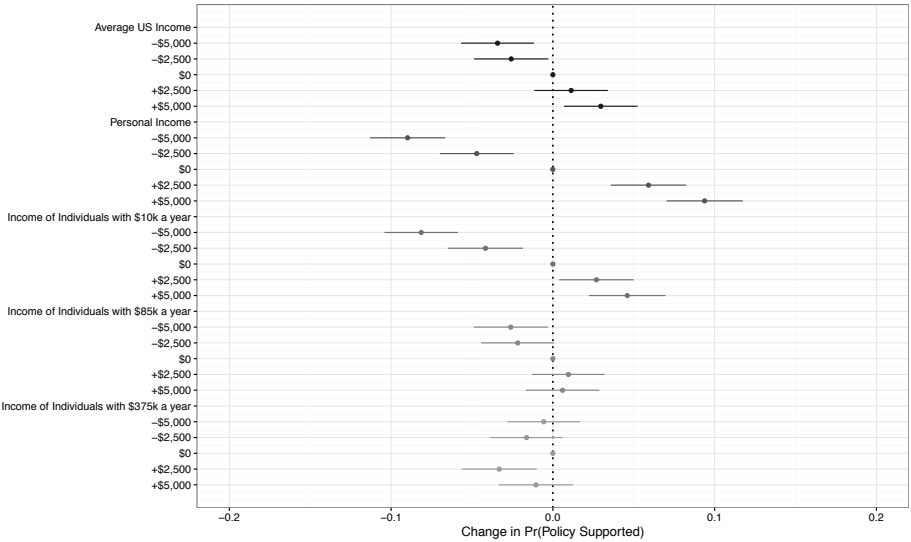
Note: The plot shows causal estimates of the effect of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support (Rating)* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: 5.54. N (respondents): 2,723, N (rated policies): 18,918.

Figure 3.C1: Causal Effects of Income Changes, Dependent Variable: Rating of Policy



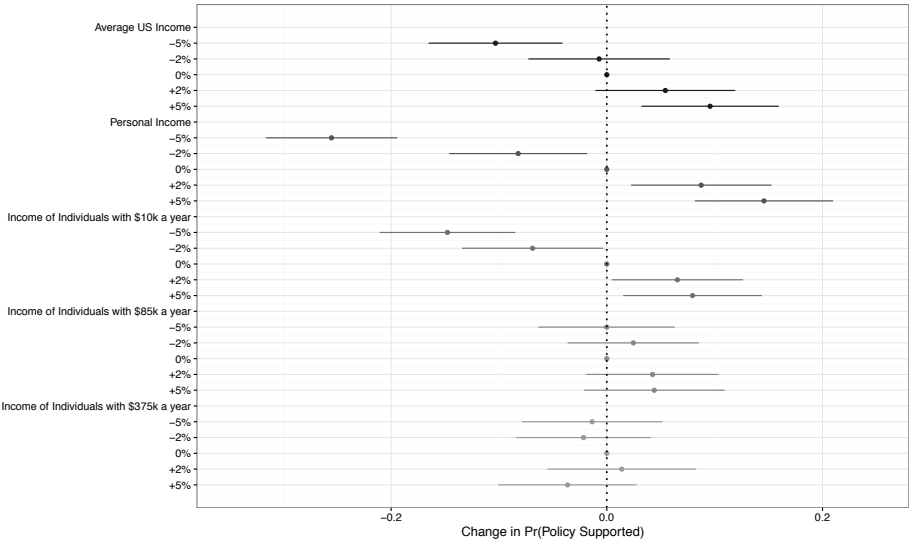
Note: The plot shows causal estimates of the effects of randomly assigned income changes on the rating of a policy on a ten-point scale. Estimates are based on a Tobit regression of *Reform Support (Rating)* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: 5.53. N (respondents) = 2,723, N (rated policies) = 18,918.

Figure 3.C2: Causal Effects of Income Changes, Dependent Variable: Rating of Policy (Tobit Estimates)



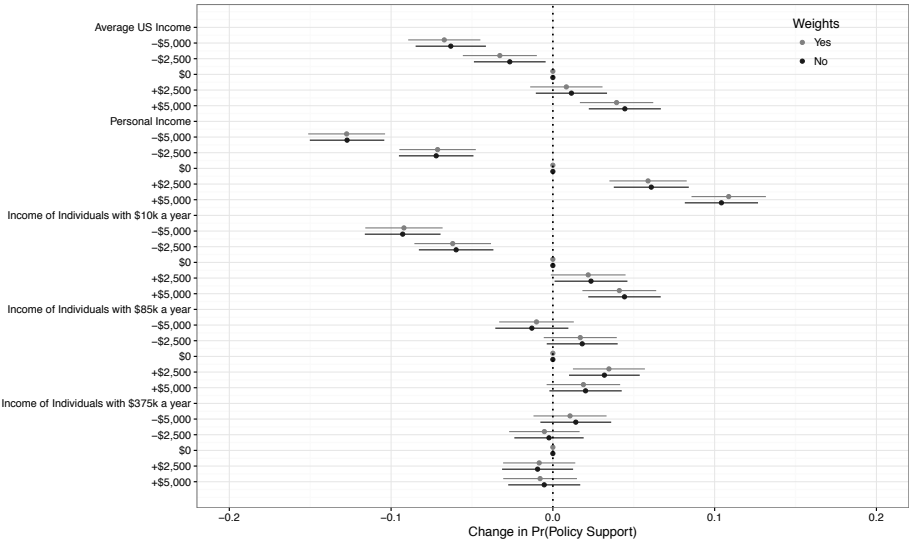
Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of the binary outcome variable *Reform Support (Rating)* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. The dependent variable equal 1 if a policy received a rating greater than 5 on a 1 to 10 scale and is 0 otherwise. Baseline level of support: .47. N (respondents): 2,723, N (rated policies): 18,918.

Figure 3.C3: The Causal Effect of Income Changes on Reform Support, Dependent Variable: Rating of Policy (Binary Measure)



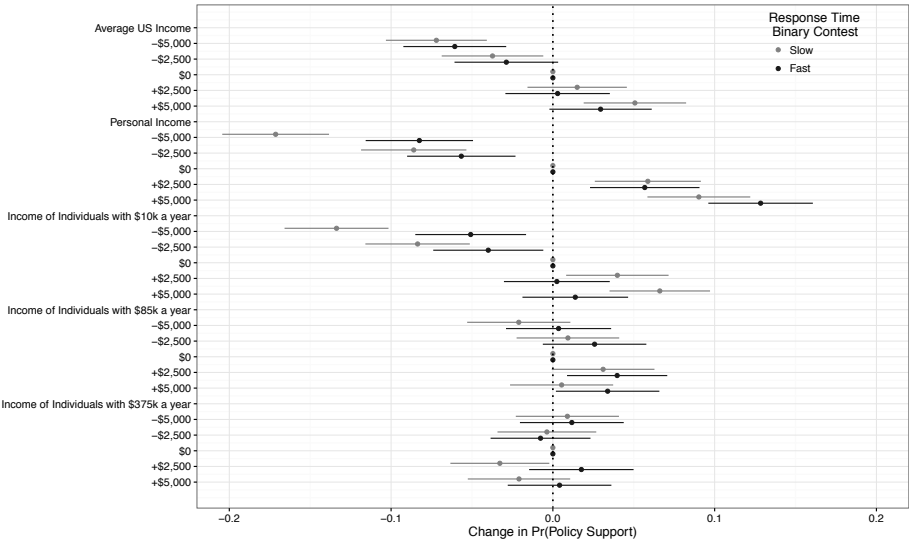
Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Results from study conducted on a sample of 300 Americans eligible to vote that were recruited on Amazon MTurk. Baseline level of support: .52. N (respondents): 307, N (rated policies): 2,222.

Figure 3.C4: The Causal Effect of Income Changes on Reform Support (Percentage Changes, Amazon MTurk Results)



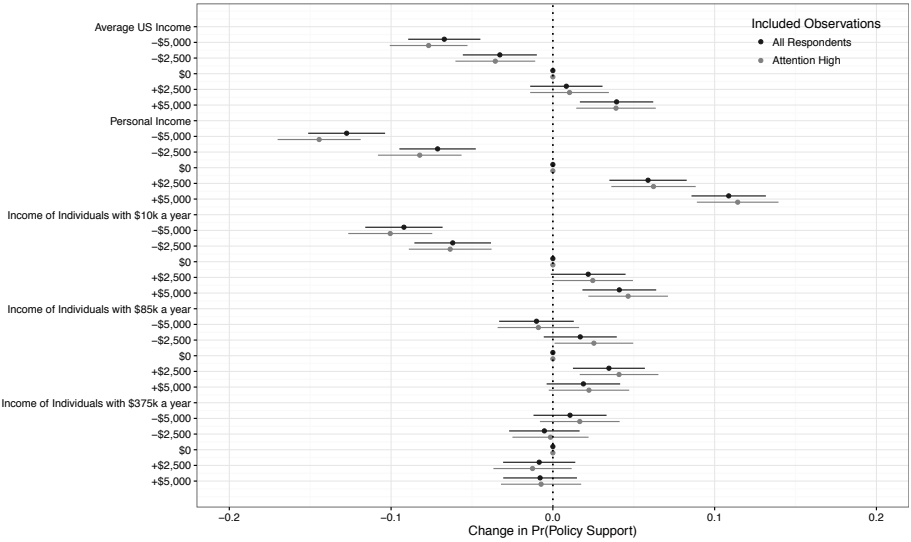
Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: Weights: .52; No weights: .52. Weights: N (respondents): 2,723, N (rated policies): 18,918. No weights: N (respondents): 2,723, N (rated policies): 18,918.

Figure 3.C5: The Causal Effect of Income Changes on Reform Support: Baseline Model with and without Weights



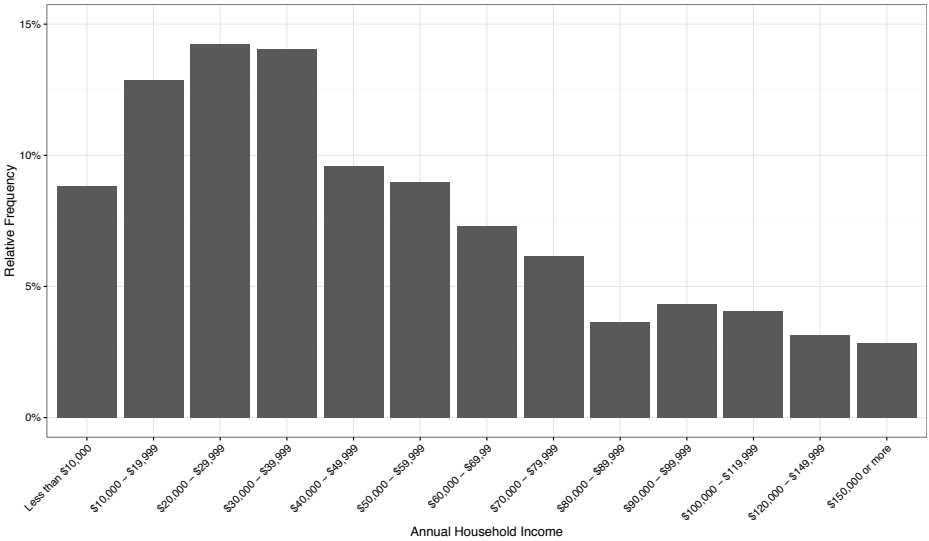
Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Categorization of response time based on time to submit page with decision in binary contest. Slow: Above median, Fast: Below and including median. Baseline level of support: Slow: .56, Fast: .49. Slow: N (respondents): 2,083, N (rated policies): 9,458. Fast: N (respondents): 2,075, N (rated policies): 9,460.

Figure 3.C6: The Causal Effect of Income Changes on Reform Support by Response Time



Note: The plot shows causal estimates of the effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Categorization of subgroups: All observations: Includes all observations, Attention: High: Only respondents that passed a screener questions. Baseline level of support: All observations: .52, Attention: High: .53. All observations: N (respondents): 2,723, N (rated policies): 18,918. Attention: High: N (respondents): 2,241, N (rated policies): 15,894.

Figure 3.C7: The Causal Effect of Income Changes on Reform Support by Attentiveness (Screener Question)



Note: This figure shows the distribution of respondent's annual household income (only consistent responses).

