

Essays on Political Participation and the Quality of Democracy

by

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Essays on Political Participation and the Quality of Democracy

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Abstract

In advanced industrial democracies, a substantial number of citizens feel alienated from mainstream politics and political elites. This dissertation analyzes factors that help mitigate two crucial aspects of citizen political alienation in these countries: political disengagement by an increasing segment of the electorate, especially poor and young voters; and the turn to radical alternatives such as far-right populist politicians and parties.

Study 1 assesses the effect of procedural information costs - in particular, uncertainty about whether one has to be 18 by the registration deadline or by Election Day - on youth voter turnout across U.S. states. Using a regression discontinuity design with official state voter records and leveraging a discontinuity in voter turnout around the registration deadline, this study shows that uncertainty about the registration requirements for first-time voters depresses voter turnout both in the immediate, and also in subsequent, elections among this group of voters, turning many of them into habitual non-voters.

Study 2 takes a new look at the relationship between levels of political participation and support for left-wing parties and policies. It reanalyzes a critical case - Australia in the early 20th century - frequently cited as a strong demonstration of such a relationship. Based on an original and more fine-grained dataset of district electoral data in combination with a difference-in-differences design, this study tests the robustness of the previously found relationship and investigates its mechanisms.

Study 3 uses survey experimentation to test the responsiveness of populist voters to mainstream political messages. Based on a large-scale survey experiment with the polling firm

YouGov shortly before the 2017 German federal election, it finds that emphasizing the good performance of the German economy was the most effective strategy to increase support for the incumbent Christian Democrats among likely rightwing populist voters.

Overall, these findings speak to ongoing debates about the ability of politicians to shape citizens' political behavior. Improving on previous quantitative research in this area, this research highlights the limitations of institutional fixes and provides new insights into the role of procedural information and political framing for civic engagement.

Lay Summary

A increasing number of citizens feel alienated from mainstream politics and political elites in advanced industrial democracies. This dissertation uses state-of-the-art quantitative techniques of survey experimentation and quasi-experiments to analyze factors that help mitigate two crucial aspects of citizen political alienation in these countries: political disengagement by an increasing segment of the electorate, especially poor and young voters; and the turn to radical alternatives such as far-right populist politicians and parties. Overall, this dissertation points to limitations of institutional fixes such as compulsory voting laws to address issues of economic inequality and highlights the crucial role of procedural information and performance-based campaign messages to increase civic engagement in established democracies.

Preface

This dissertation is original, unpublished, independent work by the author, A. Held. The survey reported in Chapter 4 was covered by UBC Ethics Certificate number H17-01708.

Table of Contents

Abstract	iii
Lay Summary	v
Preface	vi
Table of Contents	vii
List of Tables	xi
List of Figures	xiii
Acknowledgments	xv
1 Introduction	1
2 Turning 18, but Not Quite: The Impact of Procedural Information Costs on Turnout	6
2.1 Introduction	6
2.2 Procedural Information Costs and Political Participation	10
2.3 Data	14
2.3.1 Visualizing Registration Deadlines and Turnout Patterns	16
2.4 Identification Strategy	22

2.5	Results	25
2.5.1	Persistence of Effects	27
2.5.2	Alternative Explanations: Formal Rules and Concurrent Administra- tive Acts	31
2.6	Conclusion	35
3	The Electoral Consequences of Increasing Voter Turnout: New Evidence from a Natural Experiment	38
3.1	Introduction	38
3.2	Compulsory Voting, Politically Disengaged Citizens, and Partisan Support . . .	43
3.3	Compulsory Voting in Australia	48
3.4	Data and Methods	51
3.4.1	Data and Dependent Variables	51
3.4.2	Research Design	53
3.5	Results	59
3.5.1	State-Level Results	59
3.5.2	Federal-Level Results	67
3.6	Conclusion	72
4	Can Potential Populist Voters Be Framed? Results from a Survey Experiment in the 2017 German Election	76
4.1	Introduction	76
4.2	Populist Voters and Mainstream Non-Immigration Messages	79
4.2.1	Performance, Policies, and Uncertainty	84
4.3	Data and Methods	88
4.3.1	Data	88

4.3.2	Dependent Variables	91
4.3.3	Independent Variables	91
4.3.4	Methods	94
4.4	Results	96
4.4.1	Exploratory Analyses: The Role of Political Knowledge	100
4.5	Conclusion	103
5	Conclusion	107
	Bibliography	111
A	Supporting Materials to Chapter 2	124
A.1	Smoothness of Density around Registration Deadline Cutoff	124
A.2	Registration Deadline Discontinuities in Texas	125
A.3	Separate Treatment Around School Entry Cutoff	127
A.4	Discontinuity in Daily Vote Totals for Florida	128
A.5	Robustness of RD Estimates to Alternative Bandwidths	129
A.6	Robustness of RD Estimates to Alternative Model Specifications	130
A.7	Robustness of Persistence of Effect to Varying Bandwidths	134
A.8	Information on Registration Requirements Across U.S. States	135
A.9	Voter Registration Form from Florida	137
B	Supporting Materials to Chapter 3	139
B.1	Within-State Analysis of Treatment Effects	139
B.2	Within-State Analysis of Changes in Turnout and Labor Seat Share	141
C	Supporting Materials to Chapter 4	145
C.1	Question Wording of Treatments (English Translation)	145

C.2 Question Wording of Treatments in Survey (German Original) 146

C.3 Summary Statistics 148

C.4 Results with Alternative Dependent Variable: Party Favorability 149

C.5 Results with Continuous Political Knowledge Variable 149

List of Tables

Table 2.1	Turnout in U.S. Presidential Elections for 1990 Birth Cohorts	28
Table 2.2	CACE of 2008 Incorrect Procedural Beliefs on Downstream Voting	30
Table 3.1	The Adoption of Compulsory Voting in State and Commonwealth Elections Across Australia	49
Table 3.2	Effect of Compulsory Voting in State Assembly Elections	60
Table 3.3	Effect of Compulsory Voting Using Within-State Variation in Treatment In- tensity	63
Table 3.4	Effect of Compulsory Voting in Commonwealth House of Representatives Elections	71
Table 4.1	Expected Effect on Electoral Support	95
Table 4.2	Vote Intention in 2017 German Federal Election	98
Table 4.3	2017 Vote Intention by Level of Political Knowledge	102
Table A.1	Turnout in U.S. Presidential Elections for 1990 Birth Cohorts	131
Table A.2	Turnout in U.S. Presidential Elections	132
Table A.3	Voter Registration Information Across Seven U.S. States	137
Table B.1	Effects of Compulsory Voting Using Within-State Variation in Treatment Intensity - Non-Labor Candidates	140

Table B.2	Effects of Compulsory Voting Using Within-State Variation in Treatment Intensity - Independent Candidates	140
Table C.1	Party Favorability Rating in 2017 German Federal Election	150
Table C.2	Models for 2017 Vote Intention with Continuous Political Knowledge Variable	151

List of Figures

Figure 2.1	Turnout in Florida for People Born in 1986/87 and 1990/91	18
Figure 2.2	Registration Deadline Discontinuities Across U.S. States in 2008 Election .	21
Figure 2.3	Registration Timing for Floridians born Feb 4-Aug 3, 1990	33
Figure 3.1	Change in Turnout and ALP Vote Share in New South Wales, 1927-1930 .	64
Figure 3.2	Change in Turnout and ALP Vote Share in Tasmania, 1928-1931	66
Figure 3.3	Voter Turnout in State and Federal State Assembly Elections, by State . . .	69
Figure 4.1	Share of Respondents Intending to Vote for Mainstream Parties, by Treat- ment Condition	99
Figure 4.2	Share of Respondents Intending to Vote for the AfD, by Political Knowl- edge and Treatment Condition	103
Figure A.1	Distribution of Births in Florida between May 5, 1990 and May 4, 1991 . .	125
Figure A.2	Turnout in Presidential Elections in Texas	126
Figure A.3	Turnout in 2012 Presidential Election in Ohio	127
Figure A.4	Votes in 2008 and 2012 Presidential Elections in Florida - Excluding Week- ends	129
Figure A.5	Votes in 2008 and 2012 Presidential Elections in Florida - Including Week- ends	130

Figure A.6	Varying Bandwidths Around Registration Deadline Cutoff	134
Figure A.7	Voter Registration Form from Florida	138
Figure B.1	Change in Turnout and ALP Vote Share in Queensland, 1912-1915	141
Figure B.2	Change in Turnout and ALP Vote Share in South Australia, 1941-1944 . .	142
Figure B.3	Change in Turnout and ALP Vote Share in Victoria, 1924-1927	143
Figure B.4	Change in Turnout and ALP Vote Share in Western Australia, 1936-1939 .	144
Figure C.1	Effect of Economic Voting Treatment on AfD Support	150
Figure C.2	Effect of Social Inequality Treatment on AfD Support	152

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Chapter 1

Introduction

On June 23, 2016 a majority of 51.9% of British voters cast their ballot in favor of leaving the European Union in the 2016 EU Membership referendum. The outcome of the referendum took most politicians and political observers in Britain as well as abroad by surprise (Evans and Menon 2017, ch. 3). It sent shock waves across British politics, has led to a political stalemate in Westminster and is expected to have a fundamental impact on both future politics and the economy in Britain. There is little doubt that a significant number of British voters used the referendum to vent their dissatisfaction with the political establishment. However, the phenomenon of citizen political alienation is not limited to the British Isles. Events such as the election of Donald Trump in late 2016 or recent electoral successes of right-wing populist parties in several European countries have shown the extent to which this trend affects established democracies more generally. As a result, citizens, politicians and political scientists alike have focused on similar questions: What explains the frustration of an increasing number of citizens with their political elites that so suddenly erupted in Britain's referendum vote on June 23, 2016? Would election and referendum outcomes in these countries look different if groups with traditionally low levels of political participation – such as the young or the socioeconomically worse off – voted in larger numbers? What strategies can politicians both on the moderate political left and right adopt to regain the trust of disaffected citizens?

The goal of the research that is presented in the following pages is to help contribute an-

swers to at least some of these questions. In three separate, but related papers, this dissertation uses state-of-the-art econometric methods of causal inference and survey experimentation to advance our understanding of two crucial aspects of citizen alienation in advanced industrial democracies: political disengagement by an increasing segment of the electorate, especially poor and young voters; and the turn to radical alternatives such as far-right politicians and parties.

Study 1 focuses on youth voter turnout in the United States. It addresses a literature that uses large administrative datasets in combination with regression discontinuity (RD) designs to draw inferences about the effects of policy reforms on voting rates. Such studies typically compare voter turnout of young Americans whose 18th birthday falls shortly before Election Day with those who turn 18 shortly after. Using data from official state voter files from 18 U.S. states, this study shows that for many states there is a second consequential discontinuity in voter turnout that is not around the Election Day, but around the registration deadline 30 days earlier. It also shows that this discontinuity has serious policy implications. Procedural information costs that are associated with this deadline, and in particular the incorrect belief that one has to be 18 not by Election Day, but by the registration deadline, reduce turnout among certain young voters in some states by up to 25% (or 9 percentage points) in U.S. presidential elections.

Study 2 leverages an arguably exogenous shock to voter turnout due to the introduction of compulsory voting laws in Australia in the early 20th century to study the relationship between levels of political participation and support for left-wing parties and policies. Several studies find a direct link between higher levels of voter turnout and the electoral success of left-wing parties, suggesting that an institutional fix like the introduction of mandatory voting could have serious political consequences. This study improves on previous quasi-experimental research by using a unique dataset of more fine-grained electoral district election data in combination with a difference-in-differences design to provide a more direct empirical test of the assumed relationship. Based on results from this analysis, it shows that empirical support for the previously claimed association is weaker and more mixed than previously assumed. It also identifies

other factors that affect voter turnout – such as a party’s strategic choice in which districts it will run candidates and the geographic size of a district – and whose analysis might give us a fuller picture of the conditions under which higher turnout is likely to shift power toward left-wing parties.

Study 3 presents a theoretical framework that outlines the conditions under which mainstream messages may overcome populist citizens’ feeling of resentment toward the established elite and shift their vote intentions. While populist voters might be attracted by the anti-immigrant rhetoric of right-wing populist politicians, there are other policy considerations (e.g. economic redistribution, performance of the economy) that are of interest to this group of voters and where mainstream parties can credibly claim to better represent the interests of populist voters. It tests hypotheses on the effect of messages about economic performance, welfare policies, strategic considerations, and uncertainty on these people’s vote intentions through survey experimentation on a targeted sample of 1,800 citizens with a high propensity for supporting anti-immigrant populist parties during the 2017 German election. The findings suggest that populist voters are hardly swayed by these mainstream messages. However, some of them are responsive to messages that highlight the good performance of the German economy, increasing support for the incumbent Christian Democrats among this group of voters by about 6 percentage points. Overall, these results provide new insights into the potential of framing strategies for shaping citizen political behavior in democracies.

Beyond their substantive contribution to understanding citizen alienation, all three studies share two further features: they all rely on new and large data sources, and they apply sound causal identification strategies to the research on politically disaffected citizens.

Despite their recent growth across advanced industrial democracies, politically disaffected citizens still represent a small group of the population in most countries. In addition to their relatively small group size, disaffected citizens represent two additional challenges to researchers who want to study them and who rely on traditional, nationally representative public opinion surveys: 1) given their generally lower interest in politics and their lower socioeconomic status, they are less likely to be included in these surveys to begin with; and 2) even if they are

included, it is hard to identify them as a group of citizens because they do not share a clearly distinctive feature that makes them easily identifiable as “politically alienated.” For these reasons, there are limitations to the questions about these voters than can be answered with data from existing, nationally representative surveys. The three studies in this dissertation broaden the study of disaffected citizens by identifying new data sources that can be used to study their behavior quantitatively. These studies illustrate how large and unique data sources such as administrative voter registration records, fine-grained historical election returns or an original survey experiment promise to shed new light on our understanding of disaffected citizens’ engagement with politics.

The study of voter turnout has recently seen a shift toward experimental and quasi-experimental research designs that improve on traditional observational studies in this field by providing a more convincing empirical test of the effect of electoral reforms. Two papers of my dissertation directly speak to this literature by highlighting potential challenges to or limitations of some of these recent quasi-experimental studies themselves. Study 1 finds an additional discontinuity in voter turnout in the vicinity of the Election Day cutoff. This should caution researchers who use regression discontinuity designs in combination with voter file data and a person’s date of birth to take this discontinuity seriously and account for it in their study designs in order to avoid confounding through multiple treatments. Study 2 illustrates how a seemingly minor modification to a difference-in-differences design – the move from a cross-state to a within-state analysis – leads to a very different outcome. However, these findings are neither meant as a challenge to these studies nor to the use of quasi-experimental research designs more generally. Instead, they highlight the benefits of reflecting upon – and empirically testing – the assumptions that are used in these quasi-experimental research designs.

Taken together, the findings from all three studies paint a picture of politicians’ ability to shape alienated citizens’ political behavior that neither warrants exaggerated optimism nor is it reason to despair. They show that carefully crafted electoral reforms and policies are able to reach at least some of the politically disaffected citizens. They also show that these reforms make these voters more likely to go to the polls and, for some of them, to vote according to

their material self-interest on Election Day. However, the studies also point to the limitations of such interventions, which are frequently less effective than commonly thought, and they highlight some unintended – and potentially unwanted – consequences of electoral reforms.

Chapter 2

Turning 18, but Not Quite: The Impact of Procedural Information Costs on Turnout

2.1 Introduction

Formal institutional rules determine who can vote in elections and how. Extensive research has shown that these rules - from voter ID laws (Hajnal, Lajevardi and Nielson 2017; Grimmer et al. 2018) to voter registration requirements (Burden et al. 2014; Wolfinger and Rosenstone 1980) - have a direct impact on voter turnout. In contrast, we know relatively little about the beliefs that citizens hold about these rules and how these beliefs affect their likelihood to participate in elections. Yet, beliefs about procedural aspects of elections play an important role. Citizens in the U.S. are increasingly concerned about so-called disinformation campaigns. These campaigns, which are frequently attributed to radical right or foreign sources (Bennett and Livingston 2018), intentionally spread incorrect or misleading information among people from the opposing side in order to harm moderate politicians or the Democratic party (Bennett and Livingston 2018; Kim et al. 2018). While most of the recent academic debate has focused on fake news and misleading reporting about issues such as immigration or crime in the context of the 2016 presidential election (Guess, Nyhan and Reifler 2018; Kim et al. 2018), disinformation campaigns also include targeted attempts to use incorrect procedural information to deter vot-

ers from the opponent to cast a vote (Stringer 2008). In the context of the 2018 congressional election, this has even led major national newspapers to explicitly warn voters about procedural misinformation that is used by certain groups to deter citizens from voting.¹

Thus, procedural information seems to play a crucial role for voter turnout. Voters who know how to vote and where to find their polling place are more likely to participate in elections. As a result, providing citizens with information about the location of their polling place has been shown to increase turnout (Brady and McNulty 2011). Similarly, turnout of Hispanic citizens goes up if their procedural information costs are reduced through language support in Spanish (Hopkins 2011). Yet, beyond a mere focus on procedural information about the election as a means to primarily make the voting process easier or more convenient for voters, there are many situations where citizens may hold outright incorrect beliefs about the electoral process. These incorrect procedural beliefs usually do not affect all citizens, but only a subgroup of people. Yet, for those who are affected by them, the effect may be extremely strong. Especially in cases where these incorrect procedural beliefs let citizens doubt their election eligibility or where they make them erroneously think that voting costs are substantially higher than they really are, these beliefs are likely to strongly depress voter turnout among those who hold them. For example, in the absence of correcting information, voters who erroneously believe they are ineligible to vote are unlikely to cast a ballot in an election (cf. Gerber et al. 2015). Similarly, doubting the secrecy of the ballot in a context where ballot secrecy is strictly enforced may plausibly lead people who hold such beliefs to abstain from voting due to privacy concerns (Gerber et al. 2013).

However, not every person is equally likely to be susceptible to this kind of misinformation. We would expect that high-information citizens, that is, those with higher education, that are more socially embedded and that have more experience voting are less likely to be swayed by new and incorrect procedural voting or registration information. In contrast, people from low-information groups often lack the resources and the motivation to question dubious

¹See, for example, Kevin Rose, “6 Types of Misinformation to Beware of on Election Day. (And What to Do if You Spot Them.)” New York Times, November 5, 2018, available at: <https://www.nytimes.com/2018/11/05/us/politics/misinformation-election-day.html>.

procedural information about elections and thus, are more likely to hold incorrect procedural beliefs. Recent research (Meredith and Morse 2014, 2015) supports this view by showing that ex-felons – a group that is likely to have a rather low level of procedural information – are frequently poorly informed about their eligibility status in elections. A considerable number of them assumes that they are ineligible to vote even though their voting rights had been restored previously and, as a result, does not vote in elections. This study focuses on another large group of low-information citizens that has received little attention so far in this literature (but see Corvalan and Cox 2018): first-time voters who have no or little experience with the voting system and thus, who are more likely to hold incorrect procedural beliefs on the administration of elections than older and more experienced voters.

In order to assess the effect of procedural information costs among young people, this study leverages a natural experiment from the U.S. context. In particular, it uses a large dataset that is based on voter file data from 18 U.S. states in combination with a regression discontinuity design to causally identify the effect of incorrect beliefs on voter turnout. In contrast to most previous research that use a similar design (Coppock and Green 2016; Holbein and Hillygus 2016; Meredith 2009; Nyhan, Skovron and Titiunik 2017), the present study does not focus on a discontinuity in turnout around the Election Day, but instead shifts the focus on a discontinuity around the registration deadline, which, for most states, happens to be roughly four weeks earlier. Doing so, I am able to show that there is a sharp drop in turnout right at the registration deadline in many U.S. states. This suggests that the problem of incorrect beliefs among young Americans about the registration requirements is not an isolated phenomenon, but instead quite common across U.S. states. More specifically, I present evidence that shows that people who turn 18 after the registration deadline, but before Election Day are significantly less likely to vote than those who turn 18 a few days earlier. This drop in turnout can be substantively large (up to 9 percentage points or 25%) and is consistent with an account that attributes the drop in turnout right after the registration deadline to incorrect beliefs about the voter registration requirements. Moreover, in line with research on the habitual nature of voting (Coppock and Green 2016; Dinas 2012; Fujiwara, Meng and Vogl 2016; Gerber, Green and Shachar 2003;

Meredith 2009) and on the effect of voter registration drives on voter turnout (Nickerson 2015), the decision not to register and vote in their first election has long-term consequences for these young people and makes them significantly less likely to vote in future elections. For example, 37% of young Floridians who were deterred from voting in 2008 as a result of erroneously believing they are ineligible due to their 18th birthday being after the registration deadline did not vote in the 2016 election 8 years later for the simple reason of having held these incorrect beliefs previously. In addition, I address two plausible alternative explanations that attribute this drop in turnout to formal rules or concomitant administrative acts that may plausibly reduce registration costs for young people are not able to account for the sharp drop in turnout right at the registration deadline.

This study makes three contributions. First, it sheds new light on the crucial role of beliefs about procedural aspects of elections in shaping voter turnout. Based on a large amount of new evidence, it shows both their widespread existence and their long-term effects on voting behavior for a low-information group of citizens: first-time voters. Second, it makes a methodological contribution by highlighting the crucial role that temporal discontinuities can play in studying procedural information effects. While this study highlights the relevance of a temporal discontinuity around the registration deadline for youth voter turnout, similar age-based discontinuities exist in other politically important areas and determine a citizen's eligibility for social welfare benefits or citizenship and immigration status. Thus, the approach presented in this paper can be applied to other policy areas where procedural information effects are likely to be highly consequential, but have not been a focus of previous research. Third, my findings have direct policy implications. They suggest that complex rules add additional costs to citizens, making program uptake less likely. Thus, in order to increase voter turnout or the propensity of citizens to make use of specific government programs, governments do not necessarily have to provide additional (financial) incentives or run expensive mobilization campaigns, but could start by simplifying the rules.

2.2 Procedural Information Costs and Political Participation

In line with a large literature on voter turnout that goes back to Downs (1957)'s foundational "An Economic Theory of Democracy," I assume that the act of voting entails costs for voters. These costs that are associated with casting a vote include, among other things, acquiring information about the procedural aspects of an election. At the very least, voters need to know how and when to register for an election and where and how to cast their vote on Election Day. Thus, it is not surprising that interventions that reduce these procedural information costs have been shown to have a positive effect on turnout. Brady and McNulty (2011), for example, find that informing citizens about the exact location of their polling place and about how to get there on Election Day makes them more likely to vote. Hopkins (2011) find a similar positive effect for an intervention that lowers the language barriers to Hispanic citizens by offering them the option to use a Spanish-language ballot.

However, there are different kinds of procedural information and not all of them simply help citizens cast a vote more conveniently. Another type of such procedural information (or lack thereof) are incorrect procedural beliefs. Citizens may hold beliefs that make them think erroneously that certain rules do not apply to them or that these rules work differently from how they actually work. Problematic are these incorrect beliefs especially in situations where they may make the act of voting appear substantially more costly in these people's eyes than it actually is, thereby strongly reducing the likelihood of these people to turn out and vote. For example, with respect to the institution of the secret ballot, Gerber et al. (2013) find that even in a long-standing democracy such as the United States where the secret ballot has been in place for more than a 100 years, a significant share of the population doubts its secrecy. Furthermore, in a field experiment, the authors show that informing citizens about the secrecy of the ballot by mail helps to correct these incorrect beliefs and, as a result, increases voter turnout. Another area where incorrect procedural beliefs may have a strong negative effect on turnout is when they are about a person's eligibility status in an election. Citizens who outright – but erroneously – believe that they are ineligible to register or vote in an election are

not very likely to participate in that election. Therefore, I would expect a strong reduction in turnout among this group of people. Consistent with this argument, Gerber et al. (2015) find that informing misinformed citizens about their eligibility status and correcting their wrong procedural beliefs can significantly increase political participation.

Incorrect procedural beliefs are not likely to affect all citizens equally, though. Especially low-information citizens – those with little prior personal exposure to elections and those most disadvantaged socially and economically (Brady, Verba and Schlozman 1995; Delli Carpini and Keeter 1996) – are most likely to lack the resources to effectively counter incorrect procedural beliefs. Several recent studies have started to analyze political participation among members of one such low-information group: ex-felons whose voting rights have been reinstated (Gerber et al. 2015; Meredith and Morse 2014, 2015). In line with the argument presented here, these studies find that ex-felons often incorrectly assume that they are still ineligible to vote even after their voting rights have been reinstated and as a result, are less likely to vote in elections (Gerber et al. 2015; Meredith and Morse 2015). These authors also show that administering an informational treatment that corrects these wrong beliefs significantly increased turnout among this group of people (Gerber et al. 2015; Meredith and Morse 2015).² However, while most of the existing research on incorrect procedural beliefs is limited to ex-felons, the phenomenon is likely to extend to people of other low-information groups, too.

Young people are another plausible low-information group. While certainly not every young person is politically uninformed, young people tend to be – almost by necessity – politically less experienced than many older people simply due to their lower age and as a result, the fact that they had fewer opportunities to engage in political activities such as voting. This lack of political experience combined with their generally lower political knowledge (Delli Carpini and Keeter 1996) will make them less likely to detect and question incorrect procedural information than older and politically more experienced citizens. This is particularly likely to be the

²The two studies discussed in the text focus on the states of Iowa and Connecticut. Meredith and Morse (2014)'s failure to replicate the effect for two other U.S. states suggests that the effectiveness of informational treatments is potentially dependent on additional factors, such as the type of delivery or the specific content of the treatment message, which were not systematically studied in these papers.

case for first-time voters who, by definition, never had the chance to participate in an election previously.³ For example, it is possible that first-time voters whose 18th birthday is after the registration deadline, but before Election Day erroneously assume that one has to be 18 by the registration deadline already in order to vote in an upcoming election and as a result, do not register and vote in this election. While this belief is wrong and contrary to existing law in all U.S. states, such a misperception seems to be common enough among young Americans that states explicitly address this issue in their voter guides or on their state election office websites.⁴ In addition to that, not only rules about when and how people can register, but also the clarity with which this information is conveyed to citizens varies across states, thus potentially creating explaining the confusion in states with less clear instructions on websites and voter registration forms.⁵

Some evidence about procedural information costs among young voters comes from Corvalan and Cox (2018) who investigate this phenomenon in the context of Chile. Using a regression discontinuity design around the registration deadline cutoff in Chilean elections, they find that Chileans who turn 18 after the registration deadline, but before Election Day have a significantly lower voter turnout than their peers whose birthday is right before that deadline. However, as the authors point out, Chile is particular in several respects: First, it is a very young democracy where incorrect beliefs about the population might be more prevalent than in an established democracy like the U.S. Second, the temporal gap between the registration deadline and the Election Day is much larger than in any U.S. state, being between 3 and 6 months compared to only a few days or weeks in the U.S. Third, up until a change to the election law, young people in Chile had to be 18 not be Election Day, but by the registration deadline already if they wanted to vote in an election (Corvalan and Cox 2018, 8). Thus, the

³In addition, the national context is likely to matter, too. The likelihood of first-time voters holding incorrect procedural beliefs is more plausible for countries like the U.S. where the electoral rules are more complex because voters have to register themselves (as opposed to automatic enrolment by the government) and where rules about how and when to register and vote may vary by state.