Figure 3.C8: Distribution of Respondents' Annual Household Income

Table 3.D1: Distribution of Sociodemographics in Survey Sample and Voter Population

Group	Voter Population	Weighted Sample	Raw Sample
Age: 18-24	12.3	12.32	13.42
Age: 25-44	32.5	32.52	34.17
Age: 45-64	34.7	34.69	34.23
Age: 65+	20.5	20.47	18.21
Gender: Male	48.2	48.20	48.06
Gender: Female	51.8	51.80	51.94
Education: Less than HS	9.5	9.47	6.70
Education: HS graduate	29.2	29.24	32.91
Education: Some college or associate degree	30.0	30.07	35.44
Education: Bachelor's degree	20.0	19.94	14.28
Education: Advanced degree	11.2	11.28	10.67

Note: The table shows the distributions of sociodemographics in the voter population, the weighted sample, and the raw sample. Source of data on voter population sociodemographics: 2016 Current Population Survey. Obtained via the Current Population Survey Table Creator, which can be accessed via `http://www.census.gov/cps/data/cpstablecreator.html`.

Table 3.D2: Summary Statistics of Covariates

	Mean	Median	Std.Dev.	Min	Max	N (rated policies)	N (respondents)
Altruism (Amount Donated)	15.28	0	24.21	0	100	18,918	2,723
Nationalism	3.16	3	1.44	1	5	18,800	2,704
Democrat	.57	1	.50	0	1	9,906	1,424
County Unemployment Rate	4.8	4.8	1.27	1.9	20.9	18,918	2,723
Industry Unemployment Rate	4.4	4.4	1.20	2.2	6.7	14,698	2,098
Employed	.80	1	.40	0	1	9,762	1,417
Conjoint Contest Response Time (Sec.)	61.40	27.38	675.82	10.36	55039.09	18,918	2,723

Note: The table reports descriptive statistics for all covariates used in the subgroup analyses. The variable 'Democrat' distinguishes between respondents that self-identified themselves as either Democrat or Republican.

Table 3.D3: The Causal Effects of Income Changes on Reform Support

Variables	Baseline	
Average US Income		
-$5,000	-0.067***	(0.011)
-$2,500	-0.033**	(0.012)
$2,500	0.008	(0.011)
$5,000	0.039***	(0.012)
Average Personal Income		
-$5,000	-0.127***	(0.012)
-$2,500	-0.071***	(0.012)
$2,500	0.059***	(0.012)
$5,000	0.109***	(0.012)
Income of Individuals earning $10k a year		
-$5,000	-0.092***	(0.012)
-$2,500	-0.062***	(0.012)
$2,500	0.022	(0.012)
$5,000	0.041***	(0.012)
Income of Individuals earning $85k a year		
-$5,000	-0.010	(0.012)
-$2,500	0.017	(0.012)
$2,500	0.035**	(0.011)
$5,000	0.019	(0.012)
Income of Individuals earning $375k a year		
-$5,000	0.011	(0.012)
-$2,500	-0.005	(0.011)
$2,500	-0.008	(0.011)
$5,000	-0.008	(0.012)
Constant	0.524***	(0.017)
N	18,918	
R^2	0.046	

Note: The table displays estimates of OLS regressions of *Reform Support* on the indicator variables listed in the left-most column. The reference category for each attributed is an income change of $0. Robust standard errors in parentheses. * p <0.5, ** p <0.01, *** p <0.001.

Table 3.D4: The Causal Effects of Average Income Changes on Reform Support by Altruism

	Altruism: Low		**Altruism: High**	
Average US Income Change				
-$5,000	-0.080***	(0.015)	-0.053**	(0.018)
-$2,500	-0.043**	(0.015)	-0.021	(0.018)
$2,500	-0.010	(0.015)	0.030	(0.018)
$5,000†	0.018	(0.015)	0.067***	(0.018)
Constant	0.530***	(0.022)	0.520***	(0.026)
N	10840		8078	
R^2	0.045		0.049	

Note: The table displays estimates of OLS regressions of *Reform Support* on the indicator variables listed in the left-most column for individuals with low and high levels of altruism. The reference category for each attributed is an income change of $0. Robust standard errors clustered by respondent in parentheses. * p <0.5, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "Altruism: High" with each indicator variable, the coefficient on the interaction with "Altruism: High" is significant at the 5%-level.

Table 3.D5: The Causal Effects of Average Income Changes on Reform Support by Nationalism

Variables	**Nationalism: Low**		**Nationalism: High**	
Average US Income Change				
-$5,000	-0.065***	(0.015)	-0.068***	(0.017)
-$2,500	-0.033*	(0.016)	-0.036*	(0.018)
$2,500	0.006	(0.015)	0.009	(0.018)
$5,000	0.031*	(0.015)	0.051**	(0.018)
Constant	0.526***	(0.022)	0.522***	(0.026)
N	10928		7872	
R^2	0.051		0.042	

Note: The table displays estimates of OLS regressions of *Reform Support* on the indicator variables listed in the left-most column for individuals with low and high levels of nationalism. The reference category for each attributed is an income change of $0. Robust standard errors clustered by respondent in parentheses. * p <0.5, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "National Identity: High" with each indicator variable, the coefficient on the interaction with "National Identity: High" is significant at the 5%-level.

Table 3.D6: The Causal Effects of Average Income Changes on Reform Support by Party Identification

Variables	**Democrat**		**Republican**	
Average US Income				
-$5,000	-0.068**	(0.021)	-0.063**	(0.024)
-$2,500	-0.039	(0.022)	-0.047	(0.024)
$2,500	-0.015	(0.022)	0.011	(0.022)
$5,000	0.036	(0.021)	0.021	(0.024)
Constant	0.518***	(0.031)	0.567***	(0.034)
N	5608		4298	
R^2	0.046		0.057	

Note: The table displays estimates of OLS regressions of *Reform Support* on the indicator variables listed in the left-most column for individuals that identify with Democrats and Republicans. The reference category for each attributed is an income change of $0. Robust standard errors clustered by respondent in parentheses. * p <0.5, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "Republican" with each indicator variable, the coefficient on the interaction with "Republican" is significant at the 5%-level.

Table 3.D7: The Causal Effects of Average Income Changes on Reform Support by Industry Unemployment Rate

Variables	**Industry UR: Low**		**Industry UR: High**	
Average US Income Change				
-$5,000	-0.067***	(0.016)	-0.068**	(0.022)
-$2,500	-0.041*	(0.016)	-0.018	(0.023)
$2,500	0.006	(0.016)	0.028	(0.021)
$5,000	0.031	(0.016)	0.064**	(0.023)
Constant	0.530***	(0.024)	0.523***	(0.031)
N	9468		5230	
R^2	0.055		0.053	

Note: The table displays estimates of OLS regressions of *Reform Support* on the indicator variables listed in the left-most column for individuals working in industries with low and high unemployment rates. The reference category for each attributed is an income change of $0. Robust standard errors clustered by respondent in parentheses. * p <0.5, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "Industry Unemployment Rate: High" with each indicator variable, the coefficient on the interaction with "Industry Unemployment Rate: High" is significant at the 5%-level.

Table 3.D8: The Causal Effects of Average Income Changes on Reform Support by County Unemployment Rate

Variables	**County UR: Low**		**County UR: High**	
Average US Income Change				
-$5,000	-0.056***	(0.016)	-0.079***	(0.017)
-$2,500	-0.022	(0.015)	-0.045*	(0.018)
$2,500	0.011	(0.015)	0.005	(0.017)
$5,000	0.053***	(0.015)	0.024	(0.018)
Constant	0.512***	(0.022)	0.537***	(0.026)
N	9842		9076	
R^2	0.051		0.041	

Note: The table displays estimates of OLS regressions of *Reform Support* on the indicator variables listed in the left-most column for individuals residing in counties low and high levels of unemployment The reference category for each attributed is an income change of $0. Robust standard errors clustered by respondent in parentheses. * p <0.5, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "County Unemployment Rate: High" with each indicator variable, the coefficient on the interaction with "County Unemployment Rate: High" is significant at the 5%-level.

Table 3.D9: The Causal Effects of Average Income Changes on Reform Support by Employment Status

Variables	**Employment Status: Unemployed**		**Employment Status: Paid Work**	
Average US Income Change				
-$5,000	-0.081*	(0.035)	-0.053**	(0.018)
-$2,500	0.011	(0.035)	-0.034	(0.018)
$2,500	0.014	(0.033)	0.016	(0.018)
$5,000	0.031	(0.036)	0.033	(0.018)
Constant	0.505***	(0.052)	0.504***	(0.025)
N	2010		7752	
R^2	0.048		0.046	

Note: The table displays estimates of OLS regressions of *Reform Support* on the indicator variables listed in the left-most column for individuals that are unemployed and in paid work. The reference category for each attributed is an income change of $0. Robust standard errors clustered by respondent in parentheses. * p <0.5, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "Occupational Status: Paid Work" with each indicator variable, the coefficient on the interaction with "Occupational Status: Paid Work" is significant at the 5%-level.

Appendix 3.E: Income Distribution of Respondents

Table 3.E1 shows the summary statistics of the variable yearly household income (for a graphical representation see Figure 3.C8). The right-most column lists whether an income category was coded as below or including the median category or above the median category for the binary measure used as binary measure to differentiate between respondents in Figure 3.5.

Table 3.E1: Summary Statistics of Respondent's Annual Household Income

Category	Abs. Freq.	Rel. Freq.	Cum. Freq.	Income Category (binary for conjoint)
Less than $10,000	1,668	8.84	8.84	Low
$10,000 - $19,999	2,428	12.87	21.70	Low
$20,000 - $29,999	2,688	14.24	35.95	Low
$30,000 - $39,999	2,648	14.03	49.98	Low
$40,000 - $49,999	1,812	9.60	59.58	Low
$50,000 - $59,999	1,696	8.99	68.57	High
$60,000 - $69,99	1,376	7.29	75.86	High
$70,000 - $79,999	1,164	6.17	82.03	High
$80,000 - $89,999	684	3.62	85.65	High
$90,000 - $99,999	820	4.35	90.00	High
$100,000 - $119,999	764	4.05	94.04	High
$120,000 - $149,999	590	3.13	97.17	High
$150,000 or more	534	2.83	100.00	High
Total	18,872	100.00		

Note: The table lists summary statistics of respondents' income for all consistent responses.

Chapter 4

Inequality and Redistribution Behavior

*Michael M. Bechtel, Roman Liesch, and Kenneth F. Scheve**

4.1 Introduction

Societies have always engaged in some degree of wealth redistribution to realize more equitable outcomes (Adams 1966; Hirth 1978). Yet, the massive rise in inequality over the past decades has by far surpassed increases in redistribution efforts (Piketty and Saez 2014; Scheve and Stasavage 2016). This seems paradoxical since democracies allow citizens to vote for more redistribution (Meltzer and Richard 1981). We propose an explanation that can reconcile these two facts asserting that humans vary in both their general tolerance to inequality and the extent to which they are averse to favorable or unfavorable distributions of wealth (Fehr and Schmidt 1999). We conjecture that the precise composition of societies in terms of these redistribution types will affect the degree of government intervention to counter rising inequality.

*We thank the audience at the University of Colorado Boulder. We gratefully acknowledge financial support from the Swiss National Science Foundation (grants #100017_146170/1 and #PP00P1-139035). Any errors remain our own.
This is a slightly adapted version of the actual manuscript prepared for this dissertation. Any errors resulting from typesetting the document are my own.

We propose an experimental approach to the study of redistribution that measures responses to inequality as revealed by human re-allocation behavior in representative samples of the adult population. We devised a randomized "give-or-take" experiment that varied the level of inequality between two individuals by raffling two Amazon gift cards among all respondents.[1] The two gift cards could take on three values, each corresponding to a different treatment condition. In the "own poorer" condition the values were \$/€25 (own) and \$/€75 (other). In the "own richer" condition the value of the gift cards was reversed (\$/€75, \$/€25). In the "equality" condition the gift cards were worth \$/€50 each. Respondents were then given the option to either give or take an amount to the other winner. Depending on their choice, they saw a slider that allowed them to give any amount up to all of the initial endowment to the other winner (if they chose give) or take any amount from the initial endowment of the other winner (if they chose take). We embedded this experiment in surveys conducted of representative samples of the adult population in the United States (N=2,749) and Germany (N=2,217).[2]

This design offers several advantages. First, the randomization of inequality ensures that any differences in individual's allocation choices can be causally attributed to exogenous differences in the initial values of their gift cards. Second, observing actual redistribution behavior that has, in expectations, monetary consequences for a respondent, we improve over measures that rely on stated preferences for redistribution. Third, the design allows us to distinguish between responses to advantageous (own richer) and disadvantageous (own poorer) inequality, a distinction that is important to explain both attitudes toward redistribution among the rich and the poor. Finally, by studying representative samples of the American and German adult population we can characterize the composition of these societies in terms of human responses to inequality.