⁴See, for example, the Arizona voter guide at <https://www.azvoterguide.com/faqs/>. Corvalan and Cox (2018, fn7) find similar questions on the websites of 10 other U.S. states.

⁵For some illustrative evidence of these differences, see Table A.3 in the supporting information.

lower turnout for people turning 18 after the registration deadline in Chile might be the result of path dependence where some people are simply not aware of the changed rules to register in Chile. For these reasons, it is not clear to what extent the empirical evidence from Chile applies to other countries where this is different, such as the U.S.

Finally, not voting due to incorrect procedural beliefs in one election is likely to affect a person's propensity to vote in future elections. There are good reasons to assume that not voting in the present election also makes people less likely to vote in future elections. First, we know that voting is habit forming. People are more likely to vote in future elections for the mere fact of having voted previously, a phenomenon that is sometimes called "habitual voting" in the literature (Coppock and Green 2016; Fujiwara, Meng and Vogl 2016; Gerber, Green and Shachar 2003; Meredith 2009). Thus, a young person who fails to vote in the first election in which they are eligible to vote fails to make the first and crucial step toward establishing such a habit and is less likely to vote in future elections. Second, simply the fact of registering to vote early on and being added to a voter file may increase a person's likelihood to vote in future elections. Especially in countries such as the U.S. where people have to actively register to vote, registering produces some – albeit small – cost to voters. Thus, having already registered in the past could make it more attractive to people to vote in future elections. Related to that, the simple fact of being added to a voter file increases a person's likelihood to be contacted by future political election campaigns and thus, to be mobilized to cast a vote (cf. Nickerson 2015). For these reasons, any effect of incorrect beliefs that depresses voter turnout in the immediate election is likely to have long-run effects on the likelihood of a person to cast a vote in future elections.

This theory of procedural information costs leads me to formulate the following two hypotheses:

Incorrect procedural beliefs hypothesis: Young citizens who turn 18 after the registration deadline, but before Election Day are less likely to vote than otherwise identical young people who turn 18 before or on the registration deadline.

Persistence hypothesis: Young people who do not register or vote in their first election

due to incorrect procedural beliefs are less likely to vote in future elections.

2.3 Data

The empirical analysis in this study uses state voter records to calculate youth voter turnout in U.S. presidential elections. The advantage of these administrative data is that they avoid shortcomings of traditional survey-based analyses of voter turnout such as social desirability bias and sample sizes that are often too small to precisely estimate effects for societal subgroups. The voter files include registration information and voting histories for each single voter and were obtained directly from the Secretary of State offices for five states (Florida, New York, Ohio, Pennsylvania, Texas) and are taken from (Coppock and Green 2016) for 11 others (Arkansas, Colorado, Connecticut, Iowa, Illinois, Kentucky, Missouri, Montana, New Jersey, Nevada, Oklahoma, Oregon, Rhode Island). The statistical analyses focus on the larger of these states. Due to their larger population sizes, I can restrict my analysis to people who turned 18 shortly before or after the registration deadline for the 2008 U.S. presidential election and still get precise estimates of voter turnout and treatment effects for each individual state.

Despite its advantages, using voter file data for the study of voter turnout poses certain empirical challenges, above all, the risk of “differential registration bias” (DRB) (Nyhan, Skovron and Titiunik 2017). Such bias occurs if we estimate voter turnout based on the number of people registered in the voter file, but registration rates differ between treatment and control group. For example, as part of my analysis I am comparing citizens whose 18th birthday falls shortly before or after the registration deadline for the 2008 presidential election. If those marginally older were able to vote in one more presidential election, their registration rate is likely to be higher. Ultimately, DRB is a denominator problem because we usually lack the exact number of eligible voters needed to calculate voter turnout and as a consequence, have to rely on a proxy measure or alternative estimation strategies that do not require knowledge of the voting eligible population.

To avoid the risk of DRB, I adopt two strategies that have been successfully used in previous

research with voter file data. The first strategy uses birth data as a proxy for the unknown voting-eligible population (Meredith 2009; Nyhan, Skovron and Titiunik 2017). Birth data with the exact number of people born in a given state on a given day were accessed through the U.S. Center for Disease Control (CDC)’s National Vital Statistics website for people born before 1989. Due to more stringent confidentiality standards starting in 1989, data on daily birth counts for later years had to be requested from the respective Departments of Health in a given state. Birth data has two big advantages over alternative measures. First, it is temporally very fine-grained. In contrast to census data, we can collect daily birth counts and use them to calculate voter turnout by birthdate cohort, that is, for all people born on the same day. Second, birth counts are plausibly exogenous to the treatment of election eligibility. It is unlikely that a person’s date of birth could possibly have been affected by whether or not that person is eligible to vote in an election 18 years later. For these reasons political scientists have used it before to study socialization effects and procedural information costs with voter file data (Corvalan and Cox 2018; Meredith 2009; Nyhan, Skovron and Titiunik 2017).

However, one potential concern with birth data is that people move in and out of states before they turn 18. While in and out migration of states is likely to be more or less randomly distributed on both sides of the registration deadline discontinuity⁶ and thus, mere noise in our statistical analysis, I adopt a second strategy to retrieve the complier average causal effect (CACE) of procedural information costs on future voter turnout whose estimation is solely based on vote totals (Coppock and Green 2016). As a consequence, the estimation of the CACE, which I discuss below, is unaffected by differential registration rates between treatment and control group.

⁶To make sure that this assumption is not violated by young people graduating from high school and moving to other states for college, the analyses are almost exclusively based on states where the official school entry cutoff date is outside the RD estimation window that I use.

2.3.1 Visualizing Registration Deadlines and Turnout Patterns

This section provides a first, informal look at the data. In a first step, I will provide a more in-depth look at one particular state, Florida, over several election cycles while in a second step, I provide a cross-sectional overview of all 18 U.S. states for which I have voter file data in the 2008 presidential election. This serves to give us a better understanding of the nature of the discontinuity in turnout right at the registration deadline and of its prevalence both across U.S. states.

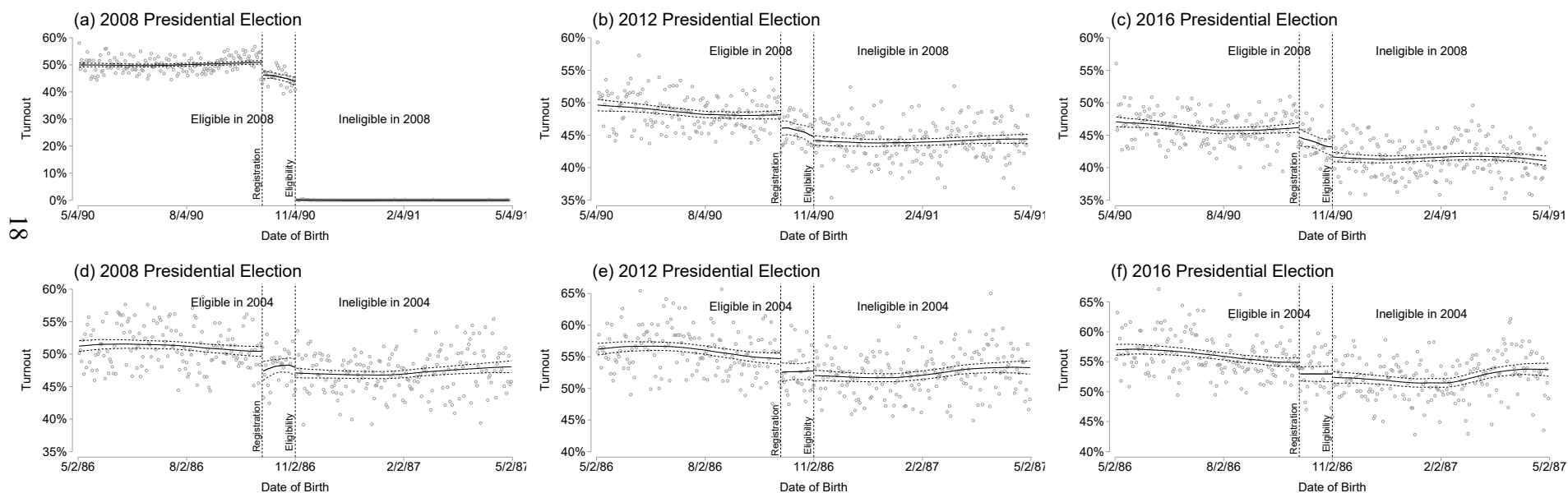
Figure 3.3 illustrates the effect of incorrect procedural beliefs on youth voter turnout for Florida for different birth cohorts and across different elections. Each graph plots voter turnout by birthdate cohorts for young Floridians in three U.S. presidential elections. The first row shows turnout for people who were born between May 4, 1990 and May 4, 1991, the second row does the same for those born four years earlier, between May 4, 1986 and May 4, 1987. In Figure 3.3a we can see a clear and stark drop in 2008 turnout directly at the registration deadline cutoff of October 7, 2008, with turnout for those turning 18 right after (i.e. to the right of) the registration deadline significantly lower than for those whose 18th birthday was a few days earlier. Interestingly, we find this discontinuity in turnout rates at the registration deadline cutoff not only for these people's first presidential election, but also in their subsequent presidential elections four and even eight years later (see Figures 3.3b-c). Comparing this with people who turned 18 four years earlier and who could vote for the first time in the 2004 presidential election, we find the same pattern: four, eight and even twelve years after the first election in which these citizens were eligible to vote, those who turned 18 right after the registration deadline of October 5, 2004 are significantly less likely to vote in presidential elections than their slightly older peers (Figures 3.3d-f). In contrast, the initially much larger discontinuity at the Election Day cutoff between eligible and ineligible voters in the 2004 and 2008 elections (see Figure 3.3a) produces a discontinuity in subsequent elections that, from graphical inspection, seems substantively smaller in size (Figures 3.3b-f). This suggests that in states where we find a clear discontinuity in turnout at the registration deadline, this dis-

continuity is temporally persistent over multiple elections and does not seem to be an isolated phenomenon restricted to a birth cohort or specific electoral context. Figure A.2 in the supporting information provides a graphical illustration of a similar discontinuity for presidential elections in Texas. It is also worth pointing out that this discontinuity is not a result of the way we measure turnout and persists if we replace the turnout measure with the daily vote totals (see Figure 4.2 below and Figures A.4 and A.5 in the supporting information).

To get an idea of the prevalence of these incorrect procedural beliefs and how they affect voter turnout across U.S. states, Figure 4.2 plots the total number of votes cast by birthdate cohort for people born May 4, 1990-May 4, 1991 for all 18 states for which I have voter file data. The advantage of using total votes over a measure that computes turnout among those registered in the voter file is that it avoids the potential pitfall of differential registration bias (Nyhan, Skovron and Titiunik 2017). DRB would be particularly problematic in this case because turning 18 right after the registration deadline is likely to reduce both a person's propensity to register and to vote, thus biasing a turnout measure based on the number of registered people. The total number of votes, on the other hand, is unproblematic because, despite some seasonal fluctuations in total eligible voters, there is no reason to expect a sudden drop in vote totals right at the registration deadline: Parents are unlikely to anticipate the registration deadline for their children 18 years later and to make the day of their child's birth dependent on that deadline. In order to empirically test these expectations, Figure A.1 in the Appendix plots the number of people who were born on each single day in Florida for the May 5, 1990 to May 4, 1991 period. We can see that in line with our expectations, the density of that variable is smooth around the registration deadline. At the same time, there is seasonal fluctuation in the number of births, with births being highest in September and lowest in May.⁷

⁷Given the typical day-of-the-week pattern of eligible voters per day, there is a much lower total number of votes for weekend days. For easier visual interpretation of the general trends around the registration deadline, graphs exclude data for people born on Saturdays and Sundays. Including data for these people does not affect the interpretation of our results at the registration deadline.

Figure 2.1: Turnout in Florida for People Born in 1986/87 and 1990/91



Each graph in Figure 4.2 fits three lines, one for each of the three groups we are interested in: pre-registration deadline eligibles to the left of the registration deadline, post-deadline eligibles in between the registration deadline and Election Day, and ineligible to the very right. Fitted lines use local mean-smoothing, with 95% confidence intervals.⁸ Results are for the 2008 presidential election.

Looking at the graphs for all 18 states, a clear pattern emerges. People turning 18 right after the registration deadline in their respective state are less likely to vote in the 2008 presidential election than those who turn 18 shortly before that deadline. For the majority of states there is a clear and statistically significant drop in the vote totals right after the registration deadline (e.g. Florida, Illinois, Texas). For four states (Kentucky, New York, Ohio, Pennsylvania) the drop in vote totals for post-deadline eligibles is relatively small and the confidence intervals overlap. Thus, we cannot be sure of a statistically significant difference between pre- and post-deadline eligibles in these states. There are only four states (Iowa, Montana, Nevada, Oregon) where there is no evidence for a procedural information effect on turnout at all, with daily vote totals in these states being roughly the same on both sides of the registration deadline. Note that two of these states, Iowa and Montana, enacted Election Day Registration (EDR) before the 2008 election.⁹ These laws allowed voters to both register and vote on Election Day itself, thus strongly reducing any registration costs to first-time voters in these two states. Overall, this pattern suggests that the discontinuity at the registration deadline is not limited to a few states, but indeed a common phenomenon across U.S. states.

Importantly for the plausibility of my causal story about the role of incorrect procedural beliefs, we can see that across states the drop in turnout is intimately linked with the registration deadline. In fact, this drop occurs right at the registration deadline and, very crucially, varies across states *in line with the specific registration deadline* in each state. Most states have a

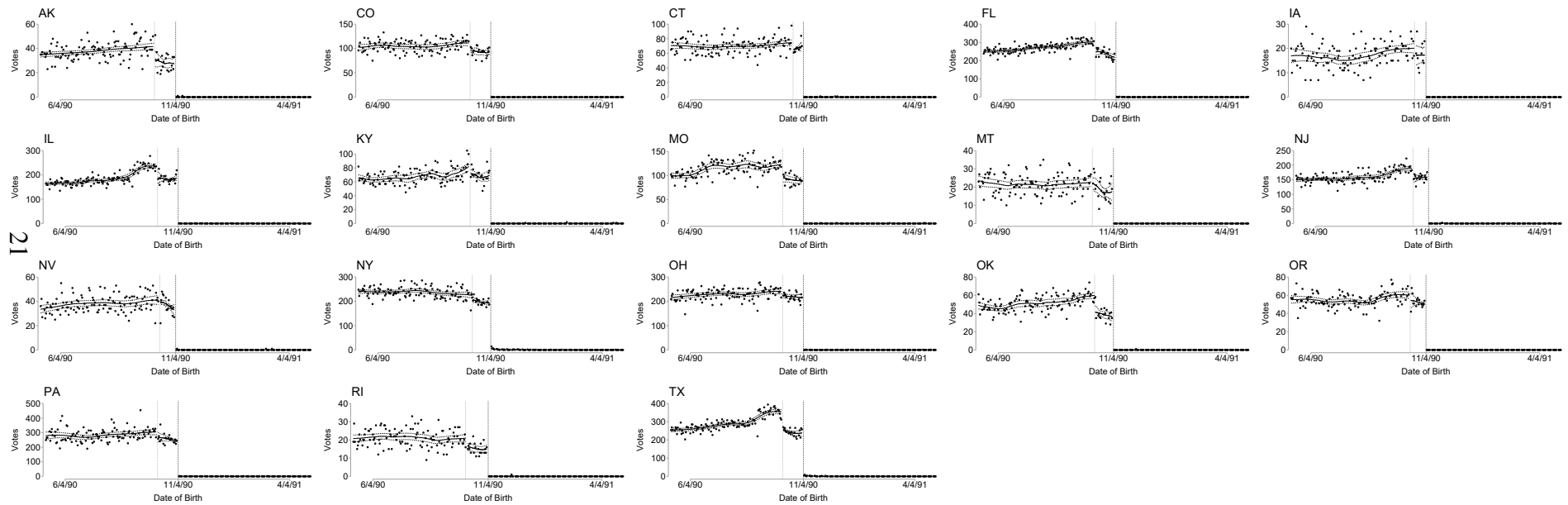
⁸I chose local mean-smoothing over fitted polynomial regression lines to allow for more flexibility. As a result, seasonal birth trends, possible confounding factors in a given state such as the school entry cutoff date or the short window between registration deadline and Election Day are less likely to affect the discontinuity at the registration deadline.

⁹Montana and Iowa enacted EDR in 2005 and 2007 respectively and are the only states in my sample with EDR in 2008.

registration deadline of 28 days and, as we would expect, turnout drops exactly for those who have their 18th birthday 28 days before Election Day (e.g. Florida, Illinois, Texas). However, the drop occurs after only 25 (e.g. Oklahoma), 21 (e.g. New Jersey) or even 14 days (e.g. Connecticut) in some states where the registration deadline is 25, 21 or 14 days before Election Day, respectively. In contrast, there is no drop in turnout before Election Day in Iowa and Montana, the only two states with Election Day Registration in 2008 in my sample. This further bolsters our confidence that it is the registration deadline - and not something else - that has to be responsible for the drop in voter turnout.

Interestingly, there is a second drop in the total number of votes in some states (e.g. Texas, Illinois) to the left of September 1. This date coincides with these states' school entry cutoff date in the 1990s. It suggests that those who turned 18 before September 1, 2008 and thus, who are more likely to have finished high school by the time of the 2008 election are less likely to vote in that election (see also Figure [A.2](#) in the supporting information). To avoid confounding due to this additional discontinuity, the statistical analyses exclude people born before September 1, 1990 and focus exclusively on people whose 18th birthday is within a one-month window around the registration deadline.

Figure 2.2: Registration Deadline Discontinuities Across U.S. States in 2008 Election



Note: Figures plot total number of votes by birthdate cohorts.

2.4 Identification Strategy

To test the previously specified hypotheses about procedural information costs, I will estimate the effect of beliefs about registration requirements, in particular the effect of erroneously thinking that one is ineligible to vote because one’s birthday is after the registration deadline, on current and future rates of youth voter turnout. The dependent variable in the empirical analyses is voter turnout, measured either at the individual level as whether or not a person in the dataset cast a vote or aggregated by birthdate cohort, that is, for people born on the same day, as described below. Individuals within a narrow window on either side of the registration deadline are as-if randomly assigned as to whether or not they face higher costs to voting due to uncertainty about whether they have to be 18 by the registration deadline or by Election Day. This allows me to use a regression discontinuity design to estimate the treatment effect of these procedural information costs on voter turnout. Given the large size of the dataset, I am able to treat the RD as a “local randomized experiment” (Cattaneo, Titiunik and Vazquez-Bare 2017; Cattaneo, Idrobo and Titiunik 2018; Dunning 2012; Nyhan, Skovron and Titiunik 2017), that is, I restrict the estimation to observations within a narrow window around the RD cutoff and use difference-in-proportions and a Wald estimator to retrieve the treatment effect. Thus, the empirical analysis primarily focuses on young Americans born September 10-November 4, 1990, that is, people who turned 18 within a one-month window on either side of the registration deadline for the 2008 presidential election in their respective state.¹⁰ Additional age cohorts are constructed analogously. The one-month (or, more precisely, 25 or 28-day window) is the maximum temporal distance between the registration deadline and the Election Day and thus, maximizes efficiency while avoiding bias due to confounding.¹¹

In a first step, I compute differences in proportions in voter turnout between those who turned 18 right after the 2008 registration deadline (i.e. October 7, 2008) and those who turned

¹⁰For comparison, Coppock and Green (2016)’s study on habitual voting uses a much larger 12-month window on either side of the RD cutoff.

¹¹For example, our estimates would potentially be confounded if the window was larger and included the Election Day or the school entry cutoff date, which might have an independent effect on voter turnout.

18 right before it for both the 2008 and 2012 U.S. presidential elections. The difference between these two groups captures the average treatment effect (ATE) of procedural information costs on turnout in each state. To avoid differential registration bias (Nyhan, Skovron and Titiunik 2017), this part of the empirical analysis calculates turnout by using total daily birth counts as a proxy for the voting eligible population in a given state.

In a second step, I estimate the complier average causal effect (CACE) that being exposed to these procedural information costs in a person's first election has on their propensity to vote in future elections. While we cannot randomly assign the non-voting as a result of procedural information costs, we can get at it by leveraging a natural experiment which as-if randomly increases the procedural information costs and thus, discourages some young people from voting. Both among those who turn 18 right before and among those who turn 18 right after the registration deadline in 2008, some are likely to think that a person has to be 18 not by Election Day, but by the registration deadline in order to vote in that election. However, this incorrect belief only materializes and affects the behavior of those who turn 18 after the registration deadline while it is inconsequential for those who turn 18 before that date. As a result, only the former experience increased procedural information costs: if they are unclear about their eligibility status in that election, they would have to take additional steps to find out, which are not necessary for those who turn 18 a few days earlier.

However, not all eligible voters who turn 18 after the 2008 registration deadline hold these incorrect beliefs and thus, are exposed to the treatment, that is, these higher procedural information costs. In fact, the treatment is restricted to those who hold these incorrect beliefs and who, as a result of holding them, do not vote in 2008. In other words, they are the compliers in this study. They are the ones who, due to higher procedural information costs, are prevented from registering and voting in 2008 and thus, are less likely to vote in future elections. To be clear, the claim here is that some of them do not register and vote and thus, are less likely to vote in future elections *only* because they erroneously believe that they are not eligible to vote in their first election.

To estimate the complier average causal effect (CACE), I use a Wald estimator (Angrist

and Pischke 2009). Following (Coppock and Green 2016) I call the procedural information costs $Z \in \{0, 1\}$, with 1 indicating that a person is exposed to a discouragement to vote in an election due to higher procedural voting costs and 0 otherwise. The estimand of interest is “voting in an upstream election V_1 on voting in a downstream election V_2 ” (Coppock and Green 2016, 1046).

My approach rests on four key assumptions (Angrist and Pischke 2009). First, Z is as-if randomly assigned at the registration discontinuity and thus, independent of potential outcomes. Second, to satisfy the exclusion restriction, Z has to affect V_2 only through its effect on V_1 . In other words, I assume that the only reason a person who was discouraged from voting in 2008 due to higher procedural information costs is less likely to vote in 2012 is because of her not voting in 2008. Third, Z impacts the endogenous regressor V_1 , an assumption that can be directly tested through the first stage of the Wald estimator. Fourth, the monotonicity assumption requires that those who are responsive to the treatment respond to it in the same way.

Defining the Average Upstream Treatment Effect (AUTE) as $E[V_{1i}(1)] - E[V_{1i}(0)]$ and the Average Downstream Treatment Effect (ADTE) as $E[V_{2i}(1)] - E[V_{2i}(0)]$,¹² the following estimator gives us the complier average causal effect (CACE) (Coppock and Green 2016, 1046-1047):

$$C\hat{A}CE = \frac{\hat{E}[V_{2i}|Z_i = 1] - \hat{E}[V_{2i}|Z_i = 0]}{\hat{E}[V_{1i}|Z_i = 1] - \hat{E}[V_{1i}|Z_i = 0]} = \frac{ADTE}{AUTE} \quad (2.1)$$

Given that voter files contain only those who are registered, I avoid differential registration bias in computing the CACE by following Coppock and Green (2016)’s strategy of using the total number of votes cast by birthdate cohort as dependent variable. I include the daily vote totals lagged by a year on the right-hand side of the equation to account for seasonal or day-of-the-week fluctuations in the number of eligible voters. This gives us a Wald estimator with the

¹² $V_{1i}(1)$ and $V_{2i}(1)$ are unit i ’s treated, $V_{1i}(0)$ and $V_{2i}(0)$ untreated potential outcomes.

following two-stage form:

$$UpstreamVotesCast_j = \gamma_0 + \gamma_1 Z_j + \gamma_2 LaggedDownstream_j + \rho_j \quad (2.2)$$

$$DownstreamVotesCast_j = \beta_0 + \beta_1 Upstream\hat{V}otesCast_j + \gamma_2 LaggedDownstream_j + \epsilon_j \quad (2.3)$$

Equation 3.2 displays the first stage. The total number of votes by birthdate cohort in the upstream election (V_j) are regressed on a treatment indicator Z_j and $LaggedDownstream_j$, the dependent variable lagged by one year. In the second stage shown in equation 3.4, Y_j is the total number of votes by birthdate cohort in the downstream election and \hat{V}_j are the predicted values from the first stage. j indexes birthdate cohorts.

2.5 Results

Table 2.1 provides statistical estimates of the effect of erroneously believing one has to be 18 to register to vote on voter turnout for five large U.S. states. I focus my analysis on people born September 10-November 4, 1990, that is, people turning 18 within a 28-day window on either side of the registration deadline. The results in the top part of the table are from the 2008 U.S. presidential election. We can see that in four of the five states there is a significant drop in turnout for those turning 18 after the registration deadline compared to those having their birthdays before October 7, 2008. This drop is largest for Florida, Illinois and Texas, three states for which the graphical analysis above (see Figure 4.2) revealed a clear discontinuity in turnout at the registration deadline. The estimated treatment effect is -6.72 percentage points for Florida and -8.89 percentage points for Texas, showing that post-registration deadline eligibles who might erroneously think they are not eligible to vote in 2008 have a much lower turnout in 2008 (45.24% for Florida and 25.39% for Texas) than pre-deadline eligibles (51.69%

and 34.27% respectively). We find a similar, but smaller effect of -2.27 percentage points for New York and -3.48 percentage points for Pennsylvania, two states where our graphical analysis above already suggested that the effect is smaller. Finally, Ohio is the only state among these six where voter turnout between pre- and post-registration deadline eligibles is roughly the same, at about 46.85%, suggesting that post-registration deadline eligibles in this state are generally better informed about the age requirement for registering to vote and voting.

The middle section of Table 2.1 presents estimates for the same group of people (i.e. 2008 eligibles), but for the 2012 presidential election four years later, that is, the downstream effect of 2008 erroneous beliefs on 2012 turnout. We can see that for Florida and Texas, the two states with the largest effect in the 2008 election among the five states included in Table 2.1, turnout for post-registration deadline eligibles is still significantly lower than for pre-registration eligibles. In Florida turnout for the former is 48.10% while it is -2.59 percentage points lower (45.52%) for those who have their 18th birthday right after the registration deadline. In Texas the difference in turnout between these two groups is -1.74 percentage points. For Illinois, New York and Pennsylvania, 2012 turnout for those who turned 18 right after the 2008 registration deadline is now only marginally lower to that of pre-registration deadline eligibles, at -1.05, -0.30 and -0.91 percentage points respectively, a difference that is no longer statistically significant at the 95% level. As for Ohio, the difference in 2012 turnout between these two groups is a positive 1.50 percentage points, suggesting that post-registration deadline eligibles do not have a lower, but in fact a higher turnout rate (40.11%) than pre-deadline eligibles (38.61%) in this state.¹³ Furthermore, all five states have similar registration rules - voters have to register roughly a month before Election Day and same-day-registration¹⁴ is not possible - and we find large effects both in “battleground” (Florida, Pennsylvania) and “non-battleground” states (Texas). This further bolsters our confidence that the effect can be attributed to erroneous be-

¹³A possible explanation for this surprising finding is that results for Ohio might be affected by that state’s late school entry cutoff date of September 30, which falls clearly within the one-month window to the left of the registration deadline that I used for the analysis (see Figure A.3 in the supporting information for further discussion).

¹⁴Same-day-registration allows voters to register and vote in one single step on Election Day.

liefs about the age requirement to register and vote in these states and not to differences in registration rules or the competitiveness of the election across states.

Finally, the voter file for Florida also shows whether a person voted in the 2016 presidential election. As we can see in the bottom section of Table 2.1, 2008 eligibles who turned 18 after the 2008 registration deadline are still significantly less likely to vote eight years after they were first eligible to vote in a presidential election than otherwise identical citizens who turned 18 a few days earlier. Based on the results in Table 2.1, their probability to cast a vote is - 2.45 percentage points lower. Interestingly, the size of the effect is practically identical to the effect four years earlier, suggesting that not voting in 2008 due to misperceptions about the registration requirements in that election has long-term effects, deterring some of these people from participating in future elections. Importantly, these results are generally robust both to varying bandwidths around the registration deadline and to different model specifications (see Tables A.1 and A.2 in the supporting information).¹⁵

2.5.1 Persistence of Effects

The results from Table 2.1 above illustrate the existence and size of the effect that incorrect beliefs about the registration requirements have on voter turnout. However, they do not give us a direct estimate of the effect that being discouraged from voting in 2008 due to these informational hurdles has on a person's propensity to abstain from voting in future elections. This complier causal average effect (CACE) of being deterred from voting in the upstream election (i.e. 2008) on turnout in a downstream election (i.e. 2010 or 2012) is reported in Table 2.2. As explained above, the coefficient on the registration beliefs variable is the ratio of the 2010 or 2012 average downstream treatment effect over the 2008 average upstream treatment effect. Multiplying this ratio by 100 gives us the percentage point difference between treated

¹⁵For most states, the size of the effect is largely unaffected by using the shorter 10 day window on either side of the registration deadline (see Table A.1 in the supporting information). Note, however, that for New York, results are sensitive to bandwidth selection and further research is required to unambiguously establish the existence of the effect identified in Table 2.1 for this state.

Table 2.1: Turnout in U.S. Presidential Elections for 1990 Birth Cohorts

	(FL)	(IL)	(TX)	(NY)	(OH)	(PA)
Registration Deadline Discontinuity						
2008						
Eligibles post-deadline	45.24	35.41	25.39	29.47	46.87	39.75
Eligibles pre-deadline	51.96	40.87	34.27	31.73	46.83	43.23
Difference	-6.72	-5.46	-8.89	-2.27	0.04	-3.48
	[-7.81, -5.62]	[-7.23, -3.68]	[-9.68, -8.09]	[-3.12, -1.41]	[-1.18, 1.26]	[-4.66, -2.29]
N	32,084	28,859	50,727	45,540	25,627	26,695
2012						
Eligibles post-deadline	45.52	30.16	24.91	29.62	40.11	35.82
Eligibles pre-deadline	48.10	31.21	26.65	29.92	38.61	36.73
Difference	-2.59	-1.05	-1.74	-0.30	1.50	-0.91
	[-3.68, -1.49]	[-2.86, 0.76]	[-2.50, -0.97]	[-1.15, 0.55]	[0.30, 2.70]	[-2.07, 0.24]
N	32,084	28,859	50,727	45,540	25,627	26,695
2016						
Eligibles post-deadline	43.93					
Eligibles pre-deadline	46.38					
Difference	-2.45					
	[-3.54, -1.36]					
N	32,084					

Note: Table presents difference-in-proportions with 95% confidence intervals in brackets for young Americans born in 1990. Each estimation uses a 28-day-window (25 for NY) on either side of the registration deadline in 2008.

and untreated compliers.¹⁶ The table presents results for the five states from Table 2.1 above for which erroneous beliefs about the registration requirements matter. It does no longer include Ohio, for which I failed to find any effect of incorrect procedural beliefs on 2008 voter turnout among post-registration deadline eligibles.

Based on the top part of Table 2.2, we can see that independent of the initial size of the effect in 2008 that I identified above, not voting in 2008 increases a person's likelihood to not vote in the 2012 election four years later. For all five states the effect is statistically significant at the 95% confidence level and substantively large, ranging from 0.174 in New York to 0.514 in Pennsylvania, with the values for the other three states falling in between this range. A \widehat{CACE} of 0.425 in Florida, for example, indicates that being discouraged from voting in 2008 because a person erroneously thinks she is not eligible to vote in that election increases her

¹⁶The dependent variable in Table 2.2 is the number of people for each birthdate cohort (i.e. people who are born on the same day) who voted in either 2010 or 2012. Therefore, the constant gives the number of people who are born on the same day and who voted in 2010 or 2012 when all other variables are set to 0.

probability of not voting in 2012 by a full 42.5 percentage points.¹⁷

Luckily, the data in the voter file for the state of Florida allows me to analyze an even more extended period of time and observe the voting behavior of the same group of people over more than two presidential elections. Based on the results in the last column of the table, we can see that not voting in the 2008 election due to incorrectly believing one is ineligible increases the probability to not vote in the 2016 presidential election 8 years later by 37.1 percentage points. This pattern is consistent with our findings in Figure 3.3 above which showed that both the 2004 and the 2008 cohorts of post-registration deadline eligibles are less likely to vote in presidential elections eight (i.e. in 2012 and 2016, respectively) or even twelve years later (i.e. in 2016 for the 2004 eligibles). After an initial drop between the first and the second presidential election, the effect seems to stabilize and persist over time.

Results from the bottom part of Table 2.2 report the downstream effect of being deterred from voting in the 2008 presidential election on voting in the 2010 midterm election. While effects are somewhat smaller, the pattern is largely the same as before. Across all five states the effect of being discouraged from voting in the 2008 election strongly and consistently increases the probability of not voting in the 2010 midterm election. The smallest effect is still a remarkable 10.1 percentage points in Texas and the largest effect is 22.7 percentage points in New York. These $CACE$ s are statistically significant at $p < 0.05$ for all states except Pennsylvania, whose estimate has a p-values of 0.09.¹⁸

Overall, these results suggest that young people who incorrectly think they are not eligible to vote in their first presidential election due to their birthday falling after the registration deadline and who, as a consequence, fail to vote in that election are significantly less likely to

¹⁷Consistent with the assumption that post-deadline eligibles are less likely to vote in 2008 than pre-deadline eligibles, the instrument of procedural voting costs is negatively correlated with voting in the upstream election in the first-stage results (not reported). This association is statistically significant for all Wald estimators reported in this study.

¹⁸Further analyses (results not reported) fail to produce a similar long-term effect for upstream midterm elections on downstream midterm or presidential elections. For example, incorrect beliefs in the 2006 midterm election only depress turnout in 2006, but do not produce a significant drop in turnout in the 2008 and 2010 elections. This suggests that incorrect procedural beliefs are more consequential for first-time eligible voters in presidential than in midterm elections.

Table 2.2: CACE of 2008 Incorrect Procedural Beliefs on Downstream Voting

	(FL)	(IL)	(NY)	(PA)	(TX)	(FL)
Presidential on Presidential						
	2008 - 2012					2008 - 2016
Registration Beliefs	.425* (.114)	.280* (.121)	.174* (.082)	.514* (.114)	.265* (.050)	.371* (.134)
Lagged DV	.424* (.143)	.411* (.153)	.667* (.088)	.317* (.127)	.454* (.089)	.471* (.158)
Intercept	37.365* (15.756)	35.472* (12.606)	35.601* (16.776)	17.333* (10.901)	55.554* (12.805)	34.111* (17.146)
N (Days)	56	56	56	56	56	56
Presidential on Midterm						
	2008 - 2010					
Registration Beliefs	.167* (.038)	.227* (.044)	.191* (.067)	.160 (.094)	.101* (.027)	
Lagged DV	.221 (.120)	.255 (.144)	-.174 (.154)	.151 (.178)	.240* (.086)	
Intercept	15.782* (5.298)	4.979 (6.434)	5.022 (10.142)	22.445 (19.161)	29.005* (6.049)	
N (Days)	56	56	56	56	56	

Note: Table presents results from a Wald estimator (second stage). Estimations use a 25 (NY) or 28-day-window (FL, IL, PA, TX) on either side of the cutoff. *p<.05.

vote in future elections. As the $CACE$ s for various states show, these longer term effects are substantively large. They are significantly larger than similar effects of previous non-voting on future voter turnout that have been identified in the literature for people who turn 18 around the Election Day (see, for example, Coppock and Green 2016). However, we have to keep in mind that the effect works on a relatively narrow group of people: those who are deterred from voting because they turn 18 between the registration deadline and Election Day, and who erroneously think they are not eligible to vote in their first presidential election. The evidence in this study suggests that this specific group of people benefits a lot from being added to the voter rolls early on and also possibly from casting a vote in their first election. Preventing these people from doing so turns many of them into non-voters in future general elections. We can assume that many of these very same people would have voted in their first and subsequent elections, had they known that they were eligible to register while still 17.

2.5.2 Alternative Explanations: Formal Rules and Concurrent Administrative Acts

How certain can we be that the drop in voter turnout that we observe right at the registration deadline is really due to young people's incorrect beliefs about the age requirement to register? Given that this study uses observational data, the credibility of the causal story about incorrect beliefs crucially depends on me being able to rule out alternative explanations that could plausibly account for the sudden drop in voter turnout among those who turn 18 right after the registration deadline. Key among them are the following two: (1) formal rules that prevent 17-year-olds from registering and (2) concurrent administrative acts that make young people more likely to register on their 18th birthday due to lower registration costs.

First, the drop in turnout for post-registration deadline eligibles could be due not so much to their beliefs that they are ineligible to register and vote, but rather to actual rules or administrative practices that prevent this group of people from or make it much more difficult for them to register and vote in their first election in some states. However, such an explanation would go against the stipulations of the 26th Amendment of the U.S. Constitution, which grants U.S. citizens "18 years of age or older" the right to vote. Accordingly, most state election codes or state voter registration forms explicitly state that citizens have to be 18 by Election Day, not by the registration deadline to register to vote. Even in states where the wording in official documents may be less clear about the specific age that is required to register (e.g. Florida, Idaho, Oklahoma, Texas – see Table A.3 in the supporting information), election officials are readily available to provide clarification. For example, employees at Secretary of State and County Supervisor of Election offices in Florida and Texas consistently confirmed to me that an otherwise eligible person is able to register in these states even before she turns 18 as long as she will be 18 by Election Day.¹⁹ Thus, legal rules and their enforcement by election officials seem unlikely to be able to explain the sharp drop in voter turnout at the registration deadline.

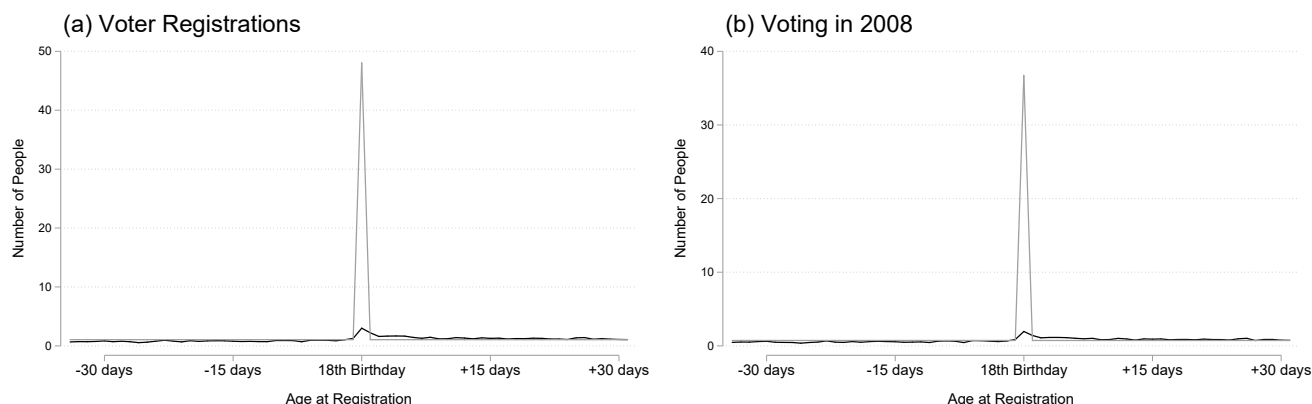
¹⁹Personal communication by phone with the Florida and Texas Secretary of State and several Florida County Supervisor of Election offices (Alachua, Baker, Bay, Bradford, Brevard, Broward, Miami-Dade, Leon, Orange, Palm Beach) on December 12, 2018.

Second, it is possible that another administrative act that young people tend to perform right on their 18th birthday significantly reduces registration costs, makes them much more likely to register on that day and thus, increases their likelihood to vote. For example, if a large number of young people were to get their driver's license exactly on the day of their 18th birthday and, at the same time, registered to vote at the DMV office, this could explain the abrupt drop in turnout for those who turn 18 right after the registration deadline. Fortunately, we can use the voter file data to test the plausibility of this alternative hypothesis empirically. First, I create a counterfactual of the number of young people who would have registered on the day of their 18th birthday if their 18th birthday had not been affected at all by the registration deadline. Second, I compare this counterfactual number to the number of people who would have had to register on their 18th birthday in order to account for the stark drop in turnout right at the registration deadline.

To estimate the number of people who register on their 18th birthday due to a concomitant administrative act, I analyze the registration behavior of young Floridians who turn 18 between February 8 and August 7, 2008. The advantage of focusing on this group of people for the counterfactual is that their decision to register on their 18th birthday is less likely to be influenced by other factors such as incorrect beliefs about the registration age. In contrast, those who turn 18 in September or early October 2008 are not suited for this purpose because their decision to register could primarily be a result of their incorrect beliefs that they have to wait until their 18th birthday to register, but have only few days left to do so before the registration deadline. Thus, finding a high number of people who register exactly on their 18th birthday among this group of citizens does not allow us to differentiate between the role of incorrect beliefs and administrative acts that citizens perform on the same day because both make them more likely to register on the day they turn 18. In contrast, for people whose 18th birthday is several months before the registration deadline, there is less reason to assume that incorrect beliefs are the main reason why many of them register exactly on the day they turn 18. Registering a few days or even weeks later has no apparent disadvantage to them.

The black line in Figure 2.3(a) plots the average number of young Floridians for a single

Figure 2.3: Registration Timing for Floridians born Feb 4-Aug 3, 1990



birthdate cohort who register 30 days before and after their 18th birthday. Of roughly 393 people who are born on the same day and who registered by the 2008 election, relatively few people registered on each single day around their 18th birthday. The average number is between 1 and 2 registrants per day, with the number of registrations increasing to 3 on the day of the 18th birthday itself. Thus, while people are somewhat more likely to register on the day they turn 18, this increase affects only a very small number of people. Can this difference in 1 to 2 more people who register on their 18th birthday account for the drop of between 6 and 7 percentage points in turnout²⁰ for those who turn 18 right after the registration deadline? The answer is no. It cannot explain the drop of 6 to 7 percentage points, which corresponds to a drop of roughly 36 votes – from an absolute number of 278 to 242 votes – for the average birthdate cohort after October 7, 2008 (see Figure 2.3(b)). The grey line in Figure 2.3(a) illustrates this graphically: it shows the number of people who would have had to register on each single day in order to account for the drop in the registration rate at the registration deadline between pre- and post-deadline eligibles.²¹ As we can see, the spike on the day of the 18th birthday for the grey line is much larger (at 48 required registrants) than for

²⁰See Table 2.1 above and Table A.1 in the supporting information.

²¹The registration rate is calculated by dividing the number of people registered by the 2008 election over those eligible to register, by birthdate cohort. Given that we do not know the voting-eligible population, I use the number of people born on the same day as a proxy for those eligible to register. The drop in the registration rate between pre- and post-deadline eligibles is 8.7 percentage points or, in absolute numbers, roughly 47 fewer registrations for each post-registration deadline birthdate cohort.

the black line (at only 3 estimated registrants), showing that the registration behavior of young Floridians (black line) is inconsistent with a causal story that attributes the drop in turnout to a concomitant administrative act that young people perform on their 18th birthday. In other words, neither formal rules nor concurrent administrative acts can explain the pattern in turnout that I identified above.

What role do social mechanisms play in this context? Most of those who turn 18 shortly before or after the registration deadline will still live at home. Yet, children at home might experience a significant social shock when they turn 18 that might include comments from their relatives about their eligibility and possibly even encouragements to vote in the impending election. This might make them more likely to register and vote in that election. Can this social mechanism possibly explain most of the difference in turnout between pre- and post-registration deadline eligibles? Like with the other alternative explanations before, this social mechanism can only account for the strong drop in turnout right at the registration deadline if it leads young people who turn 18 before the registration deadline to register exactly on the day of their 18th birthday.²² However, this is not the case. A look at the number of people in Florida who turn 18 shortly before the registration deadline reveals that they are not more likely to register on their 18th birthday than on any other day shortly preceding or following their birthday.²³ Therefore, this social mechanism is unable to account for the drop in turnout right at the registration deadline.²⁴

Finally, it is possible that voter mobilization campaigns might be responsible for the differ-

²²Instead, if it only made them more likely to register a few days before or after their birthday, we would not expect to see a discontinuity right at the registration deadline, but rather a smooth decline in turnout around that date.

²³For example, for those who turned 18 one day before the registration deadline, two people registered on their birthday and 3, 7, 7 and 2 people on each of the four days directly preceding their birthday. For those who turn 18 two days before the deadline, two people registered on their birthday and 12, 10, 6 and 12 people on each of the previous four days. Note that these numbers are higher than those in Figure 2.3 above for the simple reason that, independent of the date they turn 18, citizens are more likely to register to vote shortly before a registration deadline.

²⁴Note that this does not preclude the possibility that incorrect beliefs about the registration deadline eligibility may be amplified or dampened by this social mechanism. To the extent that relatives might provide correct (or incorrect) information about the registration requirements for the upcoming election, this might lower (or increase) procedural information costs for young voters.

ence in turnout between pre- and post-registration deadline eligibles. This would be the case if campaigns that are aimed at increasing turnout among first-time voters specifically target young citizens who turn 18 before the registration deadline. These mobilization campaigns work under financial constraints and might focus their efforts primarily on people who they know are already 18 and thus, eligible to vote in order to make the most efficient use of their resources. However, it seems highly unlikely that these campaigns would all – independent of the state they are run in – specifically target people who happen to turn 18 right before the registration deadline.

For all these reasons, incorrect beliefs about the procedural registration requirements are the most likely explanation for the sharp drop in turnout among Americans who turn 18 right after the registration deadline.

2.6 Conclusion

Not only formal institutional rules, but also beliefs about these rules matter for voter turnout. Combining RD designs with voter file data can be a powerful empirical strategy to convincingly evaluate the impact of these procedural beliefs on youth voter turnout. This study leverages a discontinuity in voter turnout among young Americans depending on whether they turn 18 a few days before or after the voter registration deadline in their respective state for the first presidential election in which they are eligible to vote. Doing so, I am able to show that incorrect procedural beliefs about whether one has to be 18 by the registration deadline or by Election Day can decrease turnout by up to a very noticeable 9 percentage points (or 25%). Crucially, incorrect procedural beliefs do not only depress voter turnout in the immediate election, but have long-lasting effects on those young people who are affected by them. In Florida 37% of young Americans who were deterred by these beliefs from registering and voting in 2008 did not vote in the 2016 presidential election 8 years later although they would have done so, had they not incurred these higher procedural information costs in 2008. What is more, alternative explanations - in particular, concurrent administrative acts such as getting one's driver's license

and registering at the same time at the DMV office - are not able to account for this drop in turnout, bolstering our confidence that it is actually beliefs and not something else that matters most in this case.

However, one caveat applies to the quasi-experimental research design used in this study. While it provides compelling evidence for the effect of procedural beliefs on voter turnout, it is based on observational data and as such, does not allow for a direct manipulation of young voters by the researcher. In other words, the strength of my empirical approach relies on me being able to plausibly rule out potential alternative explanations. In particular, I have shown that two competing hypotheses – formal rules requiring young people in some states to be 18 to register and administrative acts that Americans may perform when they turn 18 and that make them more likely to register – are unable to explain the sharp drop in voter turnout right at the registration deadline. Yet, a direct test of the effect of procedural beliefs on voter turnout would require an experimental (or field experimental) research design where the researcher is able to assign young citizens to different degrees of procedural information. Given the increased concern about the degree of misinformation in U.S. election campaigns, this represents a promising route for future research.

Two implications flow from my findings. First, a methodological implication suggests that future studies should more explicitly exploit temporal discontinuities in order to study procedural information costs. In many areas that political scientists care about – from welfare state benefits to a person’s citizenship or immigration status, just to name a few – eligibility depends on a temporal discontinuity: a person’s age, time of residence in a country, duration of employment, etc. Given that incorrect beliefs about these other deadlines are not less likely than they are for voting behavior, exploiting these temporal discontinuities may provide important insights into the extent to which erroneous beliefs limit program uptake of welfare state programs or citizenship rights.

A second implication is more policy-oriented. Citizens are often faced with a complex set of rules that adds an additional burden on them. This may deter especially low-information citizens – that is, in many cases, the very group of people that may be the primary target of a

government intervention – from enrolling into a program or registering for benefits that they are eligible for. A common strategy in these cases is to either ignore the problem or to address it by increasing the financial incentives for program uptake or to start a costly promotion campaign. Another possible solution that the findings from this paper suggest is to simplify the rules, a strategy that has the combined advantage of likely increasing program uptake without requiring governments to spend additional substantial amounts of funding.

Chapter 3

The Electoral Consequences of Increasing Voter Turnout: New Evidence from a Natural Experiment

3.1 Introduction

Does increasing voter turnout help shift the balance of power from right-wing parties to those of the political left? A large literature in political science has produced consistent evidence that shows substantial inequalities in political participation across societal subgroups in advanced industrial democracies: the poor, the less educated, the young, and ethnic minorities are generally significantly less likely to vote in elections than the average citizen (Brady, Verba and Schlozman 1995; Kasara and Suryanarayan 2015). Yet, whoever does not vote in elections is also less likely to be represented politically. However, if the voices of poor people are not heard by politicians, this is likely to perpetuate or even increase existing economic inequalities. It is this direct relationship between political and economic inequality that Lijphart (1997) had in mind when he talked about persistently low voter turnout among the poor in advanced industrial democracies as “democracy’s unresolved dilemma” in his 1996 *American Political*

Science Association (APSA) presidential address.

One of the most straightforward ways to solve this dilemma – and one that was proposed by Lijphart (1997) himself – is to increase voter turnout among the poor and other politically disadvantaged groups by making voting compulsory for every voting eligible citizen. In particular with respect to socioeconomically worse off citizens, the logic runs as follows: 1) Poor people are generally less likely to vote. 2) Making voting compulsory will disproportionately increase their share among those who cast a vote. 3) Given poor people’s policy preferences for more redistribution, they are likely to vote for left-wing parties, thus increasing the vote and seat shares of these parties. 4) Once in power, these left-wing parties will adopt redistributive policies that will disproportionately benefit the poor (cf. Lijphart 1997; Meltzer and Richard 1981).¹ While initial empirical analyses produced somewhat mixed results about the political consequences of compulsory voting, especially recent empirical evidence that uses sound causal identification strategies seems to corroborate the assumed causal link between higher voter turnout and the electoral support for left-wing parties and policies (Fowler 2013; Bechtel, Hangartner and Schmid 2016; Carey and Horiuchi 2017).

However, despite this compelling account of how increasing turnout through compulsory voting might lead to more electoral support for left-wing parties, several concerns are warranted. First, the assumed causal mechanism that links voter turnout and left-wing party support fundamentally rests on the assumption that voting patterns of poor people who did not vote previously will be similar to the average poor person once they are compelled to cast a vote. Yet, Selb and Lachat (2009) and Singh (2019b) show that non-voters who are compelled to vote tend to be more politically disaffected, less informed about politics and less politically interested. Thus, the extent to which these people are similar to other poor people who used to vote in previous elections and really will cast their vote for left-wing parties is unclear. Second, if we assume that parties behave strategically and want to maximize their electoral gains

¹Given decreasing levels of voter turnout (Franklin 2004) and increasing economic inequality in many advanced industrial democracies (e.g. Atkinson and Piketty 2007; Bartels 2008), it is not surprising that we have recently seen a renewed interest in this electoral reform among both scholars of voting behavior (Bechtel, Hangartner and Schmid 2016; Singh 2019a,b) and political theorists (Chapman 2019; Umbers 2018).

(e.g. Downs 1957), the introduction of compulsory voting is likely to affect partisan strategies. Right-wing parties might respond to an electoral reform that disproportionately increases turnout among poorer voters by shifting their policy positions to the left. In addition, the introduction of compulsory voting is likely to directly affect partisan competition within electoral districts. Candidates who previously thought they did not stand a good chance to win might reconsider their decision and run a campaign if they know that, despite possibly smaller chances of winning, their own supporters are compelled to cast a vote. These general concerns are supported by new research that finds that the empirical evidence is more mixed. Two recent quasi-experimental studies have used convincing, sound causal identification strategies and applied them to new contexts, but failed to find empirical support for the assumed relationship between turnout and left-wing support (Ferwerda 2014; Miller and Dassonneville 2016).