[1] See Appendix 4.A for more information on the experiment.

[2] See Table 4.C1 in the Appendix for more information about the composition of the distribution of sociodemographics in the raw sample, the voter population, and the weighted sample.

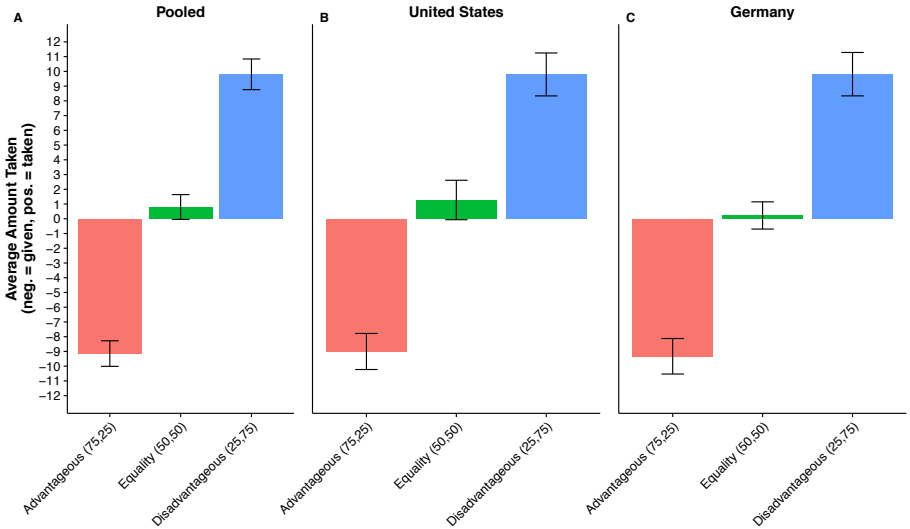
4.2 The Causal Effect of Inequality in the Give-or-Take Experiment

Figure 4.1 displays the effects of inequality on redistribution behavior as observed in the give-or-take experiment.[3] By comparing the average amount of money redistributed in each condition, we can measure the effect of advantageous inequality ("own richer") and disadvantageous inequality ("own poorer") on human redistribution behavior. We find that a-inequality ("own richer") generates a significant level of giving among respondents: On average, richer individuals give \$/€9 (12% of their own endowment) to the poorer respondent. Under conditions of equality, the amount re-allocated is statistically indistinguishable from zero. In contrast, d-inequality causes significant taking-behavior as individuals who are poorer take \$/€10 (13% of the other's endowment) from the other respondent.

When comparing Figure 4.1, B and C, we find little differences in how Germans and Americans re-allocate wealth in response to unequal initial distributions. These results suggest that inequality creates demand for the re-allocation of wealth, but the extent of redistribution does not fully remove inequality. We believe that the imperfect equalization of wealth reflects that some humans engage in re-allocation behavior that equalizes payoffs whereas others fall short of equalizing.

To explore behavioral differences in responding to inequality we asked respondents how much they would give or take conditional on different values of the other winner's initial gift card value (\$/€5, \$/€15, \$/€25, \$/€50, \$/€75, \$/€85, \$/€95) while keeping the initial value of the respondent's gift card, which was randomly assigned to be either (\$/€25, \$/€50, or \$/€75), constant (Rauhut and Winter 2010).[4] This provides us with 4,966 individual redistribution schedules that say how much and in which direction each individual would redistribute given a specific distribution of wealth, which here is understood as differences in the value of the two Amazon gift cards.

[3] Table 4.C3 in the Appendix reports those results in detail.
[4] See Appendix 4.A for more information.



Note: This figure shows the average amount taken/given in $/€ in response to advantageous (a-)inequality ("own richer"), equality, and disadvantageous (d-)inequality ("own poorer") in the (**A**) pooled data, the (**B**) United States and (**C**) Germany. Error bars indicate 95% confidence interval calculated from robust standard errors. All differences are significant (P<.001). N(total)=4,966. N(United States)=2,749, N(Germany)=2,217.

Figure 4.1: Average Amounts Taken by (In-)equality Conditions

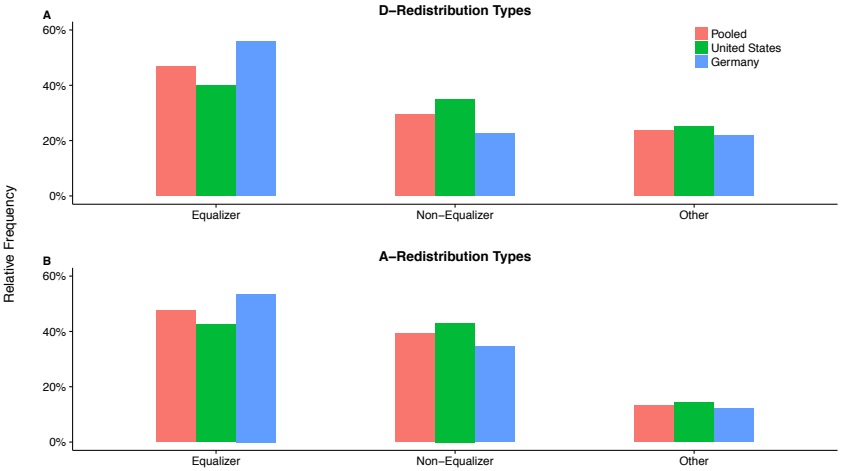
4.3 Classifying Redistribution Types

To classify individuals based on their redistribution behavior we regress the redistributed amount on the difference in the Amazon gift cards separately for scenarios in which an individual was richer than the other (advantageous or a-inequality) and scenarios in which an individual was poorer than the other (disadvantageous or d-inequality).[5] The estimated coefficient provides us with a measure of the extent to which an individual gives or takes as a function of differences in wealth. We code respondents based on how their give-or-take responses change if they are richer than the other (a-inequality) and if they are poorer (d-inequality) and distinguish between three types: *Equalizers* tend to re-allocate an amount that roughly leads to an equal distribution of wealth as measured by the final values of the two Amazon gift cards, i.e., on average, humans classified as equalizers have an elasticity of 0.5. *Non-Equalizers* do not or only very mildly redistribute wealth. On average, their sensitivity to inequality is estimated at 0. These two groups comprise the vast majority of individuals (over 70%). We also form a residual category of *Other* that includes individuals that employ other allocation rules. We use this classification to code individuals along the two dimensions of inequality: The d-inequality dimension captures how individuals respond to disadvantageous inequality and the a-inequality dimension measures how respondents react to advantageous inequality.

Figure 4.2 shows the share of redistribution types in our representative samples distinguishing between d-inequality and a-inequality.[6] Overall, about 45% of the population are Equalizers, 30% are Non-Equalizers and 25% are classified as Others based on their re-allocation behavior in response to d-inequality. When coding responses to advantageous inequality, we find 45% Equalizers, 40% Non-Equalizers, and 15% Other. Consistent with common wisdom, the share of equalizers on both dimensions is significantly greater in Germany (d-inequality: 55%, a-inequality: 50%) than in the United States (d-inequality: 40%, a-inequality: 42%). Additional analyses (Figure 4.3) suggest that d-Equalizers – individuals that tend to take an amount that equalizes payoffs when they are poorer – are more likely to be female, older, and either ideologically left or right (as opposed

[5] See Section 'Coding of Types' in Appendix 4.A for more details.
[6] See Table 4.C5 in the Appendix for detailed results.

to center).[7] A-Equalizers – individuals that tend to give an amount to the other that equalizes payoffs if they are richer – are more likely female, younger and ideologically left.[8] This suggests that redistribution types are concentrated in specific socio-demographic environments.[9]

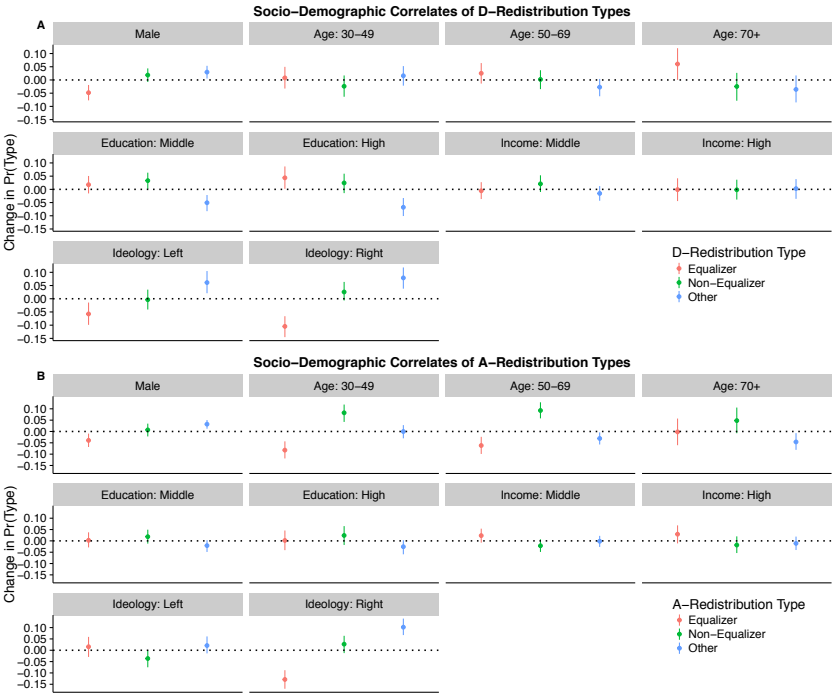


Note: Types are defined using coefficients resulting from separate auxiliary regressions of the amount given in the give or take game and the strategy method on Δpoorer (initial gift card value of other - initial own gift card value) for a-inequality and Δricher (initial own gift card value - initial gift card value of other) for d-inequality where negative amounts for Δpoorer and Δricher are replaced with 0. We multiply the corresponding coefficient for Δpoorer by -1 for classification purposes. Types are classified as follows (d-inequality and a-inequality types): $-.25 \leq$ coefficient $< .25$: Non-Equalizer, $.25 \leq$ coefficient $< .75$: Equalizer, all other values: Other. N(total)=4,966. N(United States)=2,749, N(Germany)=2,217.

Figure 4.2: Classification of Redistribution Types Based on (**A**) D-Inequality (Disadvantageous Inequality) and (**B**) A-Inequality (Advantageous Inequality) for the Pooled Dataset, the United States, and Germany

The classification we propose relies on human behavior as displayed in a highly simplified, two-member society. To evaluate whether this typology has the potential to explain real-world differences in individuals' policy opinions, we explore

[7] See Table 4.C13 in the Appendix.
[8] Table 4.C14 in the Appendix reports those results in detail.
[9] Tables 4.C15 and 4.C16 report the multinomial regression results without ideology and yield similar results.



Note: The figure shows how the probability of a specific redistribution type responds to a change in socio-demographic variables together with 95% confidence intervals computed from heteroskedasticity-robust standard errors. The simulations are based on results from a multinomial logistic regression and were implemented using Clarify (King, Tomz, and Wittenberg 2000). For male the simulated change is from female to male respondent. For all age variables the reference group is age $<$ 30. For all education variables the reference group is Education: Low. For all income variables the reference group is Income: Low. For all Ideology variables the reference group is Ideology: Center. N=4,925.