Given these mixed findings, this paper presents a new perspective on the political participation-left vote relationship. It takes into account the greater political disaffection of the group of poor voters who have been compelled to vote as well as parties' strategic response to a reform that is likely to directly affect electoral competition between parties. This theoretical account no longer assumes that power shifts from the political right to the left, but rather expects to see only minor changes to each parties' electoral fortunes and an overall balance of power between the main parties that is largely unchanged by the reform. Possibly small increases for both left- and right-wing parties are assumed to be associated with small decreases in seat and vote shares for independent candidates and smaller parties.

These theoretical expectations are tested empirically in a reanalysis of a crucial case that is often seen as the most compelling empirical evidence for the assumed positive relationship between voter turnout and left-wing parties' vote and seat shares: the adoption of compulsory voting laws at different points in time across Australian states in the first half of the 20th century (Fowler 2013). Yet, the Australian case represents a major puzzle: If compulsory voting strongly shifted the balance of political power in favor of the Australian Labor Party (ALP), why did the major non-Labor parties consistently support this electoral reform? Ignorance or misperceptions about the political consequences of the reform might explain initial support

among non-Labor parties in the 1910s. However, they can hardly account for continued support among these parties for a reform that threatened to seriously undermine their chances of winning elections twenty or thirty years later, that is, at a time by which they would have had ample evidence of this policy's effects from other Australian states.

The present quasi-experimental study builds on a similar study by Fowler (2013) and improves on it in three ways: First, the analysis uses more fine-grained, district-level electoral data. This allows to test the extent to which compulsory voting affected parties' strategic campaign behavior, and in particular the non-Labor parties' decision to run their own candidates in previously unchallenged Labor districts after the adoption of compulsory voting. While Fowler (2013)'s analysis is based on state-level aggregate data that assumes that compulsory voting did not affect candidate entry in electoral districts, the more fine-grained district-level data in this study allows for a direct empirical test of this assumption. Second, this study adds to Fowler (2013)'s cross-state analysis a within-state difference-in-differences design. It leverages variation in the treatment intensity, that is, in the extent to which compulsory voting boosted turnout across electoral districts *within* each state, to estimate the effect of this policy on a party's vote share. The advantage of this within-state analysis is that all voters are exposed to the same election campaign, parties and policy platforms and cast their vote on the same day in a given state election. Thus, such a within-state design provides a context where the parallel trends assumption that is required for this kind of analysis is highly plausible. Third, the present study extends the original study which was exclusively focused on the state level to federal (or Commonwealth) elections and tests the effect of compulsory voting in Commonwealth House of Representatives elections in Australia.

An initial replication exercise with the district-level data that are used for analysis in this study shows that they produce – once they are aggregated to the state level – the exact same results as Fowler (2013). Yet, the complete set of analyses in this paper gives a much more nuanced view on the political consequences of increased voter turnout as a result of compulsory voting. Two key findings emanate from this study: First, results both from the state-level and federal-level analyses are more mixed for Australia than previous research suggested. There is

empirical support for a class voting view that suggests that compulsory voting helps left-wing parties and hurts those on the right. However, a more thorough look at within-state and within-Commonwealth variation reveals that there is more variation, with some states aligning more closely to a class voting view and others with a minimal effects view. As a graphical analysis of within-state variation shows, additional factors – such as parties’ strategic decisions to compete in a district and geographic size of a district – seem to matter too and decisively shape the relationship between turnout and left-wing party support. Second, parties respond strategically to compulsory voting by adjusting their campaign strategies, such as the number of candidates they run in previously unchallenged seats that are held by the opposing party. Accounting for parties’ strategic behavior, the positive relationship between voter turnout and a left-wing party’s vote share becomes weaker. For example, Fowler (2013)’s finding that compulsory voting increased the ALP’s vote share by more than 9 percentage points in Australia is likely to be a very optimistic estimate, with the true effect likely to be a couple of percentage points smaller.

This study makes two main contributions. First, it directly speaks to the debate in political behavior about the political consequences of higher voter turnout. Based on a sound causal identification strategy and new, more fine-grained district level data, this study advances this debate by providing a thorough empirical analysis of a crucial case for this literature. Doing so, the study raises questions about the empirical evidence that is used to support the dominant class voting approach and presents an alternative approach that focuses more explicitly on the psychological differences between poor voters who already used to vote before the adoption of compulsory voting and those who did not. Second, it speaks to the literatures on the effectiveness of electoral reforms and on economic inequality. In particular, it highlights the limitations of institutional fixes such as compulsory voting laws in helping solve the problem of increasing economic inequality across advanced industrial democracies.

3.2 Compulsory Voting, Politically Disengaged Citizens, and Partisan Support

Ever since Lijphart (1997) advocated compulsory voting as the most effective means to address “Democracy’s unresolved dilemma” of unequal political participation, several studies have analyzed the consequences of compulsory voting on political participation and election outcomes. Yet, while a large literature has shown that compulsory voting substantially increases voter turnout (Hirczy 1994; Birch 2008), the findings about the political consequences of this electoral reform are less clear.

In this context, I present a theory of the minimal effects of compulsory voting on politics and contrast this approach with the dominant view in this literature: the class voting approach, which is also called the “bias correction argument” (Bechtel, Hangartner and Schmid 2016). Both approaches start from the assumption that an exogenous shock² to voter turnout such as compulsory voting targets citizens who are different from those who already vote: in advanced industrialized countries they are generally socioeconomically worse off (Brady, Verba and Schlozman 1995; Kasara and Suryanarayan 2015), less educated, and less interested in politics. By exogenously increasing the cost of voting (usually by fining those who do not cast a ballot), this group of citizens who would otherwise have abstained from voting is compelled to participate in elections. Based on the dominant *class voting approach*, these previous non-voters’ lower socioeconomic status makes them more likely to favor redistributive policies and thus, to have policy preferences that align with the policy positions of left-wing parties. As a consequence, increasing the share of poor voters among the electorate as a result of compulsory voting will mainly help left-wing (or Labor) parties and boost their overall electoral support at the expense of right-wing parties or candidates (Bechtel, Hangartner and Schmid 2016; Fowler 2013; Lijphart 1997; Meltzer and Richard 1981).

²While the decision to adopt compulsory voting is likely to not be exogenous, the shock to voter turnout that it produces can be considered exogenous to the extent that the policy was not primarily adopted to improve a party’s electoral fortunes at the cost of other political parties, but rather for other, more practical (e.g. efficiency, legitimacy of the election outcome) reasons.

The empirical evidence for the class voting view is mixed, with some researchers being able to find evidence that higher voter turnout benefits left-wing parties (Herron 1998; Mackerras and McAllister 1999) and others not (Highton and Wolfinger 2001; Rubenson et al. 2007). Several recent studies have attempted to advance this debate by using natural experiments in order to more convincingly isolate the causal effect in question. For example, Bechtel, Hangartner and Schmid (2016) leverage cross-cantonal variation in compulsory voting laws in Switzerland to estimate the effect of increasing voter turnout on referendum outcomes, finding that in cantons with compulsory voting support for left-wing policies increased substantially. In a study most similar to my own analysis, Fowler (2013) uses variation in the timing of the adoption of compulsory voting across Australian states to gauge the effect of an exogenous shock to voter turnout on the Australian Labor Party's vote and seat shares. Overall, that study finds a strong effect, with the Labor Party's vote and seat shares increasing by between 7-10 percentage points after voting was made mandatory.³ Thus, these quasi-experimental studies seem to have settled the debate in favor of the class voting approach. However, other studies with similarly convincing quasi-experimental research designs fail to find evidence for a relationship between voter turnout and left-wing party support in other countries (Ferwerda 2014; Miller and Dassonneville 2016; van der Eijk and van Egmond 2007). One interpretation of these conflicting findings that is consistent with the class voting approach is that increasing turnout through compulsory voting is more likely to translate into a significant boost of the left-wing vote share if the institutional context is conducive to it, but not in others. For example, Ferwerda (2014) suggests that finding a class-based effect of compulsory voting is rather unlikely in electoral contexts that use proportional instead of majoritarian electoral formulas. Given the generally larger number of effective parties and an overall higher rate of turnout under proportional representation, it is less likely to see under such a system the combination of a stark change in voter turnout with a single left-wing party that benefits from this change, which are both required for detecting any meaningful effect (Ferwerda 2014, 249). Instead of focus-

³These two studies leverage *within country* variation in the timing of the adoption of compulsory voting across geographic subunits. Other quasi-experimental studies have tried to estimate the political consequences of compulsory voting through a cross-country comparison (e.g. Carey and Horiuchi 2017).

ing on contextual factors that might moderate class voting, this paper suggests an alternative interpretation to the class voting view that questions the plausibility of the assumed positive relationship between voter turnout and left-wing partisan support.

This study proposes a second, alternative perspective on the relationship between turnout and left-wing partisan support, which I call the *minimal effects* view. It assumes a fundamental difference between traditional voters who are poor, but who would have voted even in the absence of compulsory voting and those poor voters who only vote because they are compelled to do so. People in this latter group are expected to be generally more politically disaffected, less interested in and less informed about politics (Selb and Lachat 2009; Singh 2019b). Just like other poor voters, they might have policy preferences for economic redistribution.⁴ Yet, due to their limited political knowledge, they are less likely to know which party is most likely to represent their policy preferences and thus, also less likely to have clearly formed partisan preferences and partisan attachments than socioeconomically similar citizens who used to vote in past elections (Selb and Lachat 2009).⁵

What does this lower level of political sophistication mean for their party choice on Election Day? These new voters are likely to resort to strategies that are known to be used by voters in order to compensate for a lack of knowledge. One possible strategy is to avoid taking a political decision by simply casting a blank vote. This strategy seems especially appealing to those voters who do not know what they want, are unaware of the candidates' policy positions and

⁴Recent research in Economics has started to question Meltzer and Richard (1981)'s assumption that poor people generally share preferences for economic redistribution. Alesina and Ferrara (2005) and Alesina, Stantcheva and Teso (2017) empirically show that a person's desire for redistribution is highly context dependent, with individuals believing in intergenerational mobility holding weaker preferences for redistribution. In addition, Fisman, Kuziemko and Vanutelli (2017) find that redistributive preferences among socioeconomically worse off citizens are usually weaker than often assumed, with many of them preferring a more targeted taxation of the rich than Meltzer and Richard (1981)'s theory would suggest. Finally, Kuziemko et al. (2015) show that even if poor people hold preferences for redistribution, their possible lack of trust into the government might make them not very supportive of redistributive policies. Thus, this literature provides an alternative account of why compelling the poor to vote might not increase support for left-wing parties that is consistent with the minimal effects view that is discussed in this paper.

⁵The situation would be different if voters could directly vote on policies themselves, as is the case in referendums (Bechtel, Hangartner and Schmid 2016). However, most democracies are representative democracies where citizens choose parties or politicians who represent their interests in the political arena.

do only care about the election outcome to a limited extent. Previous research that has found an increase in invalid votes after the introduction of compulsory voting provides evidence that at least some voters seem to resort to this strategy (Mackerras and McAllister 1999; Singh 2019b). Another strategy for voters is to rely on the pieces of information that are most easily available to them when making their vote choice. Voters who, for example, lack details about the policy positions of the candidates in their electoral districts are likely to lean towards candidates that have been more visible to them and for which they can assume that they have more political experience than alternative candidates who are completely unknown to them (Luskin 1990; Zaller 1992). In most cases, this will be a candidate from one of the major parties because she is more likely to be seen as the default candidate than an independent candidate or a candidate from a minor party where it will often be less clear to an uninformed voter what policies that candidate stands for. Name recognition may reinforce this tendency. Candidates from larger parties tend to have been around for longer, as is the case for the party's name. Thus, for low information citizens, the default vote choice is likely to be for such a candidate or party. In the absence of strong partisan preferences for either a left-wing or right-wing candidate (cf. Selb and Lachat 2009), uninformed citizens who are compelled to vote are expected to support main parties' candidates over independent candidates or candidates from minor parties. Unless poor voters are embedded in the Labor movement by, for example, being members of a trade union, this lack of political knowledge and interest could make it difficult for them to clearly align their material interests with the party system. Presented with right-wing candidates that are vying for the support of poorer middle class and rural voters, it might be hard for many of them to clearly identify the party that they think is mostly likely to improve their own economic situation in the medium term. Faced with the task of choosing a candidate from the list of candidates on election day, these voters' choice is likely to resemble a coin flip between the major party alternatives, mostly driven by idiosyncratic factors. As a result, compulsory voting is not expected to produce a general shift in the relative power (i.e. vote shares) between left-wing and right-wing parties, but rather to modestly increase support for both parties or have no effect at all. At the same time, there could be a small decrease in support for independent

candidates or candidates from minor parties.

In addition, parties tend to behave strategically in order to maximize their electoral returns (Downs 1957). As a result, they might react to the introduction of compulsory voting by adjusting their behavior accordingly. For example, if we assume that in a two-party context parties compete for the median voter (Downs 1957), right-wing parties may respond to a leftward shift among the median voter as a result of compulsory voting (cf. Meltzer and Richard 1981) by shifting their own policy position leftward too. However, if they do, their behavior would minimize any negative effect of compulsory voting on their vote shares. Beyond adjusting their policy position, right-wing and left-wing parties might also react to the introduction of compulsory voting by changing their campaign strategies. Campaigning is costly and a party's resources are limited. As a result, parties might strategically choose not to waste money on running candidates in districts where they do not stand a serious chance to win and rather focus their attention and resources on more competitive district. Considerations such as these are usually made responsible for the comparatively high number of uncontested seats in Australian elections in the first half of the 20th century (Sharman 2003). However, a party's calculus of whether to run their own candidate in a district is likely to be directly affected by the adoption of compulsory voting. After compulsory voting, every voting-eligible citizens has to vote. Even in districts where, for example, right-wing parties do not stand a good chance of winning and where they previously did not run a candidate, their supporters are now compelled to poll. So all else equal, the chances of a right-wing candidate to win this district increase, as does the likelihood of the right-wing party to enter into the electoral race in this district. This in turn would reduce the positive effect that compulsory voting, and higher voter turnout, are expected to have on a left-wing party's vote share, thus potentially contributing to a minimal effect of this reform on the overall partisan balance of power.

To test this minimal effects view, the empirical section reanalyzes a crucial case that is often seen as the most compelling evidence for the class voting argument: the gradual adoption of compulsory voting at the state and federal levels in Australia in the first half of the 20th century. The present study improves on Fowler (2013)'s analysis by leveraging more fine-

grained district electoral data for the analysis. The key advantage of these data is that they allow to more thoroughly test the previous findings that found a positive relationship between compulsory voting and left-wing partisan support in Australia. First, these data also include information about unchallenged districts in these elections and thus, allow for a direct empirical test of how the introduction of compulsory voting affected the main parties' decision to put up their own candidates in less competitive districts in state elections. Second, these data are the basis for additional, arguably more internally valid analyses. These analyses do no longer rely on cross-state variation, but instead leverage variation in changes in turnout across electoral districts within a single state to estimate the effect of compulsory voting on a party's vote share.

3.3 Compulsory Voting in Australia

In the late 19th and early 20th century, Australia was a pioneer as far as electoral rules are concerned and it was among the first countries to adopt universal suffrage, the Australian - or secret - ballot, instant run-off voting (also known as alternative vote) and compulsory voter registration and voting. Compulsory voting was introduced at different points in time across Australia. Queensland was the first of the six Australian states to use it for its 1915 state assembly election. The federal government passed legislation in 1924 to make voting in the federal House of Representatives elections mandatory across Australia. After that, all five remaining states followed suit and adopted compulsory voting in 1926 (Victoria), 1928 (New South Wales and Tasmania), 1936 (Western Australia) and 1941 (South Australia; see [Table 3.1](#)).

To what extent does compulsory voting in the different Australian states fulfill the requirement that its adoption was as-if randomly assigned? This requirement would be violated if the adoption of this policy was endogenous to strategic considerations by the political parties that pushed for its adoption. For example, it is possible that the Australian Labor Party supported the adoption of compulsory voting because it anticipated an electoral advantage from such a

Table 3.1: The Adoption of Compulsory Voting in State and Commonwealth Elections Across Australia

State	First Election under CV	Parties Supporting CV
Queensland	1915	Liberal (non-Labor) Party
Commonwealth	1925	Both Labor and non-Labor Parties
Victoria	1927	Both Labor and non-Labor Parties
New South Wales	1930	Both Labor and non-Labor Parties
Tasmania	1931	Both Labor and non-Labor Parties
Western Australia	1939	Both Labor and non-Labor Parties
South Australia	1944	Both Labor and non-Labor Parties

Note: With the exception of South Australia which had a Labor government at the time of the adoption of compulsory voting, all other states and the Commonwealth made voting mandatory while non-Labor governments were in power. Except for Queensland, compulsory voting laws received unanimous support from all MPs in the respective state and Commonwealth legislature.

reform.

The literature cites several reasons for the adoption of compulsory voting in Australia. While partisan motivations for the adoption of compulsory voting have mattered to some extent, they usually played a secondary role and were largely limited to early considerations of this policy in the 1910s (Brett 2019). For example, the incumbent Liberal (i.e. non-Labor) government under Premier Digby Denham in Queensland adopted compulsory voting in 1914 largely because it hoped that it would hurt the Australian Labor Party and thus, help the Liberals avoid or at least reduce the size of the anticipated defeat in the 1915 state election (Brett 2019; Evans 2006). This is in line with Helmke and Meguid (2010)'s claim that compulsory voting was sometimes supported by right-wing parties as a means to counter the electoral threat from the left. However, as it turned out, mandatory voting did not disadvantage the Labor Party and they clearly won the 1915 election in Queensland. Possibly as a result of these early experiences with compulsory voting, partisan considerations played less of a role in the following debates that led to the adoption of compulsory voting after 1924 in the Australian states and in Commonwealth elections. Instead, rather technical concerns are frequently mentioned as the decisive factors for the broad cross-partisan support for this electoral reform (Brett 2019; Evans 2006; Mackerras and McAllister 1999). First, this reform was aimed at reducing the

costs of campaigning and mobilizing voters for candidates in a context where an increasing number of candidates complained about the burden of having to drive voters in cars to the polling places on election day (Brett 2019, 132). Second, an immediate impetus for the adoption of compulsory voting was an election with a particularly low voter turnout and the concern among politicians that this might undermine their political legitimacy (Brett 2019, 134, 137). Thus, in contrast to other electoral reforms such as the adoption of the alternative vote or the redistricting of electoral boundaries, compulsory voting was a relatively uncontentious electoral reform after the mid-1920s in Australia and rather seen as a technical solution to very tangible problems that candidates faced in their districts. This rather technical perception of the reform is supported by the fact that, with the exception of Queensland in 1914, it enjoyed broad cross-partisan consensus in state assemblies and in the House of Representatives. The adoption of compulsory voting can therefore largely be considered exogenous to the Labor or the main non-Labor parties' strategic electoral considerations.

Once compulsory voting was implemented, every voter was supposed to go to the polling place and cast a ballot on election day. Eligible citizens who did not vote and could not provide a valid excuse for not voting had to pay a fine of up to two pounds, a substantial amount of money that corresponds to A\$160 or US\$120 in today's money and that is likely to have provided a strong financial incentive – especially for poorer people – to participate in elections.⁶

Most crucially for the test of the minimal effects account, previous research has shown that it is especially poor voters who were less likely to vote in Australia in the early 20th century under a system where voting was not compulsory (Fowler 2013). As a consequence, making voting mandatory for these people increases the share of poor voters among the electorate, which is a key prerequisite for the test of the proposed theory.

⁶The value of two pounds in 1925 is converted into 2018 dollars based on the Reserve Bank of Australia's pre-decimal inflation calculator, <https://www.rba.gov.au/calculator/annualPreDecimal.html> (last accessed on May 11, 2019).

3.4 Data and Methods

3.4.1 Data and Dependent Variables

The empirical analysis in this study is based on a dataset that includes electoral district data for state and federal (Commonwealth) Lower House elections in Australia for the period 1910-1950. To create this dataset, I digitized historical voting records from state assembly elections in all six states and for the Commonwealth House of Representatives (see Carr 2019; Hughes and Graham 1974*a,b*, 1976). These district-level data have two key advantages over data that is aggregated at the state level, which has been used in previous studies (e.g. Fowler 2013). First, they allow for a direct empirical test of the extent to which previous estimates of the effect of compulsory voting on the Australian Labor Party (ALP)'s vote share have been confounded by that party's changing share of unchallenged seats. The state-level aggregate vote share for the ALP in a given state election is commonly calculated by adding all votes that were cast for this party in this particular election over all voting-eligible citizens in this state who reside in electoral districts that were challenged.⁷ In other words, unchallenged districts with only one candidate who is not opposed by any other candidate within her district are commonly treated as missing data and simply excluded from this calculation. This procedure is a reasonable approach to estimate a valid vote share measure for a given state for a particular election. However, it may undermine the validity of analyses that rely on a before-after comparison of a party's vote share over time (for example, in a difference-in-differences design) if not only the vote share, but also the number or the kind of districts which are unchallenged are affected by over-time variation. Unless the exact same districts are unchallenged in all elections for a given state and thus excluded from analysis, this is a potential problem. Having the more fine-grained district-level vote shares for each political party allows me to directly test to which extent adjusting for these unchallenged seats for a given party affects that party's electoral fortunes.

⁷See, for example, the state-level aggregate election data from the Australian Politics and Elections Database, which is available at: <http://elections.uwa.edu.au/>.

The second advantage is that more fine-grained district-level data provide additional opportunities for analysis which may give new insights into how compulsory voting affects a left-wing party's electoral support. In this specific study, they allow to add to the cross-state comparison of the effect of compulsory voting an analysis that leverages within-state variation across electoral districts. This within-state analysis can use variation in turnout changes across electoral districts and link them, for example, to changes in left-wing party support to provide a more direct test of the claim that increases in voter turnout help left-wing parties. The modelling choices that are associated with this kind of within-state analysis and the advantages of this approach are discussed in more detail below.

The key dependent variable is a party's vote share over all valid votes that were cast in a given election. Further dependent variables are the overall voter turnout and a party's share of uncontested seats among all the uncontested seats in a given election. The coding of the party variables that is required to determine a party's vote and seat shares needs some explanation due to Australia's relatively fluid party labels before World War II. The Australian Labor Party (ALP) as well as the Lang Labor Party both targeted working-class voters and are coded as one single Labor party. The latter temporarily split from the Australian Labor Party after the 1929 economic crisis and competed elections in the early 1930s as a separate party, but both parties merged back together into a single party in the mid-1930s. This Labor party was usually opposed by a coalition of two non-Labor parties: the main non-Labor party, which, depending on the election and the state, is either called the Liberal, National or United Australia Party, and a smaller Country Party. Given that both parties usually formed electoral coalitions in which they supported each other's candidates against Labor, they are coded as a single party which I call "non-Labor coalition". Candidates from smaller parties (including the Social Credit Party and the Communist Party) and all independent candidates did usually not officially cooperate with either of the two main parties and are all grouped into a third category for which the terms "other parties" or "independent candidates" are used interchangeably in this paper.

3.4.2 Research Design

The challenge to empirically estimate the political consequences of increased voter turnout is that changes in voter turnout are usually not exogenous, but directly influenced by the specific electoral context of any given election such as, for example, the competitiveness of the election or the attractiveness of the candidates and their programs. This makes it difficult to assess how higher voter turnout would impact a given party's electoral success. To limit the risk of confounding, this study leverages a natural experiment: the gradual adoption of compulsory voting at different points in time across Australian states as well as within-state variation in treatment intensity across electoral districts. A possible concern with this research design might be that the timing – which state switched when – could have been shaped by strategic considerations. However, a look at the relevant literature suggests that this concern is likely to be unfounded in the Australian context. The timing – except for the early adopter Queensland in 1914 – was generally less influenced by strategic considerations and rather the result of politicians trying to reduce voter mobilization costs and potential threats to the legitimacy of election outcomes (Brett 2019; Evans 2006; Mackerras and McAllister 1999). More specifically, in some cases (e.g. for the federal level and in South Australia) a strong motivation for reform was provided by an immediately preceding election with a particularly low level of voter turnout (Brett 2019, 134, 137). In other cases, the adoption of compulsory voting seems to have followed a pattern of policy diffusion, such as when Victoria, New South Wales and Tasmania – and with a delay of a few years, possibly Western Australia – all made voting mandatory for their lower-house elections shortly after it was introduced at the federal level in 1924 (Brett 2019, 137). For this reason as well as those that were discussed in detail above, the adoption of compulsory voting produced a shock to voter turnout that can arguably be considered as exogenous. In particular, compulsory voting received cross-partisan support in parliament and seemed to have been primarily adopted for practical, as opposed to strategic, considerations in order to make the administration of elections less costly and more efficient.

For the empirical analyses in this study, two different kinds of difference-in-differences

designs are used. One design leverages cross-state variation in the timing of when a particular state adopted compulsory voting to estimate the effect of this electoral reform, and the boost to turnout that followed, on each parties' vote share. A second design uses within-state variation in treatment intensity, that is, changes to voter turnout as a result of compulsory voting. This analysis compares changes in voter turnout across electoral districts from before and after the introduction of this reform to changes in partisan support in these districts within a single state. Both analyses use the same dataset of district electoral data that was described above. For the cross-state analysis, these data are aggregated into state-level measures of voter turnout and vote shares for each party. This design follows the modelling choices in Fowler (2013) in order to allow for a direct replication of the findings from that study. The within-state analysis requires the use of district-level electoral data. It follows similarly designed studies on the women's suffrage (Morgan-Collins and Teele 2016) and only uses data from two elections in each state, that is, from the election that directly preceded and the one that followed the introduction of compulsory voting in a state.

For the cross-state analysis, the following model is used for state elections between 1910 and 1950:

$$Y_{st} = \alpha_s + \gamma_t + \delta D_{st} + \epsilon_{st} \quad (3.1)$$

where Y_{st} is a party's vote share in state s in year t , α_s are state fixed effects, γ_t year dummies, D_{st} an indicator showing whether state s had compulsory voting in year t and ϵ the error term. To adjust for dependence across elections from the same state, standard errors are clustered at the state level (Angrist and Pischke 2009). The dataset includes data for state assembly elections in all six Australian states and states had on average 14 elections in this time period.