Figure 4.3: Socio-Demographic Correlates of Redistribution Types for **(A)** D-Inequality and **(B)** A-Inequality in the United States and Germany (Pooled Data)

the correlation between redistribution type and attitudes toward two important types of government redistribution: Imposing heavy taxes on the rich and avoiding welfare spending cuts.[10] Figure 4.4 A shows results from a linear regression of individuals' policy views as measured on a five-point agree-disagree scale on Equalizer type using Non-Equalizers as the reference group.[11] We observe that d-Equalizers are on average significantly more likely to support heavy taxes on the rich than Non-Equalizers. In contrast, there exists no statistically discernible difference between those two groups when investigating support for upholding current levels of welfare spending. This correlational pattern adds to the validity of our distinction between d-inequality and a-inequality: Since the behavior we observe under conditions of disadvantageous inequality captures aversion to others being richer, d-Equalizers should support policies that aim to reduce the wealth concentration among the rich, but not necessarily advocate the provision of benefits meant to make the poorest better off. Consistent with this reasoning, Figure 4.4 B reveals that our classification of redistribution behavior in response to a-inequality predicts support for avoiding welfare spending cuts. Again, as one would expect, a-Equalizers and a-Non-Equalizers do not differ significantly on their support for high taxes on the rich. This pattern suggests that distinguishing between a-inequality and d-inequality improves our ability to explain differences in support for government redistribution.[12]

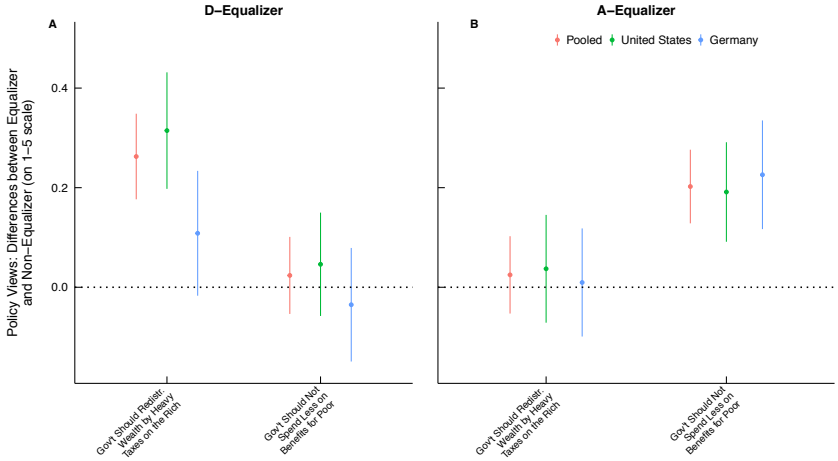
4.4 Conclusion

Clearly, the give-or-take experiment and the setting in which it was embedded strongly simplifies the decision-making process that leads to government redistribution in democracies. First, our setting created "mini"-societies in which re-allocation was costless. In the real world, redistribution requires bureaucratic effort and these costs reduce the resources available for re-allocation. Second, we did not specify the process that generated the initial distribution of wealth. Arguably, the extent to which individuals believe that the unequal distribution of

[10] See Table 4.C2 for more information about the exact question wording and the covariates.

[11] Table 4.C17 in the Appendix presents the full results for the pooled dataset and Table 4.C19 for the US and Germany.

[12] Tables 4.C21 and 4.C23 in the Appendix show that the results remain reassuringly similar using the alternative coding scheme explained in Section 'Coding of Types' in Appendix 4.A.



Note: The figure shows marginal effects of d-equalizer and a-equalizer on policy views in comparison to non-equalizer. Policy Views are measured using a five-point scale (strongly disagree - strongly agree). Dots with vertical lines indicate point estimates with robust 95% confidence intervals from ordinary (weighted) least squares regressions of policy views on d-redistribution and a-redistribution types, own initial endowment, socio-demographics, ideology, and a dummy indicator for Germany (pooled specification only). N(total) = 4,921, N(United States) = 2,733, N(Germany) = 2,188.

Figure 4.4: Marginal Effect of (**A**) D-Equalizer and (**B**) A-Equalizer on Policy Views Compared to Non-Equalizers in the Pooled Data, the United States, and Germany

wealth reflects differences in effort as opposed to luck will have an impact on their willingness to redistribute (Alesina and La Ferrara 2005). Third, we did not specify the social identity of the other winner to whom the individual could give to or take from. To the extent that humans are characterized by differential altruism and treat in-group and out-group members differently, we might expect variation in redistributive behavior conditioned on social heterogeneity (Alesina, Glaeser, and Sacerdote 2001). Fourth, we deliberately removed strategic considerations by allowing only one individual to change the distribution of wealth. However, beliefs about how others will respond to having to pay higher taxes or receiving larger social benefits will likely affect the willingness to support government intervention meant to reduce economic inequality. We anticipate that our design will be useful to integrate these factors to improve knowledge about the origins of redistribution.

Appendix 4.A: Materials and Methods

Survey Programming and Sample

We programmed and hosted the survey in Qualtrics. The online sample was provided by Respondi. Respondi maintains an own actively managed online samples that employ a combination of online and offline recruitment methods to ensure that their panel can be used for conducting representative surveys (Respondi, n.d.).

We provided Respondi with margins for socio-demographics (age, gender, education) derived from population censuses to ensure that our samples match the population margins in the United States and Germany. To remove any remaining imbalances we weighted the data using the ebalance-algorithm (Hainmueller and Xu 2013). Table 4.C1 provides information about the distribution of socio-demographic characteristics in the raw sample, the weighted sample, and the voter population. Unless indicated otherwise, all analyses use weighted data.

Give-or-Take Game and Redistribution Behavior in the Strategy

Method

The survey contained two parts to elicit individuals' aversion to advantageous (a) and disadvantageous (d) inequality. The first part was the payoff-relevant "give or take" experiment which was explicitly payoff-relevant. We provided respondents with the game instructions (see Figure 4.B1 for an example). The game was based on the option of winning one of two Amazon gift cards. The initial values of these gift cards could vary. We informed respondents about the initial value of their gift card that he/she could win and the other winner's gift card. We randomized these initial values ($ in the United States and € in Germany) to be (respondent/other winner): (25/75), (50/50), or (75/25) and informed respondents that they could increase or decrease these values by choosing to give to or take from the other winner. If a respondent chose to give, the amount would be deducted from his/her initial gift card value and added to the other winner's gift card. If a respondent decided to take, the amount would be deducted from the other winner's gift card

and added to his/her own gift card. We illustrated these two options with an example. The experiment randomized the order in which the two options were displayed.

On the screen that followed, respondents were again shown the initial gift card values and asked whether they wanted to give, take or do nothing (see Figure 4.B2). We randomized the order of the answer options "give" and "take". Respondents could use a slider to give any amount up to all of their entire initial endowment to the other winner (if they decided to give) or take any amount up to the entire initial endowment of the other winner (if they decided to take). Respondents were shown in real time the final values of both gift cards depending on the current slider position (see Figure 4.B3). Individuals could redistribute any amount as long as the resulting value of the two vouchers was zero or positive.

The second part relied on the strategy method to elicit respondents' redistribution schedules. We did not inform respondents that this part of the survey was no longer payoff-relevant. The exact question wording for the first question was:

> "Now, suppose that the initial value of your gift card was the same but the initial value of the other winner's gift card was different. Please indicate how much you would like to either take from or give to the other winner if the initial value of the gift cards is as follows:
>
> You: \$75
>
> Other: \$5"

We repeated this question (using a slightly shorter version) with the initial value of the respondent's gift card remaining constant while the other winner's initial gift card took on each of the following initial values: 15, 25, 50, 75, 85, and 95 \$/€. Respondents were not asked again how much they would redistribute if the other winner's initial gift card value was equal to the initial value in the first part of the give-or-take game. Below each question was a slider that ranged from the maximum amount a respondent could take to the maximum amount a person could give. The resulting final values of the gift cards were shown in real time depending on the slider position. This provides us with a redistribution schedule for each respondent that says how much a person redistributes conditional on the direction and level of inequality.

Coding of Types

We estimate each respondent's sensitivity to a- and d-inequality using their conditional redistribution behavior in combination with individual-level auxiliary regressions. The dependent variable is *Given* which measures the amount given (positive values) or taken in each of the proposed scenarios. Our independent variables are: ΔPoorer, which is the difference between the other respondent's initial gift card value and the respondent's own initial gift card value. The second independent variable is ΔRicher. This variable equals the difference between one's own initial gift card value and the gift card value of other respondent.

To derive individual-level sensitivities to a- and d-inequality, we estimate two auxiliary regressions for each respondent. To estimate d-sensitivity (the elasticity of redistribution behavior to disadvantageous inequality) we regress for each respondent the amount given on ΔPoorer using all observations for which ΔPoorer ≥ 0 and multiply the estimated coefficient by -1. To estimate a-sensitivity (the elasticity of redistribution behavior to advantageous inequality), we regress for each respondent the amount given on ΔRicher using all observations for which ΔRicher ≥ 0.

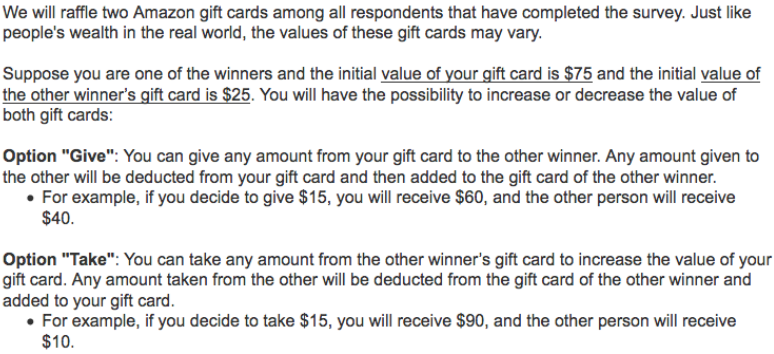
We use the estimated coefficients (d-sensitivity and a-sensitivity) to classify individuals' redistribution types. We use the following coding rule where "sensitivity" refers to the estimated coefficient:

- Equalizer: .25 $\leq$ sensitivity $<$ .75
- Non-Equalizer: -.25 $\leq$ sensitivity $<$ .25
- Other: All remaining cases.

To assess the sensitivity of our results to these coding rules, we develop an alternative coding scheme that changes the intervals that identify Equalizers and Non-Equalizers:

- Equalizer$_{Alt}$: .25 $\leq$ sensitivity $<$ .6
- Non-Equalizer$_{Alt}$: -.1 $\leq$ sensitivity $<$ .25
- Other$_{Alt}$: All remaining cases.

Appendix 4.B: Appendix Figures


We will raffle two Amazon gift cards among all respondents that have completed the survey. Just like people's wealth in the real world, the values of these gift cards may vary.

Suppose you are one of the winners and the initial value of your gift card is $75 and the initial value of the other winner's gift card is $25. You will have the possibility to increase or decrease the value of both gift cards:

Option "Give": You can give any amount from your gift card to the other winner. Any amount given to the other will be deducted from your gift card and then added to the gift card of the other winner.

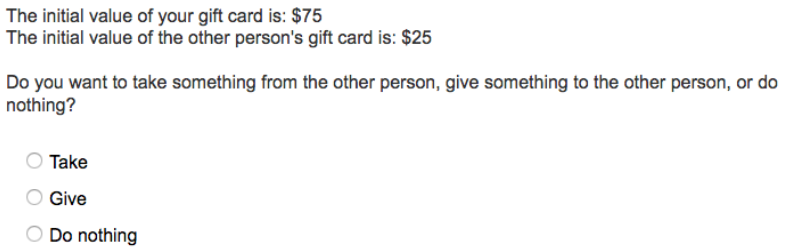
- For example, if you decide to give $15, you will receive $60, and the other person will receive $40.

Option "Take": You can take any amount from the other winner's gift card to increase the value of your gift card. Any amount taken from the other will be deducted from the gift card of the other winner and added to your gift card.

- For example, if you decide to take $15, you will receive $90, and the other person will receive $10.