In this cross-state difference-in-differences design, the estimation of the effect of compulsory voting crucially hinges on the plausibility of the parallel trends assumption. Variation in the profiles and policy platforms of the state Labor and non-Labor parties across states, state-

specific election campaigns and the fact that dates for state elections usually lie months apart from each other are all reasons to question the validity of this assumption. In addition, the assumption of parallel trends across states for the 1910-1950 period might be further challenged by these states' electoral reforms such as the changes in electoral formulas (for an overview, see Farrell and McAllister 2006, 50-51) or the redrawing of electoral district boundaries.⁸

Therefore, this study complements the cross-state analysis with an arguably more sound and internally valid within-state difference-in-differences design that leverages variation in treatment intensity across electoral districts within a single state (cf. Morgan-Collins and Teele 2016). In particular, this approach uses the change in voter turnout in a given district between the pre- and the post-reform elections as a measure of that district's treatment intensity. Districts with a larger share of poor citizens are, all else equal, expected to have a lower voter turnout pre-reform and thus to see a stronger increase in voter turnout from the pre- to the post-reform election than districts where the poor represent a smaller share of the electorate. If the class voting view is correct, we should therefore see the Labor vote share to increase more in those districts where the increase in voter turnout was larger. The fact that all electoral districts within a state were exposed to the exact same state Labor and non-Labor parties, experienced the same election campaign and had the election on the same day provides strong support to the plausibility of the parallel trends assumption in this context. The following model is used for the empirical analysis of the within-state variation:

$$\Delta Y_d = \alpha + \beta \Delta \text{Turnout}_d + \Delta \epsilon_d \quad (3.2)$$

where ΔY_d is the change in a party's vote share in each electoral district from the election preceding to the election following the adoption of compulsory voting in a given state,

⁸For example, New South Wales switched from a majority-runoff system with single-member districts to the single transferable vote in multi-member districts in 1918, and then to alternative vote in single-member districts in 1926 (Farrell and McAllister 2006, 50). The only two states that did not change their electoral formula between 1910 and 1950 are Tasmania, which used the single-transferable vote in multi-member districts, and Western Australia with the alternative vote (and compulsory preferences).

$\Delta Turnout_d$ the change in turnout between the two elections and ϵ_d the error term.⁹ $\Delta Turnout_d$ is a continuous variable that measures the intensity with which each electoral district was exposed to the compulsory voting treatment. Electoral districts with a larger number of poor voters who did not vote previously are expected to see a stronger increase in turnout as a result of the introduction of compulsory voting than districts with fewer such citizens.

The within-state analysis only includes electoral districts in the analysis where more than a single candidate competed in both elections. This means that districts that are unchallenged either in the election that precedes the adoption of compulsory voting or in the election that immediately follows this reform are completely dropped from the analysis. This approach makes sure that it is possible to calculate a meaning change in turnout and a party's vote share between the pre- and the post-reform elections. Despite several changes to their electoral formulas in the 1910-1950, no state made changes to their electoral formula around the time that they adopted compulsory voting.¹⁰

Furthermore, the redistricting (or redistribution) of electoral boundaries in between the two elections is a potential threat to the validity of this within-state design. For example, if right-wing politicians strategically redraw the electoral boundaries in order to shield themselves from the negative electoral effects of compulsory voting, the above within-state model specification would produce downwardly biased estimates for the relationship between voter turnout and a left-wing party's vote share. For most of the states in the analysis – Queensland, South Australia, Western Australia, Tasmania as well as for the federal level (see further below) – electoral boundaries remained unchanged in between the elections in question (Bennett and Bennett 1986; Hughes and Graham 1968). Furthermore, the major redistricting in New

⁹With observations from only two time points, this is equivalent to an equation with district and year fixed effects (see Angrist and Pischke 2009).

$$Y_{dt} = \beta_d + \gamma_t + \delta(Turnout_d * d_t) + \epsilon_{dt} \quad (3.3)$$

In this equation β_d are district fixed effects, γ_t a dummy for the election year with compulsory voting, the variable $Turnout_d$ the share of people who voted in each district and d_t a time dummy for observations after the introduction of compulsory voting.

¹⁰New South Wales modified its alternative vote in single-member districts in 1928 by replacing the contingent vote with compulsory preferences (Farrell and McAllister 2006, 50).

South Wales that turned its previously multi-member districts into single-member districts and that accompanied this state's transition from single transferable vote to the alternative vote happened before 1927. Thus these changes did not affect the 1927 and 1930 state elections. However, in New South Wales electoral boundaries were changed again by the non-Labor government in 1929 (Clifford, Clune and Green 2006) as were electoral boundaries in Victoria in 1926 (Hughes and Graham 1968, 464), which each redistricting falling in between the 1927 and 1930 elections in New South Wales and those in 1924 and 1927 in Victoria respectively. While it is not clear to what extent incumbent right-wing governments in these two states were able to use redistricting to shield their party from the effects of compulsory voting, this redistricting might undermine the validity of the effect estimates from these two states. As I have not been able to locate material yet that would allow me to identify the districts which were affected by this redistricting, empirical results from these two states have to be interpreted with caution.¹¹

For the estimation of the effect of compulsory voting on parties' vote share at the federal level, two empirical strategies are chosen that to some extent mirror the analyses at the state level that were explained above. One is a direct application of the within-state difference-in-differences design to the federal level. The model that is used for this analysis is the same as in equation 3.1 above, with the only difference that state districts are replaced by federal districts and state lower-house elections by the 1922 and 1925 elections to the Commonwealth House of Representatives. Importantly for the validity of this approach, there were no changes to the electoral formula or to the electoral district boundaries between 1922 and 1925.¹²

Analogous to the cross-state analysis, the second approach uses variation in the timing of when the reform was adopted across different levels of government. However, in contrast to the state-level analysis, the estimation of the effect of compulsory voting for Commonwealth

¹¹A name change for an electoral district is frequently indicative of a major redrawing of its electoral boundaries. Thus, districts which changed their name as a result of the redistricting were dropped from the analysis for these two states.

¹²The transition from single-member plurality to alternative vote for House of Representatives elections had already happened several years prior, in 1918.

(federal) elections requires cannot simply adopt the cross-state difference-in-differences design that was explained above. Due to the simultaneous introduction of mandatory voting across all six Australian states for the 1925 federal election, there is no geographic variation in the timing of when a given state adopted compulsory voting in federal elections. Thus, a standard difference-in-differences design is not applicable in this setting. Yet, it is possible to compare elections within each state by leveraging information from elections at *different levels* of government. More specifically, we can compare voting behavior right before and after the adoption of compulsory voting at the federal level with voting behavior at the state level that was not directly affected by this electoral reform.¹³ Such an analysis would zoom in a specific period in Australian politics: the adoption of compulsory voting at the federal level between the 1922 and 1925 Commonwealth elections and state elections in all six states that happened around the same time, that is, in the mid-1920s right before and after the adoption of the reform in 1924. Luckily for our purpose, both in the states and at the federal level a party system had emerged at the beginning of the 19th that, despite some regional differences,¹⁴ pitted everywhere the Labor Party against non-Labor coalition parties (Jaensch 1994, 20). Importantly, while issues such as conscription in World War I or policy responses to the 1929-1930 economic crisis led to divisions and splits within the Labor Party (e.g. Jaensch 1994; Weller and Fleming 2003, 21, 25), the early to mid-1920s were a time of little frictions between the state branches themselves or the state branches and the federal branch of the Labor Party. During this time, a primary focus of the Australian Labor Party is on promoting social welfare for the poor and improving the working conditions of those in the labor force (McKinlay 1981, 55-57). Therefore, there is no obvious reason to assume that voting patterns are any less likely to move in tandem across the federal and the state level within each state in this specific period of time (i.e. between 1922 and 1926) than they do across states (see Jaensch 1994; Moon and Sharman 2003; Sharman

¹³Such an empirical strategy is possible because the adoption of compulsory voting at the state level does not temporally coincide with the introduction of this policy at the federal level, with most states making voting compulsory a few years after the federal parliament did so.

¹⁴For example, it is regularly emphasized that the New South Wales state branch of the Australian Labor Party was more conservative than other state branches of this party (e.g. Smith 2003, 60).

and Sayers 1998). Yet, the results section below provides an empirical test of this assumption to directly address concerns in this respect.

The model that is used for this specific analysis has similarities with the cross-state analysis that is discussed above. This model is run on data from both the state and the federal level, but only for elections directly preceding and following the adoption of compulsory voting at the federal level in 1924:

$$Y_{dse} = \alpha_s + \theta_e + \delta D_{se} + \epsilon_{dse} \quad (3.4)$$

where Y_{dse} is a party's vote share in district d within state s in election e , α_s are fixed effects for each Australian state, θ_e a dummy indicating whether the election occurred right after or in the same year as the adoption of compulsory voting as opposed to occurring right before 1924, D_{se} the treatment of compulsory voting and ϵ the error term. As with equation 3.1, standard errors are clustered at the state level.

3.5 Results

The results section starts with difference-in-differences analyses of the adoption of compulsory voting in Australian states in the early 20th century. Then, the section proceeds to extend the view by looking at the federal level, that is, compulsory voting in the elections for the Australian House of Representatives.

3.5.1 State-Level Results

Table 3.2 presents the results from a difference-in-differences design that leverages the gradual adoption of compulsory voting in Australian states between 1914 and 1943. Results in columns 1 and 2 are a direct replication of Fowler (2013)'s table 2, columns 1 and 3. The adoption of compulsory voting led to a strong increase in voter turnout in these states by an average 24.7 percentage points (see column 1). In other words, voter turnout that tended to be around 68%

Table 3.2: Effect of Compulsory Voting in State Assembly Elections

	Turnout	Vote Share without Unch. Seats			ALP Unch. Seat Share	Vote Share with Unch. Seats		
		ALP	NLC	Ind		ALP	NLC	Ind
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Compulsory Voting	.247 (.044)	.093 (.028)	-.019 (.027)	-.080 (.046)	-.209 (.096)	.066 (.018)	-.001 (.027)	-.072 (.040)
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N	86	86	86	86	601	86	86	86
R ²	.930	.814	.832	.765	.220	.779	.762	.760

Note: Table presents difference-in-differences estimates. Unch.: Unchallenged. Turnout data are aggregated to the state level. Standard errors in parentheses are clustered at the state level. ALP: Australian Labor Party; NLC: Non-Labor Coalition; Ind: Independents and minor parties.

went up to around 92% as a result of this institutional electoral reform. Columns 2 to 4 report the effect of this institutional change on a party's vote share. Column 2 shows that the vote share of the Australian Labor Party increased by 9.3 percentage points as a result of compulsory voting. This effect is substantially large and statistically significant at $p < 0.05$. Interestingly, both the effect of compulsory voting on turnout and on the Australian Labor Party's vote share are practically identical to those from Fowler (2013), who reports an increase in turnout of 24.3 and in Labor's vote share of 9.2 percentage points.¹⁵ This shows that using district-level election returns from the dataset that is the basis of this study and aggregating them to the state level gives us the exact same results as those that are reported in Fowler (2013). The results in columns 3 and 4 extend the analysis to non-Labor coalition parties and independent candidates. They suggest that the gain for Labor did not come at the cost of support for the non-Labor coalition parties, whose vote share is largely unaffected by this electoral reform, but instead is primarily the result of weakened support for independent candidates, whose vote share decreased by 8 percentage points ($p = .146$). Overall, these results support a class-based account according to which a strong increase in voter turnout primarily benefits left-wing parties and speak against a minimal effects view.

However, it is possible that political parties respond strategically to the introduction of com-

¹⁵The very small difference of 0.1 percentage points in Labor vote shares between Fowler (2013)'s study and the present analysis could be due to marginally differing coding decisions about which candidates belong to the Australian Labor Party in a given election and which do not.

pulsory voting, with non-Labor coalition parties more likely to run a candidate in an electoral district after voting is mandatory. It is plausible that these new non-Labor candidates receive a lower and Labor candidates in these districts a higher vote share than the respective non-Labor and Labor candidates in other districts with a more established competition between the two parties. To the extent that compulsory voting directly affects the share of unchallenged seats that are held by each party and that results in columns 2-4 simply exclude all unchallenged electoral districts from the analysis, the results in columns 2-4 might overestimate the effect of compulsory voting on the Australian Labor Party. To test this possibility, column 5 reports the effect of compulsory voting on the share of unchallenged seats that are held by the ALP. The coefficient of -0.21 shows that the share of unchallenged Labor seats among all unchallenged seats declined by 21 percentage points after the introduction of compulsory voting. This effect is substantively large and, with a p-value of $p=0.096$, it is statistically significant at $p<0.10$. This suggests that the positive effect of compulsory voting on the Labor vote share in column 2 would probably become smaller if the strategic behavior of political parties and the changing number of unchallenged seats are taken into account.

Columns 6 to 8 present results that illustrate how estimates change once the lower share of unchallenged Labor seats after the introduction of compulsory voting is accounted for. The model specification is identical to the corresponding models in columns 2-4. However, for models in columns 6-8, unchallenged districts without any competition are no longer dropped, but included into the dataset and the missing vote shares for the main parties imputed. A seat is usually unchallenged because one party's candidate holds such a strong electoral advantage in this district that other parties assume that they do not stand a reasonable chance of winning this seat. As a result, these other parties choose not to spend their campaign resources on a race in an electoral district they are unlikely to win and instead invest them into electoral districts where the competition between their own candidate and those of the other parties is more balanced. How do parties know whether they stand a chance to win in a given electoral district? I assume that in most cases a party's best guess is to look back to previous election outcomes in that district. In particular, the outcome in the previous election is likely to serve as an indicator

for a party's chance to win in the next election. The imputation follows this reasoning and therefore replaces all parties' vote shares in an unchallenged district with the vote share that each party obtained in the most recent election in which that district was challenged. This value is likely to be the best approximation of the relative electoral power that each party holds in that district.

Following this adjustment that accounts for the parties' strategic response to the reform, the effect of compulsory voting on a party's vote share is marginally affected and decreases somewhat in size for all parties (see columns 6-8). Most importantly, compulsory voting still leads to an increase in the Labor Party's vote share that is statistically significant at $p < 0.05$, but the effect size shrinks from 9.3 to 6.6 percentage points. Thus, strategic dynamics matter for estimating the effect. While there clearly is a positive relationship between compulsory voting (and the boost in turnout it produces) and the electoral support for the Australian Labor Party, the true size of the shift in Labor's vote share is likely to be somewhat smaller than previously thought, at around 6.6 percentage points.

A possible concern with the results from Table 3.2 is that states might have been affected by factors that undermine the plausibility of the parallel trends assumption for the 1910-1950 period. Several Australian states switched to a different electoral formula or changed the district magnitude during the first half of the 20th century. In addition, parties' responses to challenges such as the two World Wars or the economic crisis of 1929-1930 varied to some extent across states. Table 3.3 therefore reports results from a within-state difference-in-differences design that are less likely to be affected by these threats to internal validity. Columns 1-6 are for each of the six Australian states while column 7 pools the data from the districts in all six states. This within-state analysis only includes districts where seats were challenged both in the election preceding the adoption of compulsory voting and in the election following it. Note also that in two of the six states, New South Wales and Tasmania, voter turnout in the state election directly preceding the adoption of compulsory voting was already relatively high, at 83% and 82% respectively. This may suggest that non-voters in these states are more likely to be 'occasional' or 'accidental' non-voters and therefore we might expect to find less of a class effect of

Table 3.3: Effect of Compulsory Voting Using Within-State Variation in Treatment Intensity

	Δ Vote Share of Australian Labor Party						
	NSW	QLD	SA	TAS	VIC	WA	All States
	1927- 1930	1912- 1915	1941- 1944	1928- 1931	1924- 1927	1936- 1939	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Turnout	.549 (.359)	-.165 (.251)	.018 (.360)	-3.060 (.373)	.269 (.409)	-.758 (.254)	-.057 (.165)
N	77	61	25	5	25	29	222
R^2	.051	.017	.000	.806	.016	.384	.270

Note: Table presents difference-in-differences estimates of treatment (change in turnout as a result of compulsory voting) on change in ALP vote share. The pooled model in column 7 includes state fixed effects. Robust standard errors in parentheses. Commonw.: Commonwealth; Elect.: Elections.

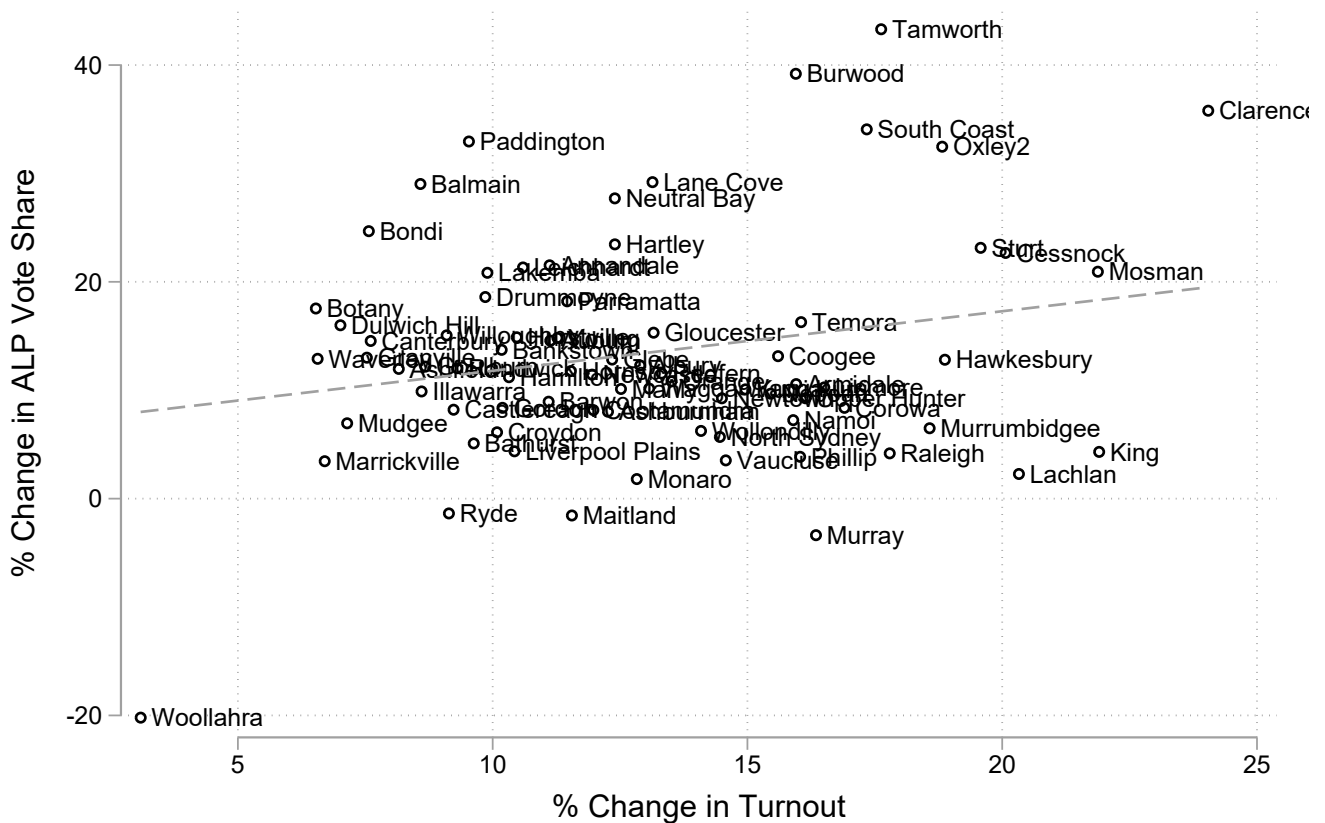
compulsory voting on turnout in these two states.

There is a positive relationship between voter turnout and the vote share for the Labor Party in only two of the six states, New South Wales and Victoria. The coefficient of 0.549 in column 1 suggests that an electoral district in New South Wales with a 10 percentage point increase in voter turnout between the 1927 and the 1930 elections saw on average a 5.5 percentage point increase in the vote share for the Labor candidate. This effect is substantively large, but not statistically significant at the $p < 0.05$ level, something that might partly be attributable to the relatively small sample size of $N=77$. The coefficient for Victoria is even smaller in size and well below the $p=0.05$ threshold of statistical significance. As it turns out, the only two states with positive coefficients, New South Wales and Victoria, are precisely the two states that saw a redistricting in between the election that immediately preceded the introduction of compulsory voting and the election that followed it. We therefore cannot exclude the possibility that the non-Labor government's redistricting might have influenced the results for these two states, possibly giving us too small an estimate of the true effect of the reform on the Labor vote share.

However, more worryingly for the class voting view is that in three of the six states the relationship is a negative one that is substantively large and statistically significant at $p < 0.05$ for two of these states, Tasmania and Western Australia. In a fourth states, South Australia, the

coefficient is substantively close to 0, suggesting that there is no relationship between turnout change and Labor vote share in this state at all. Despite the extremely small sample size of $N=5$ for Tasmania which cautions us not to put too much weight into the statistical finding from this state, the consistency of this pattern across states and the fact that results in none of these four states were possibly confounded by redistricting leave us with a pattern that is hardly reconcilable with the class voting view. This interpretation is corroborated by the result from the analysis that pools the data from all six states. The effect is small and statistically indistinguishable from 0 (see column 7). Thus, the overall pattern across the six states as well as the effect from the pooled analysis of this within-state analysis are generally more in line with a minimal effects view. This interpretation is largely supported by similar analyses for the non-Labor parties and independent candidates in all six states, which are reported in Appendix Tables B.1 and B.2.

Figure 3.1: Change in Turnout and ALP Vote Share in New South Wales, 1927-1930



Finally, Figures 3.1 and 3.2 illustrate the relationship between turnout and Labor vote share from the within-state analysis for two of the states graphically.¹⁶ The goal of the graphical exercise is to point out patterns in the data that may provide the basis for a more in depth analysis of individual electoral districts that could include their socioeconomic composition, geographic location and redistricting history. By looking at the distribution of the districts from New South Wales in Figure 3.1, the state with the potentially strongest positive relationship between turnout and Labor Party support, the following pattern appears: the positive coefficient in Table 3.3 above is mostly driven by a handful of districts in the top right hand corner of the graph that saw a strong increase in both turnout and Labor vote share between the 1927 and the 1930 elections and one district at the bottom left hand corner, Woollahra, where turnout practically stayed the same while Labor support dropped by 20 percentage points. For the rest of the districts, voter turnout and the Labor Party vote share seem mostly unrelated to each other. In other words, some of the districts in New South Wales exhibit a pattern that seems to align well with the class voting view while other districts do not. A closer analysis of the five districts in the top right hand corner of the graph shows that none of them had a Labor candidate compete against the other candidates in that district in 1927, but in all of them a Labor candidate ran three years later in 1930. As for Woollahra, the seemingly strong drop in support for the ALP is the result of redistricting and of the candidate's strategic response to it. In 1929, the district boundaries for both Woollahra and the adjacent district of Paddington were redrawn in such a way that the 1930 district of Woollahra included large parts of the former district of Paddington and the 1930 district of Paddington large parts of the former Woollahra (Clifford, Clune and Green 2006, 61,69). What is more, this 1929 redistricting is likely to have fundamentally changed the electoral composition of each of these two districts, leading both the incumbent Labor candidate from Woollahra and the incumbent Nationalist candidate from Paddington to swap districts and to successfully run for re-election in each other's district in

¹⁶The analysis in the main body of the text focuses on the state with the largest increase and the one with the largest drop in Labor vote share respectively. For graphs of the other Australian states, see Figures B.1, B.2, B.3 and B.4 in the Appendix.

the 1930 election.¹⁷ In other words, instead of being support for the class voting approach, the seemingly positive association between turnout and the ALP vote share in New South Wales is rather evidence for the candidates' strategic response to the introduction of compulsory voting or to the redrawing of electoral district boundaries.

Figure 3.2: Change in Turnout and ALP Vote Share in Tasmania, 1928-1931

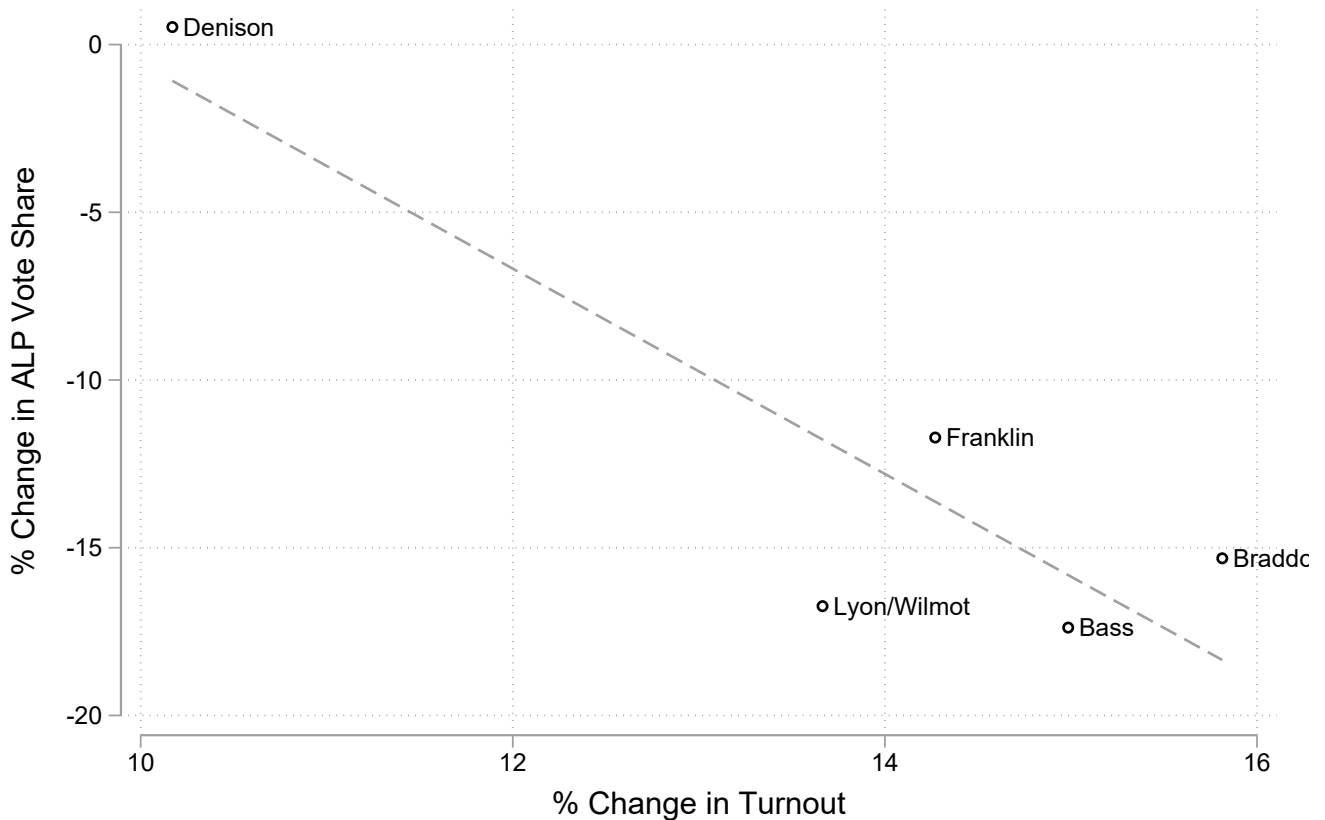


Figure 3.2 plots the data for Tasmania, the state with the strongest negative relationship between voter turnout and Labor Party support. This graph provides evidence that clearly suggests that geography – and especially district size – matters for the turnout-Labor vote share relationship. As the graph shows, the district of Denison saw the smallest increase in turnout between 1928 and 1931, but it was the district where support for the Labor Party stayed roughly the same whereas it declined strongly in all other districts. However, Denison is different from

¹⁷If these five districts in the top left-hand corner and the problematic districts of Woollahra and Paddington are dropped from the graph, the slope of the regression line in Figure 3.1 changes from 0.549 to -0.236.

the other districts in many respects. Most importantly, it is much smaller in size than all other four districts and much more urban, largely built around the Tasmanian capital of Hobart. Given that people in Denison have to travel much shorter distances to vote on election day, voting is likely to be easier and voting costs significantly lower for people who live in this state.¹⁸ As a consequence, making voting compulsory more strongly increases turnout in the larger, more rural districts of Tasmania where turnout tended to be lower before the adoption of the reform. As it turns out, these voters from the more rural areas seem less likely to vote for the Labor Party in Tasmania than people who live in the more urban capital region. However, neither the class voting nor the minimal effects view properly accounts for this finding from Tasmania that geography matters.

3.5.2 Federal-Level Results

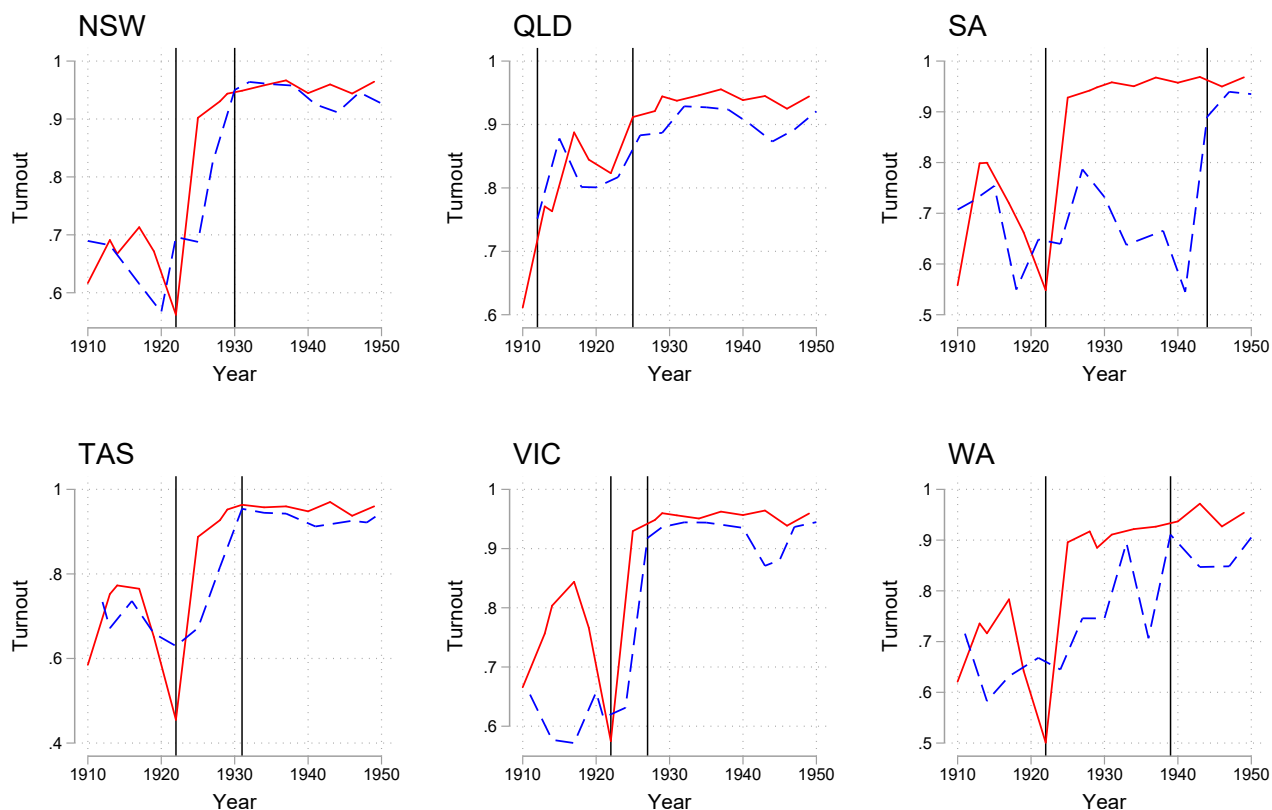
Given these somewhat mixed results from the state-level analysis, this study expands the analysis by looking at the effect of compulsory voting at the federal level. Like before, the analysis at the Commonwealth level proceeds in two steps: First, it uses variation in the timing of when compulsory voting was adopted at the federal (or Commonwealth) and the state level. Second, it leverages variation in the intensity of the compulsory voting treatment across electoral districts *within* federal elections, that is, Commonwealth elections to the House of Representatives.

At the Commonwealth level, voting was made compulsory for all states starting in the 1925 House of Representatives election. Given this simultaneous introduction of compulsory voting for all Australian states in federal elections, there is no geographic variation in the timing of when compulsory voting was adopted across Australia. Thus, a direct application of a traditional difference-in-differences design that leverages cross-state variation is not possible. Yet, the different adoption dates of compulsory voting in state and Commonwealth elections allow us to compare voting patterns in federal elections before and after the adoption of compulsory

¹⁸The next larger district of Braddon is roughly 27 times larger than Denison while the largest district of Lyons/Wilmot is about 127 times larger.

voting in a given state with the voting behavior in the same state in similarly timed elections one level below, that is, in state elections. Like the difference-in-differences design, such an analysis crucially hinges on the validity of the parallel trends assumption. While it is plausible to assume that turnout and voting patterns in federal and state elections move at least as much in tandem as they do across states, Figure 3.3 provides an empirical test of this assumption. Across all six states, Figure 3.3 shows that with a few exceptions such as Victoria between 1910 and 1920, turnout at the state and federal level tend to move in parallel. As we would expect, the only time these trends do clearly not move in tandem is after one level adopted compulsory voting, but the other did not. In these instances, which are demarcated by two vertical lines for each state, turnout in the elections under compulsory voting is significantly higher than in the elections without.

Figure 3.3: Voter Turnout in State and Federal State Assembly Elections, by State



Note: Red line shows voter turnout in federal and blue line in state assembly elections for each state. The two vertical lines indicate the adoption of compulsory voting. With the exception of Queensland where compulsory voting was adopted at the state level in 1914, the first vertical line is for the federal and the second line for the state level. The area in between the two vertical lines indicates the period in which voting was only compulsory at one level, but not the other.

Based on the results in Table 3.4, the adoption of compulsory voting increased voter turnout in Commonwealth elections by 30.4 percentage points (column 1), boosting turnout from around 60% in the 1922 federal election to just above 90% in 1925. Results in columns 2-4 suggest that this strong shift in voter turnout primarily helped the Australian Labor Party, increasing its vote share by 2.8 percentage points ($p < 0.05$), but did not affect the vote share of the non-Labor coalition parties. However, as in columns 2-4 in Table 3.2 above, results in columns 2-4 in Table 3.4 are based on an analysis that set observations for unchallenged elec-

toral districts to missing values, but included these very same districts for an election in which more than one candidate ran in them. As before, it is possible that parties respond strategically to the introduction of compulsory voting. Therefore, these results may be affected by parties' strategic decisions about whether to run their own candidate in a given district and might possibly overestimate the positive electoral effect for the ALP. To remedy this problem, columns 5-7 present results from an analysis that uses the same models as in columns 2-4, but only keeps electoral districts for which we have vote share data for both the pre- and the post-compulsory voting election. In other words, it excludes unchallenged districts not only from the election in which they were unchallenged, but completely drops them from the analysis. This gives us a balanced panel that includes the exact same electoral districts for both the pre- and the post-mandatory voting election. Based on this balanced panel, the substantively small coefficient of 0.007 in column 5 suggests that compulsory voting did not have any effect on the ALP's vote share. As for the non-Labor parties' electoral fortunes, their vote share might have increased a little (2.8 percentage points), but due to a large standard error this effect is not statistically significant at $p < 0.05$. The vote share for independent candidates slightly decreased after voting became compulsory, but the effect is small and statistically not significant (see columns 4 and 7). Thus, the analysis of the introduction of compulsory voting at the federal level that leverages variation in the timing of the reform between the federal and the state levels does not support the class-based view and is more consistent with a minimal effects perspective.

Table 3.4: Effect of Compulsory Voting in Commonwealth House of Representatives Elections

	Comparison Across Levels: Federal – State							Comparison Within Federal Level		
	Turnout	Vote Share			Vote Share			Δ Vote Share		
		Without Missing Data			With Balanced Panel					
		ALP	NLC	Ind	ALP	NLC	Ind	ALP	NLC	Ind
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Compulsory Voting	.304 (.057)	.028 (.007)	.009 (.036)	-.019 (.028)	.007 (.014)	.028 (.036)	-.015 (.030)			
Δ Turnout								.164 (.140)	-.285 (.140)	.120 (.121)
State fixed effects	✓	✓	✓	✓	✓	✓	✓			
1924-1926 election dummy	✓	✓	✓	✓	✓	✓	✓			
N	543	543	543	543	492	492	492	69	69	69
R^2	.714	.089	.069	.027	.113	.080	.032	.024	.043	.010

Note: Table presents estimates from two analyses. Columns 1-7 are analogous to a difference-in-differences design that contrasts the before-after difference in turnout and vote shares at the federal level with a before-after difference at the state level. Before-after refers to the introduction of compulsory voting at the federal level in 1924. Columns 8-10 report results from variation in treatment intensity (change in turnout between 1922 and 1925 elections) across electoral districts on the change of a party's vote share. Standard errors in parentheses are clustered at the state level for columns 1-7; for columns 8-10, heteroskedasticity robust standard errors are reported in parentheses. The unit of observation is at the district level. ALP: Australian Labor Party; NLC: Non-Labor Coalition; Ind: Independents and minor parties.

Columns 8-10 shift the focus from the cross-level analysis to a within-Commonwealth one. Results are based on a within-Commonwealth difference-in-differences analysis that tests the extent to which changes in turnout between 1922 and 1925 are related to changes in support for different political parties across federal electoral districts. As before, this research design is likely to produce internally valid estimates of the effect of compulsory voting for the federal level.

As the coefficient of 0.164 and 0.120 in columns 8 and 10 show, there is a small positive effect from an increase in voter turnout on support for the Labor Party and for independent candidates, respectively. If turnout increased by 10 percentage points between 1922 and 1925 in a federal district, the Labor vote share went up by 1.6 percentage points. This effect is moderate in size, but fails to be statistically significant at $p < 0.05$. However, things look different for the non-Labor coalition parties. The coefficient of -0.285 means that districts with a 10 percentage point increase in turnout saw support for non-Labor parties drop by 2.9 percentage points, an effect that is statistically significant at $p < 0.05$. Thus, while the within-Commonwealth analysis does not find direct support for the positive relationship between turnout and Labor support, the negative relationship between turnout and support for non-Labor parties is consistent with this view and clearly at odds with the minimal effects perspective.

3.6 Conclusion

Political scientists have debated for quite some time whether increasing voter turnout shifts the balance of political power by increasing support for left-wing parties at the expense of right-wing parties. Several recent articles that use natural experiments and leverage an exogenous shock to voter turnout due to the introduction of compulsory voting seem to provide compelling evidence that supports such a class voting perspective: compulsory voting disproportionately increases voter turnout among poorer citizens, thereby shifting the political power in favor of left-wing parties vis-a-vis their right-wing counterparts.

This paper advances a theory of minimal effects that challenges this account. This study ar-

gues that compelling less politically interested and informed voters who would not have voted otherwise to cast a vote on Election Day will at most marginally affect the overall partisan balance. Instead, we are likely to see a movement of voters away from independent candidates and minor parties toward the more visible and well known main parties on both the political left and right. This paper evaluates this theoretical account empirically by reanalyzing a crucial case: the adoption of compulsory voting in Australia in the early 20th century. The empirical findings are rather mixed. At first view, a state-level analysis seems to support the class voting view while a federal-level analysis aligns better with the minimal effects view. However, closer analysis that leverages within-state and within-Commonwealth variation gives a more nuanced picture, showing that class voting and minimal effects view are both supported in some instances and not in others. This suggests that additional factors which have only been hinted at in this study need further analysis. Key among them are parties' strategic responses to the introduction of compulsory voting as well as electoral geography – in particular, the geographical size of electoral districts. With respect to the class voting view, findings in this study suggest that the Australian Labor Party did not consistently benefit electorally from compulsory voting and if it did, benefits were often somewhat smaller than those that had been attributed to compulsory voting in previous research.

However, a word of caution is warranted. The interpretation of the effects from the analyses often face a small N problem. As a result, even substantively meaningful effects frequently fail to be statistically significant at conventional levels. Unfortunately, with observational historical data there is no direct solution to how we could improve on such an analysis because it is impossible for the researcher to rerun history in order to increase the sample size. One possible way forward that has been hinted at in the results section is a more thorough and in depth analysis of specific electoral districts. A second possibility is to match the district-level data in this study with additional district-level covariates to increase efficiency. While certainly possibly, this is a daunting task due to the fact that census districts frequently do not align with electoral districts for the period under investigation.

What are the implications from these findings? First, this study contributes to the debate on

the relationship between political participation and left-wing party support. The findings from this study caution against too optimistic a view on this relationship, suggesting that in some instances higher turnout is unlikely to increase a left-wing party's electoral support. How can we reconcile this finding with studies that have directly linked increasing voter turnout with the adoption of policies that benefit the poor? For example, Fowler (2013) directly attributes an increase in pension spending in Australia to the adoption of compulsory voting in this country. Yet, given that pension spending in Australia was largely a matter for the federal government and that compulsory voting did not shift power towards Labor at the federal level, increasing support for the ALP is unlikely to be the mechanism through which higher voter turnout affected pensions. A possible explanation that could account for the policy shift in pension spending in Australia in the absence of a significant shift in political power from the non-Labor coalition parties to the ALP could emphasize the strategic behavior of the right-wing parties. It is possible that these non-Labor parties adjusted their own policy positions and shifted towards the left in an attempt to appeal to these new, socioeconomically worse off voters. However, while quite plausible, we cannot totally rule out the alternative possibility that there was no direct effect of higher voter turnout on pension spending and that we erroneously attributed the policy shift to the adoption of compulsory voting. Given that most of the empirical leverage in the pension spending analysis in Fowler (2013) comes from a comparison of pension spending in Australia with that of New Zealand and Denmark, a qualitative, in depth analysis of these three cases could provide additional insights into the mechanism that was leading to increased pension spending in Australia in the early 20th century (cf. Castles and Uhr 2007).

Second, this study casts new light on the role of electoral reforms and their ability to help alleviate the problem of increasing economic inequality. Overall, the findings from this study suggest that compulsory voting is an effective strategy to address the issue of *political* inequality – after all, voter turnout increased strongly after the adoption of compulsory voting –, but of limited usefulness as a tool to remedy *economic* inequality. Thus, the present study illustrates both the potential and the limitations of an institutional fix such as compulsory voting. Therefore, compulsory voting is only likely to solve part of the democratic dilemma that was

identified by Lijphart (1997) in his 1996 APSA presidential address: it mechanically increases voter turnout among the poor, but it might not necessarily and automatically guarantee a better political representation of their policy preferences.

Chapter 4

Can Potential Populist Voters Be Framed? Results from a Survey Experiment in the 2017 German Election

4.1 Introduction

The last few years have seen a strong increase in the number of people across established democracies who support right-wing populist parties and politicians. These populist voters' suspicion of elites and establishment actors (Cramer 2016; Mudde 2007) would seem to make them a "lost cause" to mainstream parties. At the same time, mainstream parties have potentially powerful considerations working in their favor, at least some of the time: e.g., good economic performance (Duch and Stevenson 2008), a policy agenda of economic redistribution (Boix 1998), or government experience (Meguid 2005, 349). These include issues that should be of importance even to voters who tend to be susceptible to populist appeals. What are the possibilities and limits of influencing populist voters and having them consider issue dimensions favorable to mainstream parties?

Previous research has shown that the media, parties and politicians are able to prime citi-

zens, that is, to influence their political choices by talking about and thus, raising the salience of some topics while ignoring others (Iyengar and Kinder 1987; Johnston et al. 1992; Krosnick and Kinder 1990; Lenz 2012). To the extent that populist voters represent “frustrated issue publics” (Chou et al. 2018) who care at least to some extent about policies, we would expect them to generally be receptive to priming. After all, it is these people’s feeling that issues that matter most to them, such as restricting immigration (Dinas et al. 2019; Mutz 2018) and alleviating economic hardship that is a result of globalization (Colantone and Stanig 2018a,b; Dal Bo et al. 2018), have been ignored by the established elites that at least partially drives their decision to support radical right parties.

What messages are likely to be most effective for mainstream parties? One possible strategy for mainstream parties is to try to copy the right-wing populist party’s policy position by adopting a tougher stance on issues such as immigration, a strategy that Meguid (2005) calls “accommodative”. However, such a strategy is risky. First, it might be ineffective with populist voters who consider the populist right-wing party as the more credible proponent of radical policy change on issues such as immigration (Meguid 2008, chapter 6). Second, even if such a strategy was effective with some populist voters, it might risk alienating more moderate voters and thus, lead to a net vote loss for the mainstream party (Chou et al. 2018; Krause, Cohen and Abou-Chadi 2019). An alternative and more promising strategy for mainstream parties is therefore to emphasize mainstream issue dimensions where they arguably have a comparative advantage over right-wing populist parties, a strategy that Meguid (2005) has dubbed “dismissive”. Doing so, they might be able to appeal to populist voters without alienating their own core electorate.

For this reason, this study explores to which extent non-populist political messages are able to shift the vote intention of voters who are susceptible to right-wing populist appeals. It proposes a theoretical framework that builds on existing theories of voting behavior that conceives of voters as performance-oriented (Duch and Stevenson 2008), strategic (Downs 1957) and self-interested actors (Meltzer and Richard 1981), but that puts the psychology of potential populist voters centre-stage. In particular, it takes into account that any blunt attempt

to try to influence these voters with openly anti-populist messages is likely not to work or might even backfire by making them not less, but even more supportive of populist candidates (cf. Chong and Druckman 2007, 2010). This framework suggests that by taking these voters' concerns – such as their resentment toward established political elites, their feeling of insecurity and of not getting their fair share of the economic pie (Cramer 2016; Mudde 2007) – seriously, existing theories of voting behavior may help us design mainstream political messages that appeal to this group of voters.

This study tests these expectations through survey experimentation during a real-world election campaign in which populist messages were highly salient on a large pool of roughly 1,800 voting-age German citizens who were specifically selected for this study due to their high propensity to vote for a right-wing populist party. The key finding of this study is that none of the four messages that were tested were able to reduce support for the right-wing populist Alternative for Germany (AfD) party. However, we need to take into account that these null findings are the result of a conservative test where it was attempted to shift potential populist voters' vote intentions only two weeks before a real world election, that is, at a time where most people's vote intentions are likely to have been firmed up. In contrast to these null findings, highlighting the good performance of the Germany economy proved to be a potentially powerful strategy for the incumbent Christian Democrats, increasing their vote share among this group of voters by 6 percentage points from 14% to 20%. Given that the increased support for the Christian Democrats did not hurt the populist AfD party, this suggests that the economic message that was tested was most effective with those dissatisfied and anti-immigrant voters who had abandoned the Christian Democrats not for the populist challenger, but for mainstream opposition parties.

The findings of this study have broad implications for our understanding of populist voters, framing, and party strategies. First, it contributes to debates about strategies that are most successful for mainstream parties in the face of niche party competition. Previous research has primarily focused on strategies that directly challenge the radical right party on immigration policy by either adopting a more liberal or more restrictive stance on this issue. Instead, this

study explores the effectiveness of dismissive strategies that have received less attention so far and that aim at reducing the salience of the immigration issue by focusing the voters' attention on alternative policy considerations. Second, this study provides new insights into the issues beyond immigration that populist voters care about. In a context where immigration is a highly salient issue, this study finds only very limited evidence that these voters are still receptive to mainstream messages. Third, and in line with previous research, this study presents some exploratory analyses that hint at the crucial role of a populist voter's level of political knowledge in moderating their resistance to mainstream messages. It seems that particularly those potential populist voters with less political knowledge might be more receptive to mainstream messages that convey new and relevant information to them.

4.2 Populist Voters and Mainstream Non-Immigration Messages

There is considerable evidence for the ability to shape citizens' political evaluations. For example, the literature on priming has shown that by talking about certain issues and ignoring others, the media, parties and politicians can influence the issues to which citizens pay attention when making political choices (Iyengar and Kinder 1987; Krosnick and Kinder 1990; Johnston et al. 1992). As a result, priming makes some considerations more accessible to citizens than others and doing so, changes the "standards that people use to make political evaluations" (Iyengar and Kinder 1987, 63). Priming is a particularly powerful strategy if it is used with so-called "valence" judgements that focus citizen attention on the performance of the economy or on candidates' personal characteristics (Lenz 2012; Vavreck 2009). In addition, the closely related literature on framing has shown that even in a context of competing frames, stronger frames and those received later in time tend to have a stronger effect on people's opinions (Chong and Druckman 2007, 2010). Recent research has refined this debate by pointing out that it is often not so much the mere priming of an issue, but rather information-based persuasion that

is able to shift citizens' opinions (Leeper and Slothuus 2018). In other words, citizens do not so much reconsider their vote choice simply because an issue has been made more salient to them, but rather because they – advertently or inadvertently – received new information that allowed them to re-evaluate and potentially change their existing opinions.

However, while previous studies have extensively tested the effectiveness of a variety of different frames, these studies have largely been restricted to either the average voter or to small, non-representative samples of college students (Chong and Druckman 2007, 2010; Huber and Lapinski 2006; Lenz 2012; Mendelberg 1997). Yet the effectiveness of framing strategies, especially when combined with mainstream political messages, may be more limited with the group of voters who are least susceptible to these sorts of political appeals: the increasingly large number of citizens in established democracies who tend to distrust and feel abandoned by current political elites. These citizens frequently share a feeling of resentment towards mainstream politicians and perceive them as either unable or unwilling to solve the most urgent problems that they themselves and other people like them face today (Cramer 2016; Mudde 2007). As a consequence, they are likely to be more responsive to the anti-establishment rhetoric and appeals of (often right-wing) populist parties and politicians and rather sceptical toward messages from mainstream parties.

However, things may look different if we acknowledge that these voters are not only protest voters, but citizens who care about policies (Chou et al. 2018) and policy change. It is in this area where mainstream parties may present themselves as the better and more credible alternative to right-wing populist parties, both due to their greater experience of running a country (Meguid 2005, 349), their successful management of the economy (Duch and Stevenson 2008) or a more credible agenda for redistributive economic policies (Boix 1998). To what extent can mainstream parties use their strengths in these areas to connect with populist voters and have them consider issue dimensions that are favorable to mainstream parties?

Following Meguid (2005, 2008)'s seminal research on mainstream parties' strategies in the face of niche party competition, we can distinguish two strategies that mainstream parties can choose from if they want to lure voters away from a right-wing populist party. A mainstream

party can follow an “accommodative” strategy and compete with the radical right party on the latter’s main policy issue. By trying to copy the niche party and adopting, for example, a tougher stance on immigration, the mainstream party can hope to win back some of the voters it has lost to the niche party competitor. However, the empirical evidence about the effectiveness of this strategy is somewhat mixed. While Meguid (2005, 2008) finds that such a strategy reduces electoral support for niche parties, recent research has questioned these previous findings. Krause, Cohen and Abou-Chadi (2019)’s replication of Meguid (2005, 2008) extends the time frame by more than ten years to include the most recent elections and combines both macro-level election data and micro-level survey data, but fails to find any negative effect of this strategy on radical right party support. In addition, recent survey experimental research into right-wing populist party support has cast doubt about the usefulness of this strategy for mainstream parties. For example, Chou et al. (2018) show that adopting a right-wing populist party’s anti-immigrant policy position is likely to lead to a net loss for mainstream parties because any increase in support from previous radical right voters is more than offset by core mainstream party voters who would abandon the mainstream party. Thus, even if an accommodative strategy might reduce electoral support for a right-wing populist party, it might not be a viable strategy for a mainstream party if that strategy weakens its own electoral position and undermines its chances of being reelected.

For these reasons, the “dismissive” strategy might be a better alternative for many mainstream parties. A mainstream party that follows this strategy reduces the salience of the niche party’s core policy issue by primarily campaigning on alternative programmatic issues where the mainstream party has a natural comparative advantage over the niche party competitor. So instead of trying to replace the niche party as the better anti-immigrant party, a mainstream party could appeal to populist voters by highlighting its strength in alternative areas such as

income redistribution, health care or other social policies.¹ Assuming that some right-wing populist voters do not only or even primarily care about immigration, but also about other policy issues that directly affect them – especially welfare (Chou et al. 2018) and economic issues (Colantone and Stanig 2018a,b) –, this mainstream party strategy may be able to lure them away from the right-wing populist party. Previous research that empirically tests the effectiveness of the dismissive strategy is limited (but see Meguid 2005, 2008). In particular, there is very little research that systematically tests which non-immigration issue might be most appealing to right-wing populist voters. One exception is Chou et al. (2018) who, in a conjoint analysis, find suggestive evidence that right-wing populist voters prefer “candidates who are attentive to pensions and who propose increasing taxes on the rich” (Chou et al. 2018, 29).