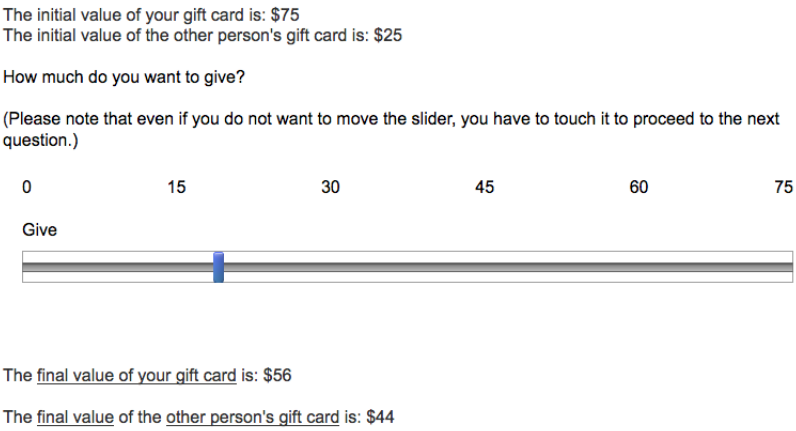
Note: This picture shows the screen that respondents saw as introduction text to the give and take experiment. In the example, the initial value of the respondent's gift card is $75 and that of the other person $25. The experiment randomized the order in which the options "Take" and "Give" were displayed and the amount given/taken in the examples was always $15.

Figure 4.B1: Screenshot of Give-or-Take Game: Instructions



Note: This picture shows the screen that respondents saw when they where asked whether they wanted to take, give, or do nothing. In the example, the initial value of the respondent's gift card is \$75 and that of the other person \$25. The experiment randomized the order in which the options "Take" and "Give" were displayed but always showed "Do nothing" as last option.

Figure 4.B2: Screenshot of Give-or-Take Game: Decision Whether to Take or Give



Note: The picture shows the screen respondents saw that chose to give in the 75$ own/25$ other condition. Respondents could use the slider to indicate the amount they wanted to give to the other person. The final values of the gift cards were updated in real time as a function of the slider position and were displayed to respondents below the slider. The range of the slider in the experiment varied and was chosen so that each respondent could take or give (depending on the choice made in the question displayed in Figure 4.B2) the maximum amount possible depending on the initial values of the gift cards. The custom start position of the slider was set to 0 and individuals that chose to do nothing in the previous screen skipped this part.

Figure 4.B3: Screenshot of Give-or-Take Game: Giving in the $75/$25-Condition

Appendix 4.C: Appendix Tables

Table 4.C1: The Causal Effect of Inequality: Amounts Taken in the Give-or-Take Game

	Population (%)	Weighted Sample (%)	Raw Sample (%)
United States (N=2,749)			
Age: 18-24	12.30%	12.34%	14.26%
Age: 25-44	32.50%	32.54%	34.99%
Age: 45-64	34.70%	34.67%	33.32%
Age: 65+	20.50%	20.45%	17.43%
Gender: Male	48.20%	48.22%	48.96%
Gender: Female	51.80%	51.78%	51.04%
Education: Less than high school degree	9.50%	9.46%	6.88%
Education: High school degree	29.20%	29.26%	32.96%
Education: Some college	30.00%	30.08%	34.78%
Education: Bachelor's degree	20.00%	19.92%	14.44%
Education: Advanced degree	11.20%	11.28%	10.94%
Germany (N=2,217)			
Age: 18-24	8.60%	8.59%	7.67%
Age: 25-44	27.50%	27.51%	29.27%
Age: 45-64	37.00%	37.00%	36.18%
Age: 65+	26.90%	26.90%	26.88%
Gender: Male	48.40%	48.43%	51.20%
Gender: Female	51.60%	51.57%	48.80%
Education: High school lowest tier	43.80%	43.62%	28.06%
Education: High school medium tier	25.70%	25.98%	44.79%
Education: High school high tier	14.50%	14.45%	12.00%
Education: University/College	16.10%	15.95%	15.15%

Note: The table shows the distributions of socio-demographics in the population, the weighted sample, and the raw sample. The population socio-demographics are taken from the following sources: United States: 2016 Current Population Survey, obtained from the Current Population Survey Table Creator, which can be accessed via `http://www.census.gov/cps/data/cpstablecreator.html`. Germany: German Statistical Office, 2011 Population Census (Statistisches Bundesamt, 2016) and data on education was taken from Bechtel, Hainmueller, and Margalit (2014).

Table 4.C2: Measurement and Coding of Variables

Amount Taken	Amount taken in the give-or-take game (in $/€) explained above. Amounts taken are positive, amounts given negative.
Government should redistribute wealth by heavy taxes on the rich	Based on the question "To what extent do you agree or disagree with the following statements:" The exact wording of the item was "The government should redistribute wealth by heavy taxes on the rich." We measured respondents' attitude towards this statement on a scale of 1 (strongly disagree) to 5 (strongly agree). We randomized the polarity of the answer scale and adapted the question text accordingly.
Government should not spend less on benefits for the poor	Based on the question "To what extent do you agree or disagree with the following statements:" The exact wording of the item was "The government should spend less on benefits for the poor." We measured respondents' attitude towards this statement on a scale of 1 (strongly disagree) to 5 (strongly agree) and recoded the answers to invert the item and the answer scale. We randomized the polarity of the answer scale and adapted the question text accordingly.
Male	Self-reported gender. Coded into binary variable where 1 equals male and 0 female.
Age	Self-reported age. Recoded into the categories 18-29, 30-49, 50-69, and 70+.
Income	Self-reported household income. Recoded into income: low (income in the lowest quartile), income: middle (interquartile range), and income: high (income in the upper quartile) with unweighted data.

Table 4.C2 (continued)

Education	Self reported highest level of education achieved. US respondents were coded into the following categories: low: up to and including high school degree or equivalent, middle: Some college (1-4 years, no degree) and associate's degree (including occupational degree), high: Bachelor's degree and advanced degrees. German respondents were coded into the following categories: low: up to and including secondary school leaving certificate (Haupt-(Volks-)schulabschluss), middle: polytechnic secondary school of the former GDR (Abschluss polytechnische Oberschule der DDR), intermediate secondary school or similar degree (Realschul- oder gleichwertiger Abschluss), university of applied sciences entrance qualification (Fachhoschulreife), higher education entrance qualification (Abitur), and vocational education (Berufsausbildung), high: university of applied science degree (Fachhochschulabschluss) and university degree.
Ideology	Self-reported placement on left-right-scale (0-10). The question wording was: "In politics people sometimes talk of "left" and "right". Where would you place yourself on this scale, where 0 means the left and 10 means the right?" Recoded into the categories Ideology left (0-2), ideology: center (3-7), and ideology: right (8-10).

Table 4.C2 (continued)

Employment Status	Self-reported employment status. The question wording was: "Which of these descriptions best describes your situation (in the last seven days)?" Answer categories included for the United States: "In paid work or away temporarily (employee, self-employed, working for your family business)", "In education, (not paid for by employer) even if on vacation", "Unemployed and actively looking for a job", "Unemployed, wanting a job but not actively looking for a job", "Permanently sick or disabled", "Retired", "In community service", "In military service", "Doing housework, looking after children or other persons", "Don't know", and "None of these". Answer categories for Germany: "Paid Work", "Doing Apprenticeship", "Unemployed and actively looking for job", "Unemployed, wanting a job but not actively looking for job", "Mini- and Midi-Job", "Unemployed at the moment", "Short-time work at the moment", "Retired", "Housework", "Military, community service, voluntary social year", "In school". "Studying", "Not able to work", "Don't know", "No Answer". Recoded into employed, unemployed, retired, in education, and other.

Note: This table describes the variables and variable codings.

Table 4.C3: The Causal Effect of Inequality: Amounts Taken in the Give-or-Take Game

	(1) Pooled	(2) United States	(3) Germany
Disadvantageous Inequality	9.80***	9.79***	9.81***
	(0.53)	(0.74)	(0.75)
Equality	0.80*	1.27*	0.22
	(0.43)	(0.68)	(0.47)
Advantageous Inequality	-9.15***	-9.00***	-9.33***
	(0.44)	(0.62)	(0.61)
Observations	4,966	2,749	2,217
R-squared	0.15	0.126	0.198

Note: This table reports linear regressions of amounts taken on the initial type of inequality for the pooled dataset (model 1), the United States (model 2), and Germany (model 3) using survey weights. Amounts taken are measured by individual's amount taken in the give-or-take game (taken: positive, given: negative). The initial type of inequality is coded as follows (respondent's initial gift card value in \$/€ / other's initial gift card value in \$/€): (25/75): Disadvantageous Inequality, (50/50): Equality, (75/25) Advantageous Inequality. Robust standard errors reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C4: The Causal Effect of Inequality: Amounts Taken in the Give-or-Take Game Without Survey Weights

	(1) Pooled	(2) United States	(3) Germany
Disadvantageous Inequality	9.95***	10.00***	9.90***
	(0.51)	(0.75)	(0.69)
Equality	0.87**	1.25*	0.40
	(0.42)	(0.67)	(0.48)
Advantageous Inequality	-9.22***	-9.08***	-9.41***
	(0.43)	(0.62)	(0.57)
Observations	4,966	2,749	2,217
R-squared	0.152	0.128	0.198

Note: This table reports linear regressions of amounts taken on the initial type of inequality for the pooled dataset (model 1), the United States (model 2), and Germany (model 3) using survey weights. Amounts taken are measured by individual's amount taken in the give-or-take game (taken: positive, given: negative). The initial type of inequality is coded as follows (respondent's initial gift card value in \$/€ / other's initial gift card value in \$/€): (25/75): Disadvantageous Inequality, (50/50): Equality, (75/25) Advantageous Inequality. Robust standard errors reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C5: Frequency of Redistribution Types

D-Redistribution Type	Pooled	USA	Germany
Equalizer	46,93%	39.79%	55.79%
Non-Equalizer	29.43%	35.04%	22.47%
Other	23.63%	25.16%	21.74%
Total	100.00%	100.00%	100.00%
A-Redistribution Types	**Pooled**	**USA**	**Germany**
Equalizer	47.48%	42.60%	53.54%
Non-Equalizer	39.23%	43.06%	34.48%
Other	13.29%	14.34%	11.98%
Total	100.00%	100.00%	100.00%

Note: This table reports the distribution of d- and a-redistribution types in the sample (with weights) separately for the pooled dataset, the United States, and Germany. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Poorer} \geq 0$ (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Richer} \geq 0$. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: $-.25 \leq$ sensitivity $< .25$: Non-Equalizer, $.25 \leq$ sensitivity $< .75$: Equalizer, all other values: Other. N(total)=4,966. N(United States)=2,749, N(Germany)=2,217.

Table 4.C6: Frequency of Redistribution Types Without Weights

D-Redistribution Type	Pooled	USA	Germany
Equalizer	47.08%	39.83%	56.07%
Non-Equalizer	29.42%	34.96%	22.55%
Other	23.50%	25.21%	21.38%
Total	100.00%	100.00%	100.00%
A-Redistribution Type	**Pooled**	**USA**	**Germany**
Equalizer	47.58%	42.74%	53.59%
Non-Equalizer	39.05%	42.78%	34.42%
Other	13.37%	14.48%	12.00%
Total	100.00%	100.00%	100.00%

Note: This table reports the distribution of d- and a-redistribution types in the sample (without weights) separately for the pooled dataset, the United States, and Germany. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Poorer} \geq 0$ (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Richer} \geq 0$. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: $-.25 \leq$ sensitivity $< .25$: Non-Equalizer, $.25 \leq$ sensitivity $< .75$: Equalizer, all other values: Other. N(total)=4,966. N(United States)=2,749, N(Germany)=2,217.

Table 4.C7: Joint Distribution of D-Redistribution and A-Redistribution Types in the Pooled Sample

		A-Redistribution Type			
		Equalizer	*Non-Equalizer*	*Other*	*Total*
D-Redistribution Type	*Equalizer*	30.38%	13.50%	3.06%	46.93%
	Non-Equalizer	10.25%	16.93%	2.26%	29.43%
	Other	6.86%	8.81%	7.97%	23.63%
	Total	47.48%	39.23%	13.29%	100.00%

Note: This table reports the joint distribution of d- and a-redistribution types in the sample (with weights) for the pooled dataset. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Poorer} \geq 0$ (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Richer} \geq 0$. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: $-.25 \leq$ sensitivity $< .25$: Non-Equalizer, $.25 \leq$ sensitivity $< .75$: Equalizer, all other values: Other. N=4,966.

Table 4.C8: Joint Distribution of D-Redistribution and A-Redistribution Types in the Pooled Sample Without Weights

		A-Redistribution Type			
		Equalizer	*Non-Equalizer*	*Other*	*Total*
D-Redistribution Type	*Equalizer*	30.31%	13.49%	3.28%	47.08%
	Non-Equalizer	10.33%	16.87%	2.22%	29.42%
	Other	6.95%	8.68%	7.87%	23.50%
	Total	47.58%	39.05%	13.37%	100.00%

Note: This table reports the joint distribution of d- and a-redistribution types in the sample (without weights) for the pooled dataset. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where ΔPoorer ≥ 0 (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where ΔRicher ≥ 0. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: -.25 $\leq$ sensitivity $<$ .25: Non-Equalizer, .25 $\leq$ sensitivity $<$ .75: Equalizer, all other values: Other. N=4,966.

Table 4.C9: Joint Distribution of D-Redistribution and A-Redistribution Types in the United States

		A-Redistribution Type			
		Equalizer	*Non-Equalizer*	*Other*	*Total*
D-Redistribution Type	*Equalizer*	24.55%	12.58%	2.67%	39.79%
	Non-Equalizer	11.40%	21.10%	2.54%	35.04%
	Other	6.64%	9.38%	9.13%	25.16%
	Total	42.60%	43.06%	14.34%	100.00%

Note: This table reports the joint distribution of d- and a-redistribution types in the sample (with weights) for US respondents. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where ΔPoorer ≥ 0 (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where ΔRicher ≥ 0. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: -.25 $\leq$ sensitivity $<$ .25: Non-Equalizer, .25 $\leq$ sensitivity $<$ .75: Equalizer, all other values: Other. N=2,749.

Table 4.C10: Joint Distribution of D-Redistribution and A-Redistribution Types in the United States Without Weights

		A-Redistribution Type			
		Equalizer	*Non-Equalizer*	*Other*	*Total*
D-Redistribution Type	*Equalizer*	24.41%	12.59%	2.84%	39.83%
	Non-Equalizer	11.53%	20.92%	2.51%	34.96%
	Other	6.80%	9.28%	9.13%	25.21%
	Total	42.74%	42.78%	14.48%	100.00%

Note: This table reports the joint distribution of d- and a-redistribution types in the sample (without weights) for US respondents. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Poorer} \geq 0$ (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where $\Delta\text{Richer} \geq 0$. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: $-.25 \leq$ sensitivity $< .25$: Non-Equalizer, $.25 \leq$ sensitivity $< .75$: Equalizer, all other values: Other. N=2,749.

Table 4.C11: Joint Distribution of D-Redistribution and A-Redistribution Types in Germany

		A-Redistribution Type			
		Equalizer	*Non-Equalizer*	*Other*	*Total*
D-Redistribution Type	*Equalizer*	37.60%	14.64%	3.55%	55.79%
	Non-Equalizer	8.81%	11.75%	1.91%	22.47%
	Other	7.13%	8.09%	6.52%	21.74%
	Total	53.54%	34.48%	11.98%	100.00%

Note: This table reports the joint distribution of d- and a-redistribution types in the sample (with weights) for German respondents. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where ΔPoorer ≥ 0 (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where ΔRicher ≥ 0. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: -.25 $\leq$ sensitivity $<$ .25: Non-Equalizer, .25 $\leq$ sensitivity $<$ .75: Equalizer, all other values: Other. N=2,217.

Table 4.C12: Joint Distribution of D-Redistribution and A-Redistribution Types in Germany Without Weights

		A-Redistribution Type			
		Equalizer	*Non-Equalizer*	*Other*	*Total*
D-Redistribution Type	*Equalizer*	37.62%	14.61%	3.83%	56.07%
	Non-Equalizer	8.84%	11.86%	1.85%	22.55%
	Other	7.13%	7.94%	6.31%	21.38%
	Total	53.59%	34.42%	12.00%	100.00%

Note: This table reports the joint distribution of d- and a-redistribution types in the sample (without weights) for the German respondents. Coding of types based on individual redistribution behavior in the give-or-take game and the strategy method part where we kept the initial value of the respondent's gift card constant while varying the other winner's initial gift card value. Types are defined using coefficients resulting from auxiliary regressions of the amount given in the give-and-take game and the strategy method on ΔPoorer (initial gift card value of other - initial own gift card value) for all observations where ΔPoorer ≥ 0 (for d-redistribution types) and regressions of the amount given in the give-and-take game and the strategy method on ΔRicher (initial gift card value of other - initial own gift card value) for all observations where ΔRicher ≥ 0. (for a-redistribution types). We multiply the corresponding coefficient for ΔPoorer by -1 for classification purposes. Types are classified as follows: -.25 $\leq$ sensitivity $<$ .25: Non-Equalizer, .25 $\leq$ sensitivity $<$ .75: Equalizer, all other values: Other. N=2,217.

Table 4.C13: The Socio-demographic Correlates of D-Redistribution Types

	(1) D-Equalizer	(2) D-Other
Male	-0.16**	0.06
	(0.07)	(0.08)
Age: 30-49	0.10	0.14
	(0.10)	(0.11)
Age: 50-69	0.05	-0.12
	(0.09)	(0.11)
Age: 70+	0.21	-0.07
	(-0.14)	(0.17)
Income: Middle	-0.08	-0.13
	(0.08)	(0.09)
Income: High	0.01	0.01
	(0.100)	(0.12)
Education: Middle	-0.08	-0.31***
	(0.08)	(0.09)
Education: High	0.01	-0.35***
	(0.10)	(0.12)
Ideology: Left	-0.12	0.22*
	(0.11)	(0.12)
Ideology: Right	-0.35***	0.17
	(0.10)	(0.10)
Germany	0.76***	0.34***
	(0.07)	(0.09)
Constant	0.29***	-0.19
	(0.10)	(0.11)
Observations	4,925	4,925

Note: This table reports coefficients from a multinomial regression of d-redistribution types on socio-demographics for the pooled dataset with d-non-equalizer as the base outcome (without weights). The reference categories for the covariates are: age 18-29, income: low, age: 18-29, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C14: The Socio-demographic Correlates of A-Redistribution Types

	(1) A-Equalizer	(2) A-Other
Male	-0.01	0.23**
	(0.06)	(0.09)
Age: 30-49	-0.40***	-0.22*
	(0.09)	(0.13)
Age: 50-69	-0.38***	-0.48***
	(0.09)	(0.12)
Age: 70+	-0.14	-0.52**
	(0.13)	(0.20)
Income: Middle	0.11	0.05
	(0.07)	(0.10)
Income: High	0.11	-0.03
	(0.09)	(0.13)
Education: Middle	-0.05	-0.19*
	(0.07)	(0.11)
Education: High	-0.07	-0.24*
	(0.09)	(0.13)
Ideology: Left	0.15	0.24*
	(0.10)	(0.14)
Ideology: Right	-0.36***	0.44***
	(0.09)	(0.11)
Germany	0.44***	0.14
	(0.07)	(0.10)
Constant	0.34***	-0.96***
	(0.09)	(0.13)
Observations	4,925	4,925

Note: This table reports coefficients from a multinomial regression of a-redistribution types on socio-demographics for the pooled dataset with a-non-equalizer as the base outcome (without weights). The reference categories for the covariates are: age 18-29, income: low, age: 18-29, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C15: The Socio-demographic Correlates of D-Redistribution Types Without Ideology

	(1) D-Equalizer	(2) D-Other
Male	-0.19***	0.07
	(0.07)	(0.08)
Age: 30-49	0.09	0.13
	(0.10)	(0.11)
Age: 50-69	0.05	-0.14
	(0.09)	(0.11)
Age: 70+	0.19	-0.08
	(0.14)	(0.17)
Income: Middle	-0.07	-0.13
	(0.08)	(0.09)
Income: High	-0.01	0.03
	(0.10)	(0.12)
Education: Middle	-0.08	-0.30***
	(0.08)	(0.09)
Education: High	-0.01	-0.34***
	(0.10)	(0.12)
Germany	0.81***	0.33***
	(0.07)	(0.08)
Constant	0.22**	-0.11
	(0.10)	(0.11)
Observations	4,937	4,937

Note: This table reports coefficients from a multinomial regression of d-redistribution types on socio-demographics for the pooled dataset with d-non-equalizer as the base outcome (without weights). The reference categories for the covariates are: age 18-29, income: low, age: 18-29, Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C16: The Socio-demographic Correlates of A-Redistribution Types Without Ideology

	(1) A-Equalizer	(2) A-Other
Male	-0.12*	0.26***
	(0.06)	(0.09)
Age: 30-49	-0.41***	-0.23*
	(0.09)	(0.13)
Age: 50-69	-0.38***	-0.50***
	(0.09)	(0.12)
Age: 70+	-0.17	-0.51**
	(0.13)	(0.20)
Income: Middle	0.11	0.04
	(0.07)	(0.10)
Income: High	0.08	-0.00
	(0.09)	(0.13)
Education: Middle	-0.04	-0.19*
	(0.07)	(0.11)
Education: High	-0.07	-0.20
	(0.09)	(0.13)
Germany	0.48***	0.08
	(0.06)	(0.09)
Constant	0.31***	-0.83***
	(0.09)	(0.13)
Observations	4,937	4,937

Note: This table reports coefficients from a multinomial regression of a-redistribution types on socio-demographics for the pooled dataset with a-non-equalizer as the base outcome (without weights). The reference categories for the covariates are: age 18-29, income: low, age: 18-29, Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C17: Correlations Between Redistribution Types and Policy Views: Pooled Results

	(1) Gov't Should Redistr. Wealth by Heavy Taxes on Rich	(2) Gov't Should not Spend Less on Benefits for Poor
D-Equalizer	0.26***	0.02
	(0.04)	(0.04)
D-Other	0.36***	-0.13***
	(0.05)	(0.05)
A-Equalizer	0.02	0.20***
	(0.04)	(0.04)
A-Other	0.10*	-0.09
	(0.06)	(0.06)
Own Initial Gift Card Value: 50	0.01	0.06
	(0.04)	(0.04)
Own Initial Gift Card Value: 75	-0.07	0.03
	(0.05)	(0.04)
Male	0.06*	-0.05
	(0.04)	(0.03)
Income: Middle	-0.09**	-0.17***
	(0.04)	(0.04)
Income: High	-0.17***	-0.30***
	(0.05)	(0.05)
Age: 30-49	0.07	0.16***
	(0.05)	(0.05)
Age: 50-69	-0.04	0.22***
	(0.06)	(0.06)

Table 4.C17 (continued)

Age: 70+	-0.26***	0.24***
	(0.09)	(0.08)
Education: Middle	-0.09**	-0.03
	(0.04)	(0.04)
Education: High	-0.10**	-0.13***
	(0.05)	(0.05)
Employed	0.02	-0.23***
	(0.05)	(0.05)
Unemployed	-0.01	-0.07
	(0.08)	(0.08)
Retired	0.1	-0.08
	(0.06)	(0.06)
In Education	-0.21**	-0.14
	(0.10)	(0.09)
Ideology: Left	0.50***	0.41***
	(0.05)	(0.05)
Ideology: Right	-0.31***	-0.58***
	(0.06)	(0.05)
Germany	0.58***	0.43***
	(0.04)	(0.04)
Constant	3.28***	3.66***
	(0.08)	(0.07)
Observations	4,921	4,921
R-squared	0.131	0.164

Table 4.C17 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics using the pooled dataset. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-redistribution types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C18: Correlations Between Redistribution Types and Policy Views: Pooled Results Without Weights

	(1) Gov't Should Redistr. Wealth by Heavy Taxes on Rich	(2) Gov't Should not Spend Less on Benefits for Poor
D-Equalizer	0.25***	0.05
	(0.04)	(0.04)
D-Other	0.34***	-0.11**
	(0.05)	(0.05)
A-Equalizer	0.03	0.19***
	(0.04)	(0.04)
A-Other	0.07	-0.10*
	(0.06)	(0.06)
Own Initial Gift Card Value: 50	0.00	0.06
	(0.04)	(0.04)
Own Initial Gift Card Value: 75	-0.07	0.04
	(0.04)	(0.04)
Male	0.04	-0.06*
	(0.03)	(0.03)
Income: Middle	-0.10**	-0.17***
	(0.04)	(0.04)
Income: High	-0.19***	-0.31***
	(0.05)	(0.05)
Age: 30-49	0.08	0.14***
	(0.05)	(0.05)
Age: 50-69	-0.02	0.23***
	(0.05)	(0.05)

Table 4.C18 (continued)