The choice of the strategy – accommodative versus dismissive – affects the issue that mainstream parties may use to appeal to voters, but it does not tell us much about the type of messages, that is, whether a message is mainstream or populist. I call a message *populist* if it fulfills two criteria:² First, it proposes a radical policy shift from the current policy status quo. Second, this radical policy shift is presented as an effective solution to a perceived policy problem, but it is an ineffective or suboptimal way of addressing the problem.³ For example, most right-wing populist parties support a drastic reduction of any future immigration or even a total ban of certain immigrant groups to their country. Such a policy position represents a marked shift from the status quo in all liberal democracies. Right-wing populist parties frequently justify their tough stance on immigration as the best way to address an existing policy problem (e.g. a supposedly high crime rate or labor market repercussions from technological change).

¹Meguid (2005, 2008) identifies a third mainstream party strategy, which she calls adversarial. In this case, a mainstream party competes with the niche party on the latter party’s core issue, but instead of adopting a position more similar to the niche party, the mainstream party emphasizes how its policy stance is different from the niche party’s. This strategy will generally increase electoral support for the niche party (Ibid.) – possibly partially fuelled by a backlash effect (cf. Chong and Druckman 2007, 2010) among voters who are susceptible to populist appeals – and therefore, is not a viable strategy to reduce niche party support.

²I thank Milan Svolik for shaping my thinking on these two criteria.

³In line with standard definitions of populism in the literature, this definition does not link populism to any particular policy issue, but instead highlights its anti-establishment character (Mudde 2007; Mudde and Kaltwasser 2017; Norris and Inglehart 2019). It is different from these other definitions in more explicitly spelling out how this anti-establishment character translates into policies that are supported by populist parties.

However, while the proposed populist “solution” might seem appealing to some people as an easy fix to a serious policy problem, it is either unlikely to solve the problem or, if it does, comes at a higher cost for certain societal groups (e.g. people in economic sectors that benefit from immigration) than alternative, less radical policy solutions. In other words, it is not so much the fact of wanting to reduce immigration levels that makes a policy position populist, but rather the extremeness with which populist radical right parties want to achieve this goal. In contrast, I define as “mainstream” a message that is not populist, that is, that does not fulfill these two criteria.

In line with this study’s analytic goals, its focus is on mainstream messages that can be used as dismissive strategies by mainstream parties. Given that immigration is the core issue for most right-wing populist parties in Europe, a dismissive strategy has to focus non-immigration issues. Thus, this study analyzes mainstream messages that are not on immigration. For simplicity, it uses *mainstream message* as a short form for “mainstream non-immigration message”.

Finally, the context in which citizens are exposed to new messages is likely to matter, too. All else equal, we would expect that shaping potential populist voters’ vote intention with mainstream messages during and especially toward the end of a real-world election campaign will prove more difficult than early in a campaign. Over the course of an election campaign these voters will have been exposed to many frames and counterframes – including both mainstream and populist ones – and party cues leaving little room for any new mainstream message to alter their vote choice late in the campaign (Chong and Druckman 2007; Druckman 2004; Kalla and Broockman 2018). This is particularly likely to be true for many established democracies these days where major events, such as the economic repercussions of the 2008 financial crisis and the influx of Syrian and other refugees since 2015, have helped raise the salience of issues that benefit especially right-wing populist parties and their anti-immigrant policy agendas.

4.2.1 Performance, Policies, and Uncertainty

Despite the challenges to shape the vote choice of citizens who are susceptible to right-wing populist appeals with mainstream messages, carefully crafted non-populist messages may shift these people's vote away from populist and toward mainstream parties. Grounded in existing theories of voting behavior, this study presents four alternative theoretical logics of framing that specifically focus on this group of voters and that outline the conditions under which we would expect these citizens to be swayed by non-populist messages. Based on these theoretical logics, a series of hypotheses are specified in this section. All hypotheses were registered in a pre-analysis plan at the EGAP website before data access by the researcher.⁴

The first of these theoretical logics assumes that, in line with a long literature on economic voting (e.g. Duch and Stevenson 2008; Powell and Whitten 1993), citizens who are inclined to vote for populist parties are rational retrospective voters who care about the performance of the economy. Just like any other voter, they want their country's economy to do well and evaluate incumbent politicians accordingly. In particular, they use information about past economic performance to assess the economic competence of the incumbent government. This assessment influences their selection of future government parties that they want to be economically competent (Duch and Stevenson 2008, 28-31).⁵ As a consequence and despite their fundamental dissatisfaction with some of the government's policies (e.g. on immigration), these voters will be responsive to messages that highlight the current state of the national economy (cf. Alt, Lassen and Marshall 2016; Lenz 2012; Vavreck 2009). They are expected to support the incumbent party or, in the case of coalition governments, the party of the incumbent prime minister if the economy is doing well and to prefer other parties if this is not the case. Thus, emphasizing the good economic performance is likely to increase support for the incumbent whereas highlighting economic difficulties will decrease it. Applying this to a national context where the economy is doing well, this leads to:

⁴The EGAP preregistration ID number is 20170920AA.

⁵In contrast to such a selection model, a sanctioning model emphasizes much more voters' desire to punish bad incumbents for past economic performance at the ballot box (Duch and Stevenson 2008, 10-15).

Hypothesis 1 (Economic Voting): Emphasizing and providing information about the economy when the economy is doing well in a country makes voters who are susceptible to populist appeals

- a) less likely to vote for the right-wing populist party and
- b) more likely to vote for the party of the incumbent prime minister or chancellor.

Just like for other citizens, populist voters' vote choice is likely to be shaped by both strategic (Downs 1957; Cox 1997) and expressive (Pons and Tricaud 2018) motivations. Given their general level of dissatisfaction with the established elite, some populist voters may use their vote strategically and vote for an "outsider" party to throw the incumbent parties or politicians out of office. In contrast, those who are more strongly driven by expressive motivations might care less about who will be in power after the election and vote for populist parties primarily to signal their protest to incumbents (Mudde 2007, 226-229). Yet in some situations, these two motivations may be in conflict with each other. For example, many of these voters share a feeling that the current political elite builds a sort of cartel in which politicians are primarily interested in clinging to political power and no longer offer true policy alternatives (cf. Katz and Mair 1995), something that has been found to drive up support for populist challenger parties (Kriesi 1999, 419-420). However, in order to disturb this political cartel, it might be more effective not to vote for a populist party, but rather for a mainstream opposition party. This is especially true in the context of a grand coalition government where a strategic voter might want to strengthen mainstream opposition parties so that this opens up viable coalition alternatives to the existing grand coalition government. These strategic versus expressive motivations will be most starkly in contrast in situations where the major mainstream parties have declared one or more populist parties as uncoalitionable. In such a context, casting a protest vote for a populist party to signal one's opposition to the grand coalition government will make it more difficult to build majority coalition governments that do not need the support of both of the two major mainstream parties. Therefore, telling populist voters about their option to cast their vote strategically not for their preferred (i.e. populist) party, but against the continuation of the grand coalition might sway the more strategically minded among them.

Hypothesis 2 (Strategic Voting): Priming the strategic aspect of their vote and informing them about how to use it to reduce the electoral chances of a future grand coalition will make voters who are susceptible to populist appeals

- a) less likely to vote for the right-wing populist party and
- b) more likely to vote for the mainstream opposition parties who are potential future coalition partners outside of a grand coalition.

Voters may support politicians or political parties not so much for their past economic performance in office or because they are attracted by their policy promises (Lenz 2012), but also because they expect from them a reduction in political and economic uncertainty (cf. Jacobs and Matthews 2017). This might be particularly true for voters who lean toward (right-wing) populist parties. These people often share the feeling that due to macro-level developments such as globalization, the recent surge in mass immigration or economic and political crises, things are getting worse for them (Colantone and Stanig 2018*a,b*; Cramer 2016; Mudde 2007; Mutz 2018; Norris and Inglehart 2019) and they feel insecure and vulnerable. As a consequence, they may be less opposed to mainstream politicians who they think may credibly reduce uncertainty from these international economic and political risks. This will especially be true for incumbent mainstream politicians who have proven their leadership skills in previous international negotiations with other world leaders. Thus, explicitly mentioning uncertainty and the importance of leadership characteristics might make populist voters more likely to vote for the incumbent and her party, especially if the incumbent is generally perceived to be a capable leader with strong leadership skills.

Hypothesis 3 (Uncertainty): Emphasizing political uncertainty and the importance of leadership characteristics makes voters who are susceptible to populist appeals

- a) less likely to vote for the right-wing populist party and
- b) more likely to vote for the party of the incumbent prime minister or chancellor.

Finally, voters who are dissatisfied with mainstream politicians are likely to care not only about performance, but also about policies. In many cases it is these citizens' very disagreement with how politicians have handled certain issues – from immigration to Eurozone finan-

cial bailouts and trade policy – that has pushed them to consider voting for populist alternatives in the first place (Chou et al. 2018; Colantone and Stanig 2018^{a,b}; Mutz 2018). A long line of research in political science that goes back to Meltzer and Richard (1981)’s seminal article starts from the assumption that citizens’ self-interest shapes their attitudes toward economic redistribution and their vote choice. Voters who are leaning toward populist parties are likely to be no different in this respect. What is more, many of them are “economic losers” (Dal Bo et al. 2018) who have the feeling of not getting the share of the economic pie that they deserve and of being ignored by mainstream politicians (Cramer 2016). Therefore, explicitly addressing the issue of social inequality and supporting policies that aim at effectively reducing the gap between rich and poor will allow left-wing parties to win back some of these alienated voters. This will particularly be true if left-wing parties manage to convince these voters that they can be trusted to adopt policies that benefit these voters once they are in power (cf. Kuziemko et al. 2015). In a context where extreme left parties are considered uncoalitionable by the mainstream left or where voting for the more moderate left-wing party increases the chances of a future left-wing government, support will mostly go to the mainstream left-wing party. In contrast, in situations where this is not the case, voters who care about redistribution may choose the far-left party with its more extreme redistributive policy agenda instead in order to achieve a more substantial left-wing policy shift on redistribution (Kedar 2005). For the German context, this leads to:

Hypothesis 4 (Social Inequality): Emphasizing social inequality combined with information about the main left-wing party’s policy positions and their main competitors’ positions on this issue makes voters who are susceptible to right-wing populist appeals

- a) less likely to vote for the right-wing populist party and
- b) more likely to vote for the main left-wing party strongly supporting redistributive policies.

The next section discusses a survey experiment that was conducted during the 2017 German

federal election to test these hypotheses.⁶

4.3 Data and Methods

4.3.1 Data

The survey experiment was specifically designed to sample a large group of voters who are susceptible to populist appeals in the context of a real-world election campaign where populist messages were prevalent. It is on this group of voters that is least likely to be swayed by non-populist messages that the above hypotheses will be tested.

Campaign Context. The survey experiment was conducted shortly before the 2017 German federal election. This was the first federal election after Germany had seen a large influx of mostly Syrian refugees in 2015 and 2016, with a peak of 890,000 asylum seekers registering with German authorities in 2015 alone.⁷ This influx of refugees in combination with reports in German media about crimes committed by groups of young men from mostly North African countries in several German cities led to a sharp increase in support for the anti-immigrant populist Alternative for Germany (AfD) party in opinion polls and state elections across Germany in 2016 and 2017. In this context the survey was fielded between September 7 and September 15, 2017, that is, shortly before the German federal election held on September 24, 2017 and thus, in the peak of the election campaign.⁸ Respondents were surveyed between September 7 and September 15, 2017, that is, roughly two weeks before the election on September

⁶The registered pre-analysis plan also specifies a series of hypotheses that test for heterogeneous treatment effects among strongly and weakly populist voters. However, the separation of survey respondents into strongly and weakly populist voters based on demographic characteristics proved more difficult than initially assumed, making any results highly sensitive to the researcher’s judgement about how these two groups are built. For this reason, the study focuses only on the pre-registered hypotheses of the treatment effects for the whole sample of populist voters.

⁷Numbers are from the Office of Migrants and Refugees (BAMF), Germany.

⁸YouGov Deutschland administered the survey and I did not have access to any of the survey data before its official pre-registration on EGAP.

24, 2017.⁹ As a result, most respondents in the survey will have been extensively exposed to real-world campaign messages of the 2017 German federal election by the time they take the survey. Most importantly, the AfD party heavily criticized the incumbent government and German chancellor Angela Merkel for their immigration policies during the campaign, thus priming the issue of immigration in this election.

Sampling procedure. This study is interested in the voting behavior of people with a high propensity to vote for populist parties. To identify these people, it uses respondents' anti-immigration attitudes as a proxy for their susceptibility to vote for the German AfD party. Doing so has several advantages over alternative approaches. Especially in the German context where the recent surge in support for the populist AfD is directly linked to the salience of the immigration issue, focusing on immigration attitudes is probably the best way to tap respondents' *potential* to vote for populist parties. Consistent with this assumption, data from the German Longitudinal Election Study (GLES) show that in the fall of 2017 anti-immigration attitudes among German voters are highly predictive of AfD vote in that election.¹⁰ On the other hand, asking respondents in the pre-screening question directly about their attitude toward the AfD would likely have restricted the pool to the narrow group of hard-core AfD partisans. Furthermore, asking respondents about their partisanship shortly before the measurement of the outcome variable, i.e. vote intention, might have suppressed any treatment effects from non-populist priming messages.

Participants were recruited by the survey firm YouGov. In order to gauge respondents' anti-immigration attitudes, this study used a pre-screening question that directly asked respondents about their immigration attitudes and retained only those respondents from the YouGov online panel with the most extreme anti-immigration attitudes for the survey experiment. The pre-screening question is a slightly adapted standard item from the GLES tapping respondents' position on the libertarian-authoritarian value dimension. It reads as follows (English transla-

⁹The large majority of respondents (77%) filled in the survey after September 11, 2017.

¹⁰For example, AfD vote intention in the GLES pre-election survey is only 2.59 percent for those who score 8 and below on the 1 to 11 anti-immigration scale in the GLES while it is much higher, at 20.16 percent, for those who score 11 on this scale.

tion): “Do you think that immigration to Germany should be made rather easier or rather more difficult?”¹¹ Respondents indicate their support or opposition to this statement on an 11-point scale where higher values correspond to a more anti-immigration attitude. In order to administer the survey experiment only to the most anti-immigrant third of respondents, YouGov started by retaining only respondents from categories 8 to 11 on this pre-screening question and, after an initial soft launch, narrowed it further down to only include respondents from the most anti-immigrant category 11. As a result, the vast majority of respondents in the sample (1,664 out of 1,786) fall into the highest (i.e. most anti-immigration) category on this variable.

Sample characteristics. The final sample consists of 1,786 German voting-age citizens with a high propensity to vote for the anti-immigrant populist AfD party. This sample is largely representative of the overall German population with respect to gender, age and education.¹² However, as intended by the pre-screening, the sample is *not* representative with respect to respondents’ partisan affiliations. There is a clear over-representation of partisans for the right-wing populist AfD party, with most respondents (21%) in our sample identifying with this party.¹³ Similarly, AfD vote intention among people in the sample is at 27%¹⁴ and thus, substantially higher than the average support for this party in the German population, which was at around 10% at the time of the survey.

The survey itself is administered online. In contrast to other surveys like the GLES which are conducted by phone or personal interview, administering the survey online has the advantage that results are less affected by social desirability bias (Gooch and Vavreck 2019). In particular, respondents are expected to more truthfully answer the pre-screening question about

¹¹“Sind Sie der Meinung, die Zuzugsmoeglichkeiten fuer Auslaender sollten eher erleichtert oder eher eingeschaenkt werden?” To the extent that the pre-screening question primes the issue of immigration in respondents’ minds and thus, raises the salience of this issue, the results in this study will represent a rather conservative estimate of the effect of non-populist messages on these voters.

¹²There is a small under-representation of less educated people and, quite surprisingly, of very young people in the sample. The share of people aged 18-29 in the sample is 9% whereas their share is 14% in the overall population. Data on the German population are from the website of the German statistical office.

¹³Support for the two major mainstream parties, the CDU/CSU and the SPD, follows at 18% and 14% respectively. See summary statistics in Appendix for details.

¹⁴This number is calculated based solely on people in the control group.

their anti-immigration attitudes and express their vote intention for the right-wing populist AfD party when they fill in the survey at their own computer at home than if they were asked these questions in a direct conversation with an interviewer.

4.3.2 Dependent Variables

Four dichotomous variables measure respondents' vote intention for each of the following four parties or combinations of parties: the right-wing populist Alternative for Germany (AfD) party, the center-right Christian Democratic Party (CDU/CSU), the center-left Social Democratic Party (SPD), and for the two mainstream opposition parties, the Liberals and the Greens (FDP/Greens), combined. For each of these variables, respondents are coded as 1 if they picked the respective party as their answer to the survey question "Which party would you vote for if there was a federal election this Sunday?"¹⁵ and as 0 otherwise.¹⁶

4.3.3 Independent Variables

Treatments. The survey experiment has four treatments and all 1,786 participants were randomly assigned to either the control or one of the four treatment groups. The treatments correspond to hypotheses 1 to 4 that are specified in the theory section above. In each case, a treatment consists of roughly two sentences that directly precede the question that taps a respondent's vote intention in the upcoming election and are displayed on the same screen as the vote intention question. The four treatments read as follows (English translation):¹⁷

Economic performance: "The German economy is doing very well, with unemployment at a historic low. The upcoming election will shape Germany's economic prospects for the next years."

¹⁵This question was asked in German and represents the standard survey item tapping respondents' vote intention that is used in election surveys in Germany. For the original German translation, see the Appendix.

¹⁶The focus of this study is on respondents' vote intention. Results with an alternative dependent variable of party favorability, which was part of the registered pre-analysis plan, are reported in Table C.1 in the Appendix.

¹⁷The German text of the treatments can be found in the Appendix.

Strategic voting: “The CDU/CSU is very likely to win the federal election and form the new government. Whoever wants political change has only one option: to make the FDP or the Greens as strong as possible in order to avoid another four years of a Grand Coalition.”

Uncertainty: “These are uncertain times and the world order is unstable. In times like these many think it is important for Germany to have an experienced leader.”

Social inequality: “In the upcoming election the SPD wants to make Germany fairer, provide more support to people who are struggling, and have the rich pay more in taxes. The CDU/CSU and the FDP oppose those changes. Thus, the outcome of the election is likely to have a direct impact on the extent of social inequality in Germany.”

Based on assignment status, four dichotomous variables indicate whether a respondent received either of the four treatments.

Finally, it is worth emphasizing that two of the treatments, the economic voting and the social inequality message, do not only prime respondents, but also contain informational components about the different political parties in the election. The former provides information about the current economic situation of stable economic growth and low unemployment in Germany and, at least implicitly, links this to the performance of the parties in government. The latter explicitly informs respondents about the policy positions of the German mainstream parties on the issue of social inequality, telling them which parties are willing to raise taxes in order to redistribute income from the rich to the poor and which parties do not.¹⁸ As a result, any effects that this study will uncover from these two treatments could be a combination of pure priming (or “emphasis framing”) and information-based persuasion (see Leeper and Slothuus 2018). To the extent that these two treatments prove more effective than the other two treatments, this would be consistent with recent empirical findings in the framing literature that show that information-based persuasion tends to be more effective than pure emphasis framing (Leeper and Slothuus 2018).

Political Knowledge. The survey includes five questions that tap different aspects of re-

¹⁸The strategic voting message also contains an informational component by telling people that the CDU/CSU is likely to win the election and by explicitly naming potential parties that might help avoid a new grand coalition.

spondents' political knowledge.¹⁹ Two of the questions are about procedural aspects of the German electoral system: One of them is about the precise election threshold of valid votes that parties need to win in order to be allocated any seats in the national legislature (answer: 5 percent); the other asks respondents which of the two votes each voter has in the election is more important for the allocation of seats in parliament (answer: the second vote). The other three questions gauge the extent to which respondents are informed about the election campaign itself by asking them to name the party each of the lead candidates from three smaller parties (CSU, FDP, Greens) belongs to.²⁰ Based on the additive index of political knowledge from these questions ranging from 0 to 5 ($\alpha = .72$), a knowledge dummy variable was created where respondents below the mean are coded as less politically knowledgeable and those above it as more politically knowledgeable.

Control Variables. To increase efficiency, the analysis that was pre-registered online includes a series of standard control variables that have been shown to influence citizens' voting behavior. The most important of these variables is *party identification*. To avoid any priming of partisanship and thus, confounding of the treatment effects, this item was not included in the survey itself. Instead, it is based on party identification information that is routinely collected and regularly updated by YouGov for all members of their online panel. Using this information, six partisanship indicators are created, one each for AfD, CDU/CSU, SPD, FDP/Greens, and mainstream parties' (i.e. CDU/CSU, SPD, FDP, Greens, the Left) combined partisanship, as well as one for those with no partisanship at all.

As socio-demographic control variables, this study includes indicators for gender, different age groups, region (East versus West Germany), levels of education (bottom tier, medium tier, top tier), income groups (including a dummy for respondents with missing income values), trade union membership, and most important topic (immigration, terrorism, crime). Finally, a dichotomous variable indicates if a respondent does not fall into the most extreme (i.e. re-

¹⁹The section on political knowledge is not part of the pre-analysis plan.

²⁰While the knowledge questions were asked after randomization, these questions are about factual knowledge. As a result, the knowledge variable is likely to be "a measure of pre-randomization conditions, and treatment assignment had no effect on measurement error" (Lin, Green and Coppock 2016, 16).

strictive) category of the pre-screening question about immigration attitudes, which is the case for 122 of our 1,786 respondents. With the exception of union membership and income, all control variables were measured before respondents were exposed to a treatment. In the case of union membership and income, it is assumed that responses to these factual questions are unaffected by the treatments. Following common practice in the literature, missing values for trade union membership, party identification and most important topic are treated as absence of these things and are coded as 0.

4.3.4 Methods

To test the hypotheses that were specified above, I estimate the following OLS model²¹

$$Y_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \beta_4 T4_i + \gamma X_i + \epsilon_i \quad (4.1)$$

where Y_i is a respondent's vote intention toward one of the parties (1) AfD, (2) CDU/CSU, (3) SPD or (4) FDP/Greens,²² $T1_i$ to $T4_i$ are indicators for the four campaign message treatments of economic performance, strategic voting, uncertainty and social inequality primes respectively, X_i is a vector of covariates and i indexes individual respondents. One-tailed significance tests are used if the direction of a hypothesis was explicitly specified in the pre-analysis plan (see Table 4.1). In all other cases, two-tailed significance tests are adopted. In all cases, Huber-White robust standard errors are calculated. Based on the hypotheses, we would expect negative coefficients on all four treatments with AfD vote intention and positive coefficients

²¹Unless explicitly stated otherwise, the analysis follows the study design that was pre-registered on EGAP, with two additions. First, the models in the pre-analysis plan included a dummy variable to identify the 122 respondents that are less anti-immigrant than the rest of the sample. Initial analyses suggest that these 122 citizens react differently to the four treatments than the other 1,664 respondents. Therefore, interaction terms between this dummy and the treatment variables should have been included in the pre-registered model and were added in the analysis. Second, the pre-analysis plan explicitly breaks down the party identification variable only for the model on AfD vote intention, but fails to do so for the models for the other political parties. Models of the vote intention for the other parties are therefore specified analogously by: (1) including a party identification dummy for the party whose vote share is analyzed by a given model, (2) an AfD partisanship dummy and (3) a dummy for no partisan affiliation (reference category: partisanship for all other mainstream parties).

²²FDP and Greens are treated here as a single party so as to capture support for mainstream opposition parties more generally.

on $T1_i$ and $T3_i$ with CDU/CSU vote intention, $T4_i$ with the SPD and $T2_i$ for the FDP and Greens. For additional analyses on how political knowledge conditions the effect of the four treatments, regressions additionally include a political knowledge dummy and its interaction with each of the four treatments to the model.²³

All models control for a respondent's answer to the pre-screening question about their immigration attitudes by including an indicator for those less opposed to immigration and interactions between this indicator and the four treatments.²⁴ This serves to avoid any confounding of our estimates by the 122 respondents who are less hostile to immigration and thus, are likely to respond differently to the treatments.

Table 4.1 summarizes the four treatments and the expected effects from each of them on the electoral support for different German political parties: the Christian Democratic Union (CDU/CSU) of incumbent chancellor Angela Merkel, the main left-wing Social Democratic Party (SPD), the right-wing populist Alternative for Germany (AfD) party and the two mainstream opposition parties, the Liberals (FDP) and the Greens.

Table 4.1: Expected Effect on Electoral Support

	(1) AfD Populist	(2) CDU/CSU Incumbent	(3) SPD Left-Wing	(4) FDP/Greens Opposition
Economic Performance (H1)	–	+		
Strategic Voting (H2)	–			+
Uncertainty (H3)	–	+		
Social Inequality (H4)	–		+	

– : decreased support; + : increased support. AfD: far-right populist party; CDU/CSU: centre-right Christian Democrats/Christian Social Union (party of incumbent chancellor); SPD: centre-left Social Democrats (coalition partner in grand coalition); FDP: Liberals; empty cells: no predictions from theoretical model.

Multiple Comparisons Adjustment. Due to the large number of hypotheses that are tested in this study, the p-value threshold to determine whether a treatment is statistically significant

²³The model with the knowledge dummy and its interactions was not specified in the pre-analysis plan.

²⁴These interactions are not included in the pre-analysis plan.

needs to be adjusted. In classical hypothesis testing, the “alpha value” indicates a researcher’s willingness to commit a Type I error. With the commonly accepted alpha level of 0.05, we would expect to incorrectly reject the null hypothesis about 5% of the time. However, the risk to incorrectly reject a true null hypothesis increases with the number of hypotheses that are tested. A standard way to counteract this risk is to adjust the target p-value downwards so that when this new threshold is used for hypothesis testing, the chance of making a mistake stays at the alpha level of 0.05. Based on the frequently used Bonferroni correction, we would obtain a new target p-value of 0.00625 ($= 0.05/8$) by dividing the p-value of 0.05 by the number of hypotheses (in this case, eight) that are tested. However, the Bonferroni correction is increasingly considered as too restrictive because it does not account for the fact that the multiple comparisons tend to be correlated with each other, commonly leading researchers to not reject null hypotheses that should be rejected, that is, to commit a Type 2 error (Coppock 2015; Gelman, Hill and Yajima 2012). Therefore, an approach that provides a better compromise between trading off Type 1 against Type 2 error is to simulate the error rate and use results from the simulation to determine the appropriate p-value for hypothesis testing (Ibid.). Following Coppock (2015), this procedure gives us a target p-value of $p=0.00841$ that is slightly larger than the p-value from the Bonferroni correction. Based on this new threshold, a treatment effect from any of the 8 statistical tests in this study with a p-value below $p=0.00841$ can be considered statistically significant at the 95% level.