Age: 70+	-0.23***	0.25***
	(0.09)	(0.08)
Education: Middle	-0.08**	-0.04
	(0.04)	(0.04)
Education: High	-0.09*	-0.14***
	(0.05)	(0.05)
Employed	0.02	-0.24***
	(0.05)	(0.05)
Unemployed	0.04	-0.07
	(0.07)	(0.07)
Retired	0.10	-0.11*
	(0.06)	(0.06)
In Education	-0.23**	-0.13
	(0.09)	(0.08)
Ideology: Left	0.47***	0.41***
	(0.05)	(0.05)
Ideology: Right	-0.27***	-0.58***
	(0.06)	(0.05)
Germany	0.59***	0.43***
	(0.04)	(0.03)
Constant	3.29***	3.65***
	(0.07)	(0.07)
Observations	4,921	4,921
R-squared	0.123	0.163

Table 4.C18 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics using the pooled dataset without weights. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-redistribution types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C19: Correlations Between Redistribution Types and Policy Views: Country Results

	(1) USA	(2) GE	(3) USA	(4) GE
	Gov't Should Redistr. Wealth by Heavy Taxes on Rich		Gov't Should not Spend Less on Benefits for Poor	
D-Equalizer	0.31***	0.11*	0.05	-0.04
	(0.06)	(0.06)	(0.05)	(0.06)
D-Other	0.46***	0.12	-0.10	-0.18**
	(0.07)	(0.08)	(0.07)	(0.08)
A-Equalizer	0.04	0.01	0.19***	0.23***
	(0.06)	(0.06)	(0.05)	(0.06)
A-Other	0.08	0.07	-0.15*	0.01
	(0.08)	(0.09)	(0.08)	(0.10)
Own Initial Gift Card Value: 50	-0.00	0.04	0.02	0.14**
	(0.06)	(0.06)	(0.06)	(0.06)
Own Initial Gift Card Value: 75	-0.09	-0.03	-0.01	0.08
	(0.06)	(0.06)	(0.06)	(0.06)
Male	0.01	0.12**	-0.10**	-0.00
	(0.05)	(0.05)	(0.05)	(0.05)
Income: Middle	-0.11*	-0.05	-0.28***	-0.05
	(0.06)	(0.05)	(0.05)	(0.05)
Income: High	-0.09	-0.31***	-0.39***	-0.17**
	(0.08)	(0.08)	(0.07)	(0.07)
Age: 30-49	0.10	0.06	0.09	0.29***
	(0.07)	(0.08)	(0.07)	(0.09)

Table 4.C19 (continued)

Age: 50-69	-0.14*	0.16*	0.16**	0.33***
	(0.07)	(0.09)	(0.07)	(0.09)
Age: 70+	-0.46***	0.16	0.15	0.41***
	(0.13)	(0.12)	(0.11)	(0.12)
Education: Middle	-0.17***	0.02	-0.03	-0.04
	(0.06)	(0.05)	(0.05)	(0.05)
Education: High	-0.12*	-0.12	-0.13**	-0.04
	(0.07)	(0.08)	(0.07)	(0.07)
Employed	0.06	0.01	-0.27***	-0.10
	(0.06)	(0.10)	(0.06)	(0.10)
Unemployed	0.01	0.00	-0.12	0.08
	(0.09)	(0.14)	(0.09)	(0.15)
Retired	0.08	0.11	-0.05	0.01
	(0.09)	(0.11)	(0.08)	(0.11)
In Education	-0.06	-0.20	-0.29**	0.17
	(0.15)	(0.15)	(0.14)	(0.14)
Ideology: Left	0.46***	0.51***	0.54***	0.27***
	(0.07)	(0.06)	(0.07)	(0.07)
Ideology: Right	-0.43***	0.04	-0.70***	-0.12
	(0.07)	(0.10)	(0.06)	(0.10)
Constant	3.35***	3.77***	3.85***	3.72***
	(0.10)	(0.13)	(0.09)	(0.13)
Observations	2733	2188	2733	2188
R^2	0.1	0.1	0.2	0.1

Table 4.C19 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics in the United States and Germany. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-inequality types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C20: Correlations Between Redistribution Types and Policy Views: Country Results Without Weights

	(1) USA	(2) GE	(3) USA	(4) GE
	Gov't Should Redistr. Wealth by Heavy Taxes on Rich		Gov't Should not Spend Less on Benefits for Poor	
D-Equalizer	0.32***	0.08	0.07	-0.01
	(0.06)	(0.06)	(0.05)	(0.05)
D-Other	0.46***	0.08	-0.09	-0.16**
	(0.07)	(0.07)	(0.06)	(0.07)
A-Equalizer	0.04	0.01	0.21***	0.19***
	(0.05)	(0.05)	(0.05)	(0.05)
A-Other	0.05	0.03	-0.13*	-0.03
	(0.08)	(0.08)	(0.08)	(0.09)
Own Initial Gift Card Value: 50	-0.02	0.03	-0.00	0.15***
	(0.06)	(0.05)	(0.06)	(0.06)
Own Initial Gift Card Value: 75	-0.10*	-0.00	-0.02	0.12**
	(0.06)	(0.06)	(0.06)	(0.06)
Male	-0.01	0.10**	-0.10**	-0.02
	(0.05)	(0.04)	(0.05)	(0.04)
Income: Middle	-0.10*	-0.07	-0.28***	-0.04
	(0.06)	(0.05)	(0.05)	(0.05)
Income: High	-0.11	-0.32***	-0.41***	-0.16**
	(0.07)	(0.07)	(0.07)	(0.07)
Age: 30-49	0.12*	0.08	0.09	0.27***
	(0.07)	(0.08)	(0.07)	(0.08)

Table 4.C20 (continued)

Age: 50-69	-0.13*	0.21**	0.17**	0.37***
	(0.07)	(0.08)	(0.07)	(0.08)
Age: 70+	-0.45***	0.20*	0.16	0.44***
	(0.12)	(0.11)	(0.11)	(0.11)
Education: Middle	-0.16***	0.02	-0.03	-0.04
	(0.06)	(0.05)	(0.05)	(0.05)
Education: High	-0.10	-0.12	-0.13**	-0.06
	(0.07)	(0.07)	(0.07)	(0.07)
Employed	0.07	-0.01	-0.26***	-0.12
	(0.06)	(0.09)	(0.06)	(0.09)
Unemployed	0.06	0.01	-0.12	0.11
	(0.09)	(0.13)	(0.08)	(0.13)
Retired	0.06	0.07	-0.05	-0.05
	(0.09)	(0.09)	(0.08)	(0.09)
In Education	-0.14	-0.21	-0.29**	0.22*
	(0.14)	(0.13)	(0.12)	(0.12)
Ideology: Left	0.44***	0.47***	0.50***	0.31***
	(0.07)	(0.06)	(0.07)	(0.07)
Ideology: Right	-0.35***	0.01	-0.71***	-0.09
	(0.07)	(0.09)	(0.06)	(0.09)
Constant	3.34***	3.82***	3.85***	3.70***
	(0.09)	(0.12)	(0.09)	(0.12)
Observations	2733	2188	2733	2188
R^2	0.1	0.1	0.2	0.1

Table 4.C20 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics in the United States and Germany without weights. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-inequality types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C21: Correlations Between Redistribution Types and Policy Views: Pooled Results, Alternative Coding

	(1) Gov't Should Redistr. Wealth by Heavy Taxes on Rich	(2) Gov't Should not Spend Less on Benefits for Poor
D-Equalizer$_{Alt}$	0.26***	0.03
	(0.05)	(0.04)
D-Other$_{Alt}$	0.33***	-0.16***
	(0.05)	(0.05)
A-Equalizer$_{Alt}$	0.05	0.20***
	(0.04)	(0.04)
A-Other$_{Alt}$	0.13**	-0.07
	(0.05)	(0.05)
Own Initial Gift Card Value: 50	0.01	0.07*
	(0.04)	(0.04)
Own Initial Gift Card Value: 75	-0.06	0.03
	(0.04)	(0.04)
Male	0.06*	-0.05
	(0.04)	(0.03)
Income: Middle	-0.09**	-0.18***
	(0.04)	(0.04)
Income: High	-0.16***	-0.30***
	(0.05)	(0.05)
Age: 30-49	0.07	0.15***
	(0.05)	(0.05)
Age: 50-69	-0.03	0.21***
	(0.06)	(0.06)

Table 4.C21 (continued)

Age: 70+	-0.26***	0.23***
	(0.09)	(0.08)
Education: Middle	-0.09**	-0.04
	(0.04)	(0.04)
Education: High	-0.10**	-0.14***
	(0.05)	(0.05)
Employed	0.02	-0.23***
	(0.05)	(0.05)
Unemployed	-0.01	-0.07
	(0.08)	(0.08)
Retired	0.10	-0.07
	(0.06)	(0.06)
In Education	-0.20**	-0.15
	(0.10)	(0.09)
Ideology: Left	0.50***	0.41***
	(0.05)	(0.05)
Ideology: Right	-0.32***	-0.58***
	(0.06)	(0.05)
Germany	0.57***	0.43***
	(0.04)	(0.04)
Constant	3.25***	3.69***
	(0.08)	(0.07)
Observations	4,921	4,921
R-squared	0.131	0.166

Table 4.C21 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics using the pooled dataset and the alternative coding rules described in Appendix 4.A. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-redistribution types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C22: Correlations Between Redistribution Types and Policy Views: Pooled Results, Alternative Coding, Without Weights

	(1) Gov't Should Redistr. Wealth by Heavy Taxes on Rich	(2) Gov't Should not Spend Less on Benefits for Poor
D-Equalizer$_{Alt}$	0.24***	0.06
	(0.04)	(0.04)
D-Other$_{Alt}$	0.32***	-0.12***
	(0.05)	(0.04)
A-Equalizer$_{Alt}$	0.06	0.19***
	(0.04)	(0.04)
A-Other$_{Alt}$	0.11**	-0.09*
	(0.05)	(0.05)
Own Initial Gift Card Value: 50	0.00	0.07*
	(0.04)	(0.04)
Own Initial Gift Card Value: 75	-0.06	0.05
	(0.04)	(0.04)
Male	0.04	-0.05
	(0.03)	(0.03)
Income: Middle	-0.10**	-0.18***
	(0.04)	(0.04)
Income: High	-0.19***	-0.31***
	(0.05)	(0.05)
Age: 30-49	0.09*	0.13***
	(0.05)	(0.05)

Table 4.C22 (continued)

Age: 50-69	-0.01	0.22***
	(0.05)	(0.05)
Age: 70+	-0.23***	0.24***
	(0.09)	(0.08)
Education: Middle	-0.08**	-0.04
	(0.04)	(0.04)
Education: High	-0.08*	-0.15***
	(0.05)	(0.05)
Employed	0.02	-0.24***
	(0.05)	(0.05)
Unemployed	0.04	-0.07
	(0.07)	(0.07)
Retired	0.10	-0.11*
	(0.06)	(0.06)
In Education	-0.23**	-0.13
	(0.09)	(0.08)
Ideology: Left	0.47***	0.41***
	(0.05)	(0.05)
Ideology: Right	-0.27***	-0.57***
	(0.06)	(0.05)
Germany	0.59***	0.43***
	(0.04)	(0.03)
Constant	3.26***	3.68***
	(0.07)	(0.07)
Observations	4,921	4,921
R-squared	0.123	0.165

Table 4.C22 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics using the pooled dataset and the alternative coding rules described in Appendix 4.A without weights. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-redistribution types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C23: Correlations Between Redistribution Types and Policy Views: Country Results, Alternative Coding

	(1) USA	(2) GE	(3) USA	(4) GE
	Gov't Should Redistr. Wealth by Heavy Taxes on Rich		Gov't Should not Spend Less on Benefits for Poor	
D-Equalizer$_{Alt}$	0.29***	0.11*	0.06	-0.03
	(0.06)	(0.07)	(0.06)	(0.06)
D-Other$_{Alt}$	0.45***	0.07	-0.08	-0.28***
	(0.07)	(0.08)	(0.06)	(0.07)
A-Equalizer$_{Alt}$	0.07	0.02	0.20***	0.21***
	(0.06)	(0.06)	(0.05)	(0.06)
A-Other$_{Alt}$	0.14*	0.04	-0.16**	0.05
	(0.07)	(0.08)	(0.07)	(0.08)
Own Initial Gift Card Value: 50	-0.01	0.04	0.02	0.16***
	(0.06)	(0.06)	(0.06)	(0.06)
Own Initial Gift Card Value: 75	-0.09	-0.02	-0.01	0.10*
	(0.06)	(0.06)	(0.06)	(0.06)
Male	0.01	0.13**	-0.10**	-0.00
	(0.05)	(0.05)	(0.05)	(0.05)
Income: Middle	-0.11*	-0.05	-0.28***	-0.05
	(0.06)	(0.05)	(0.05)	(0.05)
Income: High	-0.09	-0.31***	-0.39***	-0.17**
	(0.08)	(0.08)	(0.07)	(0.07)
Age: 30-49	0.11	0.06	0.08	0.28***
	(0.07)	(0.08)	(0.07)	(0.08)

Table 4.C23 (continued)

Age: 50-69	-0.13*	0.16*	0.15**	0.33***
	(0.07)	(0.09)	(0.07)	(0.09)
Age: 70+	-0.45***	0.16	0.14	0.41***
	(0.13)	(0.12)	(0.11)	(0.12)
Education: Middle	-0.16***	0.02	-0.03	-0.05
	(0.06)	(0.05)	(0.05)	(0.05)
Education: High	-0.12*	-0.12*	-0.13*	-0.06
	(0.07)	(0.08)	(0.07)	(0.07)
Employed	0.06	0.01	-0.27***	-0.10
	(0.06)	(0.10)	(0.06)	(0.10)
Unemployed	0.01	0.01	-0.12	0.08
	(0.09)	(0.14)	(0.09)	(0.15)
Retired	0.08	0.11	-0.05	0.02
	(0.09)	(0.11)	(0.08)	(0.11)
In Education	-0.05	-0.21	-0.29**	0.16
	(0.15)	(0.15)	(0.14)	(0.14)
Ideology: Left	0.46***	0.51***	0.54***	0.27***
	(0.07)	(0.06)	(0.07)	(0.07)
Ideology: Right	-0.44***	0.05	-0.69***	-0.11
	(0.07)	(0.10)	(0.06)	(0.10)
Constant	3.31***	3.77***	3.87***	3.75***
	(0.10)	(0.13)	(0.09)	(0.13)
Observations	2733	2188	2733	2188
R^2	0.1	0.1	0.2	0.1

Table 4.C23 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics in the United States and Germany and the alternative coding rules described in Appendix 4.A. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-inequality types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Table 4.C24: Correlations Between Redistribution Types and Policy Views: Country Results, Alternative Coding, Without Weights

	(1) USA	(2) GE	(3) USA	(4) GE
	Gov't Should Redistr. Wealth by Heavy Taxes on Rich		Gov't Should not Spend Less on Benefits for Poor	
D-Equalizer$_{Alt}$	0.29***	0.08	0.08	-0.00
	(0.06)	(0.06)	(0.05)	(0.05)
D-Other$_{Alt}$	0.45***	0.05	-0.06	-0.24***
	(0.06)	(0.07)	(0.06)	(0.07)
A-Equalizer$_{Alt}$	0.08	0.02	0.21***	0.17***
	(0.06)	(0.05)	(0.05)	(0.05)
A-Other$_{Alt}$	0.13*	0.01	-0.15**	0.00
	(0.07)	(0.07)	(0.07)	(0.07)
Own Initial Gift Card Value: 50	-0.02	0.04	-0.00	0.17***
	(0.06)	(0.05)	(0.05)	(0.05)
Own Initial Gift Card Value: 75	-0.10	0.00	-0.02	0.14**
	(0.06)	(0.06)	(0.05)	(0.05)
Male	-0.01	0.10**	-0.10**	-0.01
	(0.05)	(0.04)	(0.05)	(0.04)
Income: Middle	-0.11*	-0.07	-0.28***	-0.04
	(0.06)	(0.05)	(0.05)	(0.05)
Income: High	-0.11	-0.32***	-0.41***	-0.16**
	(0.07)	(0.07)	(0.07)	(0.07)
Age: 30-49	0.13*	0.08	0.08	0.27***
	(0.07)	(0.08)	(0.07)	(0.08)

Table 4.C24 (continued)

Age: 50-69	-0.11	0.21**	0.15**	0.36***
	(0.07)	(0.08)	(0.07)	(0.08)
Age: 70+	-0.43***	0.20*	0.15	0.43***
	(0.12)	(0.11)	(0.10)	(0.11)
Education: Middle	-0.15***	0.02	-0.03	-0.05
	(0.06)	(0.05)	(0.05)	(0.05)
Education: High	-0.10	-0.12	-0.13**	-0.07
	(0.07)	(0.07)	(0.07)	(0.07)
Employed	0.07	-0.01	-0.26***	-0.12
	(0.06)	(0.09)	(0.06)	(0.09)
Unemployed	0.06	0.01	-0.12	0.11
	(0.09)	(0.13)	(0.08)	(0.13)
Retired	0.06	0.07	-0.05	-0.04
	(0.09)	(0.09)	(0.08)	(0.09)
In Education	-0.13	-0.21	-0.30**	0.22*
	(0.14)	(0.13)	(0.12)	(0.12)
Ideology: Left	0.44***	0.47***	0.50***	0.31***
	(0.07)	(0.06)	(0.07)	(0.07)
Ideology: Right	-0.37***	0.01	-0.71***	-0.08
	(0.07)	(0.09)	(0.06)	(0.09)
Constant	3.29***	3.82***	3.87***	3.74***
	(0.10)	(0.12)	(0.09)	(0.12)
Observations	2733	2188	2733	2188
R^2	0.1	0.1	0.2	0.1

Table 4.C24 (continued)

Note: This table reports coefficients from linear regressions of policy views on redistribution types, own initial gift card value, and socio-demographics in the United States and Germany and the alternative coding rules described in Appendix 4.A without weights. Dependent variables are measured on a five-point scale (1 = strongly disagree, 5 = strongly agree). The reference category for d- and a-inequality types is "non-equalizer". The reference categories for the other covariates are: own initial gift card value: 25, income: low, age: 18-29, education: low, occupation: other, ideology: center. Robust standard errors are reported in parentheses (*** p<.01, ** p<.05, *p<.10).

Chapter 5

Concluding Remarks

The present thesis contributes to the literature on political behavior as it analyzes to what extent individuals take into account different considerations related to the distribution of economic benefits when forming political preferences and attitudes towards political institutions. The evidence suggests that while egocentric economic considerations play an important role in influencing political behavior, some individuals also take into account how policies and politicians influence the well-being of other societal groups such as the poor and also seem to act accordingly. The findings of this thesis thus support well-established accounts of preference formation while also highlighting the importance researchers should pay to specific forms of other-regarding preferences.

By illuminating the connection between political behavior and egoistic as well as other-regarding preferences, the contributions in this thesis not only show that individuals react to unfavorable macroeconomic outcomes as illustrated in Chapter 2, but also offer a social norms-based interpretation as to why this may be the case: Voters also care about less privileged members of the society. Chapter 3 shows that citizens take into account how a policy affects the least well-off when forming policy preferences. The results presented in Chapter 4 lend further credence to this idea by showing that individuals are willing to forgo some of the money they could win that was randomly assigned to them to give it to those who are worse off than themselves. The fact that types derived from the behavior in this experiment also correlate with redistributive policy preferences suggests that social norms-based approaches improve our understanding of why certain policies

are implemented and why not.

This thesis approaches the question of whether and why economic considerations influence political behavior by mainly combining literature from the fields of political science and economics. It thereby attempts to show that combining ideas, theoretical concepts, and findings from different disciplines may help to shed light on long-standing debates and thus contribute to learning more about fundamental challenges that lie ahead. The recent years have seen an increase in the political science literature that incorporates findings from research on social norms. These analyses help to learn more about the underlying processes and mechanisms and thus also lead to a better understanding of why sociotropic considerations matter to the electorate. At the same time, these findings also suggest that this approach promises fruitful avenues for future research as these ideas can be applied to study many other outcomes that are not only theoretically relevant but also matter in the real world.

From a policy-perspective, these three contributions highlight the importance of economic considerations and benefits to the electorate. While the results show that both personal economic well-being and macroeconomic performance matter to voters and thus corroborate the idea expressed in the campaign slogan "It's the Economy, Stupid" that brought Bill Clinton into office (Anderson and Jackson 2005), the findings also suggest that at least some individuals care about the well-being of others. This gives policymakers leeway to implement policy reforms that improve the situation of the worst off. In fact, the results even suggest that voters are to a certain extent willing to accept to incur some costs on themselves if it benefits the poor, which highlights the possibilities that governments may have to implement measures targeted at redressing inequality via redistribution. Considering the increased attention this topic has received over the past few years, this thesis thus makes an important contribution as it shows the importance citizens attach to the well-being of economically disadvantaged individuals.

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Research Interests

I apply a range of quantitative methods to model the relationship between economic outcomes and politics to study public opinion. My research heavily relies on causal inference methods and also includes online survey experiments.

Education

University of St.Gallen, St.Gallen, Switzerland — *02/2014 – present*
PhD, International Affairs and Political Economy
Dissertation Committee: Prof. Dr. Michael M. Bechtel (Supervisor), Prof. Dr. Roland Hodler (Co-Supervisor), and Prof. Dr. Catherine E. de Vries

University of St.Gallen, St.Gallen, Switzerland — *09/2011 – 06/2013*
M.A., International Affairs and Governance
Master s thesis: *Role Perceptions of Swiss Members of Parliament (German)*

Sciences Po, Paris, France — *01/2013 – 05/2013*
M.A. Erasmus Exchange, International Affairs

University of Zurich, Zurich, Switzerland — *09/2007 – 07/2011*
B.A., Political Science
Minors: Computer Science and Public Law

Additional Training

Essex/Nuffield CESS Experimental Summer School
Introduction to Experiments (2016), University of Oxford

Leuven-Montréal Winter School on Elections and Voting Behavior
Block Course on Elections and Voting Behavior (2016), Universite de Montreal

Essex Summer School in Social Science Data Analysis
Panel Data Analysis for Comparative Research (2015), Methods for Causal Inference (2014), University of Essex

St. Gallen Summer School in Empirical Research Methods
Time Series Analysis - Introduction (2015), University of St. Gallen

PhD-Course University of Zurich
Modern Statistical Inference for the Social Sciences (2014), University of Zurich

Research Visit

University of Oxford, Oxford, UK — *09/2015 – 02/2016*
Visiting Doctoral Student, Department of Politics and International Relations

Teaching Experience

University of St. Gallen — *Spring 2015*
Quantitative Methods (BA Class, German): Teaching Assistant

University of Zurich — *Spring 2009, Fall 2009/2010*
Methods in Political Science and Methodology in Political Science: Research Design (BA Class, German): Student Teaching Assistant

Professional Experience

School of Economics and Political Science, University of St. Gallen — *03/2014 – 02/2017*
Research Assistant
I was part of the research team of Prof. Michael M. Bechtel and wrote my dissertation as part of the SNSF-funded project Mass support for nancial bailouts .

Arena, Swiss Public Television — *12/2014 – 02/2014*
Intern
I assisted the team in preparing and producing the weekly political discussion program on TV.

Müller Consulting und Partner *06/2013 – 10/2013*

Intern

I contributed to the research for documents and the organization of events.

International Relations and Security Network, Zurich *06/2012 – 08/2012*

Summer Student Editor

Together with a team of other summer student editors, I helped expanding the digital library.

Romansh Public Radio Station *Summer 2008, 2009, 2010, 2011*

Intern/Radio Reporter

I conducted interviews, researched background information, prepared, and recorded radio reports.

Awards and Honors

Best Young Scholar Paper Award, awarded at the conference on Inequality & Fairness of Political Reforms at the University of Mannheim	*2016*
PhD Exchange Grant, Swiss National Science Foundation (CHF 8 900)	*2015*
Conference Travel Grant, Swiss Academy of Humanities and Social Sciences (CHF 500)	*2015*

Conferences / Workshops

Conference on Inequality and Fairness of Political Reforms	*2016*
European Political Science Association	*2015, 2016*
Swiss Political Science Association	*2015, 2016*
ECPR Standing Group on Citizenship, Portsmouth	*2015*
German Political Science Association, Comparative Politics Section	*2015*