4.4 Results

Table 4.2 presents the results of the survey experiment. It shows the effect of each of the four mainstream messages on the vote intention in the 2017 German federal election for people who are very strongly opposed to any further immigration to Germany and thus, susceptible to appeals from the anti-immigrant populist Alternative for Germany (AfD) party. The coefficients that are displayed in bold directly test the hypotheses that were specified above. Column 1 shows the effect of the four treatments on support for the Alternative for Germany (AfD) party.

Based on these results and contrary to what was expected, none of the four messages leads to a reduction in overall support for the Alternative for Germany (AfD) party among this group of voters. All four coefficients are substantively small and indistinguishable from 0. Thus, the main hypotheses are not confirmed. None of the four messages seems to have an impact on support for the populist party.

However, the situation looks a little bit different for the treatment effects that are anticipated to be positive, which are displayed in columns 2 to 4. Focusing exclusively on the four coefficients in bold which test the specified hypotheses, two of the four coefficients of primary interest are substantively large and in the expected direction: The coefficients of 0.059 and 0.038 in column 2 suggest that providing messages to respondents about the good performance of the German economy and appealing to their desire for less uncertainty both benefit chancellor Angela Merkel and her incumbent CDU/CSU and increase support for this party among anti-immigrant citizens by 5.9 and 3.8 percentage points respectively.²⁵ However, accounting for the multiple comparisons in this study, only the former, that is, the economy frame, is statistically significant at the 95% level with a p-value of 0.007, which is below the target p-value of 0.00841. The p-value of 0.047 of the uncertainty frame is well above this threshold and thus, not statistically significant. Finally, there is no evidence that either the strategic voting or the social inequality frame work on these voters. Both coefficients are close to 0 and not statistically significant at conventional levels.

Based on these regression estimates, Figure 4.1 illustrates the effects of the treatments on mainstream parties graphically. For each of the three mainstream parties, it plots the predicted vote intention for the control group and to the left of it, the predicted vote share after exposure to one of the treatments. The CDU/CSU vote share, which is relatively low in this sample with only about 14%, climbs to roughly 20% and 18% after exposure to the economic performance and the uncertainty frames. The two treatments for the FDP/Greens and the SPD leave the overall vote intention for either party more or less unaffected at around 11% and 17%

²⁵Models without control variables, which are not part of the pre-analysis plan, produce coefficients of 0.035 (instead of 0.059) and 0.005 (instead of 0.038). The difference in effect sizes is likely to be due to imbalances in respondents' partisanship between the treatment and control groups.

respectively.

Table 4.2: Vote Intention in 2017 German Federal Election

	(1)	(2)	(3)	(4)
	AfD	CDU/CSU	SPD	FDP/Greens
Economic Voting	-0.0085 (0.027)	0.059 (0.024)	-0.015 (0.022)	-0.046 (0.019)
Strategic Voting	0.0014 (0.027)	0.044 (0.023)	-0.043 (0.022)	-0.0073 (0.021)
Uncertainty	0.0038 (0.027)	0.038 (0.023)	-0.014 (0.022)	-0.037 (0.019)
Social Inequality	-0.000087 (0.027)	0.0052 (0.021)	0.0047 (0.022)	-0.010 (0.020)
Constant	0.17 (0.044)	0.018 (0.037)	0.066 (0.039)	0.090 (0.034)
Controls	Yes	Yes	Yes	Yes
Observations	1786	1786	1786	1786
R^2	0.45	0.41	0.42	0.27

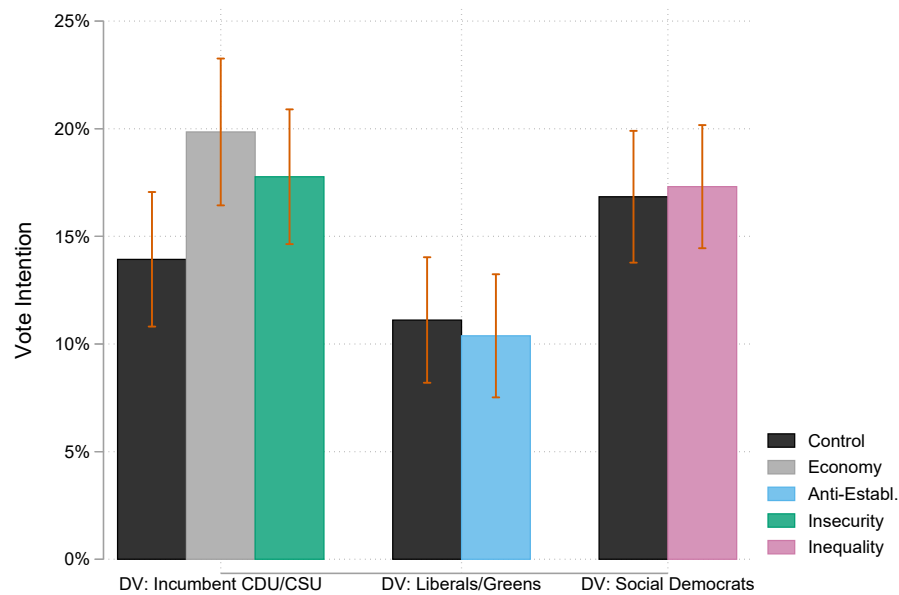
Note: Dependent variable is vote intention in 2017 German federal election for the party indicated at the top of each column. OLS regression with control variables: indicators for region (East vs. West), age group, gender, income group, education level, party identification, and most important topic (immigration, crime, terrorism).

Models control for 122 respondents who are less anti-immigrant (indicator and interaction with treatments).

Coefficients of primary interest for hypothesis tests are in bold.

All in all, the evidence provides very limited support that citizens susceptible to AfD appeals are responsive to non-populist campaign messages. Most importantly, exposure to these messages does not lead to an overall reduction in electoral support for the German right-wing populist party. Results in Table 4.2 do not provide support for any of the (a) variants of the four hypotheses that predict a decrease in the vote share for populist parties (H1a, H2a, H3a, H4a). However, what we see is some shift in vote intentions between mainstream parties: Increased popularity of the incumbent CDU/CSU primarily comes at the expense of small mainstream opposition parties like the liberal FDP and the Greens (see column 4 in Table 4.2). Evidence from Table 4.2 and Figure 4.1 support the (b) variants of the economic voting hypothesis (H1b) whereas there is little support for the uncertainty, strategic voting and the inequality hypotheses

Figure 4.1: Share of Respondents Intending to Vote for Mainstream Parties, by Treatment Condition



Note: DV = dependent variable. Graphs include 95% confidence intervals.

(H2b, H3b, H4b).²⁶ This may indicate that rather than reaching those hard-core populist voters who have already turned to the populist AfD party, the economic voting message primarily manages to win back dissatisfied voters who have defected to other mainstream parties.

A second observation from the results in Table 4.2 is that they seem to align well with Lenz (2012)'s previous findings about the superiority of performance primes over policy primes: Even with strongly anti-immigrant citizens, priming the economy – a valence issue – proves a more effective means to win votes by the incumbent party than priming redistributive policy – a position issue –, which does apparently not manage to shift votes. However, as the next section shows, this conclusion may need some qualification once the group of strongly anti-immigrant voters is split into different levels of political knowledge and people with low and high political knowledge are analyzed separately.

²⁶There is some suggestive evidence in Table 4.2 that the strategic voting message might have shifted some votes from the SPD to the CDU/CSU. While such a shift is not part of the hypotheses that were specified above, it would be consistent with the overall goal of this treatment, which is to increase the vote share for a possible governing coalition among mainstream parties that would not include the SPD. Coefficients are slightly above the conventional threshold of $p=0.05$ and thus, well above the target p -value of 0.00841 that is used in this study.

4.4.1 Exploratory Analyses: The Role of Political Knowledge

A possible reason why the previous analysis failed to find any framing effects on the support for the anti-immigrant populist AfD party could be that voters respond differently to these messages. For example, extensive previous research has shown that a person's level of political knowledge affects how she processes new information and how likely this new information is to change her beliefs (e.g. Zaller 1992). Thus, this section explores the extent to which a respondent's responsiveness to the various treatments may be moderated by her level of political knowledge.²⁷

How would we expect a populist voter's level of political knowledge to affect the previously discussed treatments? In order for a new message to shift a person's vote intention, it both needs to be received and accepted by this person. In other words, new information needs both to be available and accessible to the voter in order for it to shift her opinions (Zaller 1992). While *availability* of new information is less of a concern in the context of a survey experimental intervention where both less and more knowledgeable respondents receive the same pieces of information, there will be differences between these two groups with respect to the *accessibility* of this information, that is, with how they process this information (see also Chong and Druckman 2007, 2010). Following Zaller (1992), more knowledgeable voters with an inclination toward right-wing populism would be expected to be more likely to resist mainstream primes that try to shift their vote away from these parties and toward non-populist parties. Due to their higher level of political attentiveness, these voters will already be aware of arguments both in favor and against different political parties and thus, their vote intention is less likely to be moved by a single message in a survey. Less knowledgeable voters, on the other hand, will lack the contextual knowledge necessary to evaluate and resist the information conveyed in the treatment messages and as a result, be more likely to change their vote. In other words, less knowledgeable populist voters are expected to be more likely to respond to the treatments in this study than more politically knowledgeable ones.

²⁷This exploratory analysis on the moderating effect of political knowledge was not pre-registered.

To examine the moderating effect of political knowledge, Table 4.3 shows results from models that interact each of the treatment variables with a political knowledge indicator. Overall, there is some evidence that is consistent with moderation by political knowledge, in particular for the economic voting and the uncertainty framing in column 1. Each of these two treatments is associated with a strong decrease in support for the right-wing populist AfD party among less knowledgeable respondents, by 8.9 and 9.9 percentage points respectively. In line with the theoretical expectation about the greater resistance to these messages by more knowledgeable voters, these two treatments do not reduce AfD support among more knowledgeable voters. At 4.1 ($= 13.0 - 8.9$) and 6.1 ($= 16.0 - 9.9$) percentage points respectively, the effects are much smaller and have also changed in direction, suggesting that these two treatments would make more knowledgeable voters rather more – and not less – likely to vote for the populist AfD party in an election.²⁸ Figure 4.2 plots the predicted vote intention for the populist AfD party for these two treatments for both knowledge groups. AfD support drops from 29% to roughly 20% and 19% after exposure to the economy and inequality treatments among the less knowledgeable while support among the more knowledgeable stays the same or possibly even slightly increases.

Given the exploratory nature of the analysis on the moderating effect of knowledge, this study refrains from reporting p-values and tests of statistical significance for this part of the analysis. Not only do *post hoc* analyses not lend themselves for classical hypothesis testing, but we also lack any way of determining how to adjust for multiple hypothesis testing. As the heterogeneous treatment effects were not part of the pre-analysis plan, it is impossible to apply the previously discussed Bonferroni correction or, alternatively, simulation to determine the appropriate level of statistical significance in this case. This means that without further empirical testing, we should treat the evidence that is consistent with a moderating effect of political knowledge on populist voters' vote choice very cautiously.

As for the other treatments, evidence for moderation by political knowledge is even less

²⁸This small *increase* in support for the AfD among more knowledgeable voters is consistent with Chong and Druckman (2007) and Chong and Druckman (2010)'s finding that frames frequently backfire with motivated respondents and may lead to an effect that is the opposite of what was intended with the initial frame.

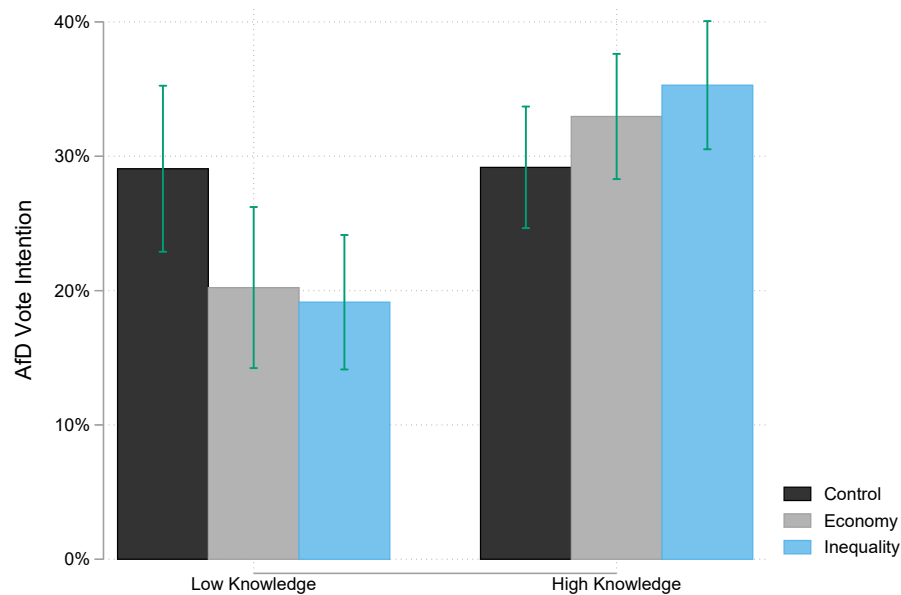
Table 4.3: 2017 Vote Intention by Level of Political Knowledge

	(1) AfD	(2) CDU	(3) SPD	(4) FDP/Greens
Economic Voting	-0.089 (0.044)	0.023 (0.035)	0.023 (0.038)	-0.059 (0.028)
Strategic Voting	-0.042 (0.041)	0.067 (0.034)	-0.037 (0.036)	-0.013 (0.032)
Uncertainty	-0.032 (0.045)	0.056 (0.037)	-0.0019 (0.038)	-0.033 (0.029)
Social Inequality	-0.099 (0.040)	0.0093 (0.032)	0.050 (0.039)	0.0082 (0.033)
Knowledge	0.0010 (0.039)	-0.0024 (0.034)	0.033 (0.032)	0.0056 (0.030)
Economy*Knowledge	0.13 (0.054)	0.057 (0.047)	-0.061 (0.045)	0.021 (0.038)
Strategy*Knowledge	0.075 (0.053)	-0.041 (0.045)	-0.0068 (0.044)	0.0100 (0.041)
Uncertainty*Knowledge	0.057 (0.053)	-0.027 (0.045)	-0.019 (0.045)	-0.0057 (0.038)
Inequality*Knowledge	0.16 (0.050)	-0.0065 (0.041)	-0.072 (0.045)	-0.029 (0.041)
Constant	0.20 (0.048)	0.016 (0.039)	0.048 (0.045)	0.087 (0.037)
Controls	Yes	Yes	Yes	Yes
Observations	1786	1786	1786	1786
R^2	0.46	0.41	0.42	0.27

Note: Dependent variable is vote intention in 2017 German federal election for the party indicated at the top of each column. 688 respondents are in the less knowledgeable group and 1098 in the more knowledgeable one. OLS regression with control variables: indicators for region (East vs. West), age group, gender, income group, education level, party identification, and most important topic (immigration, crime, terrorism). Models control for 122 of the 1,786 respondents who are less anti-immigrant (indicator and interaction with treatments).

Coefficients of primary interest for this study are in bold.

Figure 4.2: Share of Respondents Intending to Vote for the AfD, by Political Knowledge and Treatment Condition



clear. A cursory look at the coefficients in Table 4.3 seems to suggest that treatment effects for low knowledge respondents in the sample tend to be larger in absolute terms, but these effects are substantively smaller than for the two previously discussed treatments.²⁹

All in all, the results from Table 4.3 may suggest that further empirical tests of hypotheses H1a, H2a, H3a and H4a would be desirable. At least for the less knowledgeable voters in the sample, there is some suggestive evidence that both economic performance (H1a) and social inequality (H4a) framing may substantively reduce support for the populist AfD party, practically cutting support for this party by a third among those who are inclined to vote for it.

4.5 Conclusion

This study examines the power of framing effects in a context where we would expect them the least: among voters who are susceptible to populist appeals in a context where they are likely to have previously been heavily exposed to populist messages due to a real-world election

²⁹A possible exception is the economic voting message and CDU/CSU vote intention in column 2.

campaign. Evidence from a survey experiment suggests that this group of voters is hardly responsive to any of the messages that were tested. The main finding is that overall support for right-wing populist parties is unaffected by these messages. In other words, there is little evidence that would support hypotheses H1a, H2a, H3a and H4a.

There is only very limited evidence that supports the (b) variants of the hypotheses. As predicted by one of the theoretical logics that were presented above, being able to shift these voters' attention to performance-related issues – in particular, a well-performing economy – benefits the incumbent party and may help them win elections (Hypothesis H1b). However, contrary to what was expected, this increased support for the incumbent does not primarily come at the expense of the right-wing populist challenger, but instead seems to shift votes away from minor mainstream opposition parties toward the party of the incumbent chancellor. There is no empirical support for hypotheses H2b, H3b or H4b.

I hypothesize and find some evidence that suggests that these null-findings may partially be due to the moderating effect of political knowledge.³⁰ While the economic and the social inequality frames strongly reduce support for a populist party among the less politically knowledgeable, they have no effect – or possibly even backfire – on more knowledgeable voters.

In uncovering these effects, the study extends our understanding of the scope of framing effects to a group of voters that has hitherto received rather scant attention: voters who are susceptible to support (right-wing) populist parties. Overall, the ability of mainstream parties to use mainstream non-immigration messages to successfully lure these voters away from anti-immigrant right-wing parties is very limited. To the extent that it seems possible to potentially sway the vote of at least some of them and if we take into account the exploratory analyses on the moderating effect of political knowledge, it seems that there is not one single strategy that is clearly superior to all other framing strategies and that works with all potential populist voters. For mainstream parties, this means that they have to make trade-offs when selecting a specific campaign strategy, deciding to focus more on certain groups of populist voters than

³⁰This aspect of the theoretical framework is not part of the registered pre-analysis plan.

on others. While the results of this study suggest that economic performance messages may be the most promising electoral strategy, politicians trying to target specific groups of voters (e.g. potential populist voters with little political knowledge) might be better off highlighting their policy positions on issues such as economic redistribution. Thus, the choice of the ideal framing strategy for potential populist voters depends both on the specific goals of politicians and on the target audience of their messages. Preliminary evidence suggests that badly chosen strategies are not only ineffective, but potentially may even backfire and harm a party's electoral success.

A few caveats apply. The empirical test in this study represents a very conservative test of mainstream parties' ability to sway potential populist voters. Most respondents were surveyed less than two weeks before the 2017 German federal election, that is, at a time where they had already been extensively exposed to both populist and mainstream political messages. As a result, most of them are likely to have made up their mind about the upcoming election. Therefore, future research might test the effectiveness of mainstream non-immigration messages at an earlier point in time during or possibly even completely outside of a real-world election campaign, that is, at a time where vote intentions are not firmed up yet.

It is also worth pointing out that the null findings might partly be the result not so much of the substantive content of the messages that were used in this study, but rather of the way they were formulated. In designing the various mainstream non-immigration messages, priority was given to the realism of the frames by trying to mimic political messages that could plausibly have been used by mainstream parties in the 2017 German election. However, there might be a trade off between the greater realism of a frame and its persuasive strength, especially during the final stages of an election campaign. Thus, future research might consider broadening the scope of this study and putting more weight on the argumentative strength of the frames than on their realism.

Finally, it is worth emphasizing that two of the treatments – the economic voting and the social inequality messages – provide information both about the current state of the economy and about mainstream parties' policy positions. Any treatment effects that suggest that

populist voters may be swayed by mainstream messages were concentrated among these two informational frames. This study design does not allow to distinguish between information-based persuasion and pure emphasis framing. Yet, the fact that exploratory analyses suggested that both treatments had a disproportionate effect among the less knowledgeable respondents would be consistent with an account that highlights the crucial role of information-based persuasion. Populist voters who are least politically informed and thus, most difficult to reach by mainstream parties are the ones that are usually considered as the most staunch supporters of right-wing populist parties. Thus, future research might want to explore more fully the extent that information may play in shaping these citizens' vote choice.

Other questions remain for future research to consider as well. First, the findings from this study are based on findings from a multi-party context. This particular context means that dissatisfied voters can choose between smaller mainstream opposition parties and more extreme right-wing populist parties to express their dissatisfaction with the incumbent government. Given that practically all of the shift in votes that this study uncovered happened between mainstream opposition and mainstream incumbent parties, it is not clear to what extent economic performance messages (and, to a lesser extent, uncertainty messages) is an equally powerful strategy in a two-party context. Second, future research could also more thoroughly investigate the conditions under which some of the frames that are included in this study may backfire and produce counterproductive effects. This issue has only been tangentially touched upon in this study, but I find some evidence that suggests that especially among the more politically knowledgeable voters, treatments that might be perceived as too directly opposed to right-wing populist parties might produce backlash effects.

Chapter 5

Conclusion

This dissertation aimed at identifying electoral reforms and campaign strategies that may increase political participation and reduce support for populism among politically alienated citizens. To do so, it used econometric methods for causal inference – a regression discontinuity design in Study 1 and a difference-in-differences design in Study 2 – and survey experimentation in combination with large original datasets to test the effectiveness of government interventions and campaign messages. Improving on previous quantitative research in this area, this research highlights the limitations of institutional fixes and provides new insights into the role of information and political framing for civic engagement: Study 1 shows that informational ambiguity due to institutional complexity may confuse young voters and lead some of them to question their eligibility status, thus depressing voter turnout among this group of voters. Study 2 finds only limited support for the assumed positive relationship between voter turnout and support for left-wing parties. The mixed findings from Study 2 raise doubts about mandatory voting laws as an effective tool to address economic inequality in established democracies. Study 3 identifies mainstream economic messages as a potentially promising strategy for mainstream parties to win back moderately dissatisfied voters.

The findings from these studies have direct practical implications for electoral reform debates, political campaign strategies and policy making in advanced industrial democracies. For example, voter turnout is particularly low among young, and especially first-time, voters.

Some people attribute the blame for this primarily to young voters themselves and to their lack of interest in politics and elections. However, findings from Study 1 suggest that for some of them, it is not so much that they do not want to vote, but that the procedural information costs represent particularly high hurdles to them. They may be confused by the specificities of their state voter registration rules and as a result, erroneously think that they are ineligible to vote. Thus, a direct implication from Study 1 is that in order to increase voter turnout particularly among those who turn 18 shortly before Election Day, reforms should aim at making the act of registering to vote easier for these citizens. Some U.S. states have recently switched to Election Day registration where citizens can both register and vote at the same time on Election Day. A few U.S. states have followed Oregon and now automatically add their citizens to the voter roles. In line with the findings on procedural information costs, both of these reforms will significantly reduce the risk that first-time voters are misled by registration rules in these states. Thus, both reforms are likely to lead to higher voter turnout among this group of citizens.

Findings from Studies 2 and 3 have the potential to inform debates about the appropriate strategies of how to address concerns about increasing economic inequality and the rise of populist parties across advanced industrial democracies. To help tackle the former problem, some people – possibly most prominently former U.S. president Barack Obama – have mused about the benefits of making voting compulsory. The idea is that if poor people, who are generally less likely to vote, are compelled to cast a vote on Election Day, this will help left of the center political parties, potentially bringing them to power and as a consequence, leading to more redistributive policies. However, findings from Study 2 caution against putting too much hope into mandatory voting laws as a means to reduce economic inequality. While these laws definitely prove very effective in bringing poor voters to the polls and thus, help to address the problem of **political** inequality, the political consequences of more equal political participation on economic redistribution seem rather modest. Only a slightly more satisfying insight for mainstream politicians who are electorally challenged by right-wing populist parties comes from Study 3. Mainstream parties, especially those that are right of the center, are increasingly seen as trapped in a situation where they lose voters to anti-immigrant right-wing parties, but

refrain from adopting too anti-immigrant a political stance in order to avoid alienating more moderate core voters. In this situation, Study 3 primarily highlights the constraints that mainstream parties face in luring voters away from anti-immigrant populist parties. However, it also shows that by downplaying the immigration issue and focusing instead its efforts on the economy, an incumbent moderate right-wing party might be able to increase its net electoral support. This happens not so much through drawing voters away from the more populist alternative, but rather through winning back dissatisfied voters that they have previously lost to more moderate opposition parties.

A few caveats apply, however. While a major strength of these three studies is that their findings are based on three large and unique datasets, these datasets have their own limitations. For example, in an ideal world Study 1 would not have to rely on birth data as a proxy for the voting eligible population to calculate its youth voter turnout measure, but could directly use the number of voting eligible citizens by birthdate cohort. While using birth data does not lead to any bias in the treatment effect estimates – the difference in turnout between treated and untreated young citizens is robust across a variety of specifications –, one needs to be aware that the turnout rates in this study are likely to be somewhat different from those in the real world. Another shortcoming is not so much with the existing data, but rather with the data that we do not have. For example, making inferences based on the data from Australian elections in Study 2 is limited by the small number of Australian states and, within some of these states, the relatively small number of electoral districts. Given these data limitations and the mixed findings of this study, this research cannot claim to offer the final answer on the question whether higher voter turnout among the poor leads to higher support for left-wing parties. Yet, as a result of the fine-grained nature of the data and the research design, we have internally valid effect estimates that give us reason to question aspects of the received wisdom on this topic.

Several implications for future research directly flow from these findings. First, future research could use the insights from Study 2 to more thoroughly investigate the contextual factors that seem to shape the relationship between voter turnout and support for left-wing

parties. Preliminary findings from this dissertation suggest that geographic factors – especially whether an electoral district is urban or rural and its geographic size – are powerful factors in shaping both voter turnout among the poor and partisan support. Second, the finding in Study 3 that even potential populist voters are receptive to economic frames invites additional work on these people’s policy preferences and, above all, how they trade off different policy preferences if they are in conflict to one another.

Finally, to the extent that rule complexity is not limited to elections, but extends to many areas in which government agencies interact with citizens, the findings from Study 1 demand for a broader analysis of procedural information costs and their impact on program uptake. Many social welfare programs require that citizens actively enrol into them. Previous research largely attributes non-enrolment into such programs either to discrimination by bureaucrats or to a lack of interest among the poor. However, the findings from Study 1 suggest a possible alternative explanation. It could be that complex rules about program eligibility lead some of those who are most in need of these programs – the less educated and the socioeconomically less well off – to erroneously think they are ineligible and as a result, they fail to enrol. Given the widespread use of social welfare programs to alleviate extreme poverty and to help those who are most vulnerable in society, figuring out whether non-enrolment into beneficial government programs by poor people is an active choice or rather the result of unclear eligibility rules warrants further investigation.

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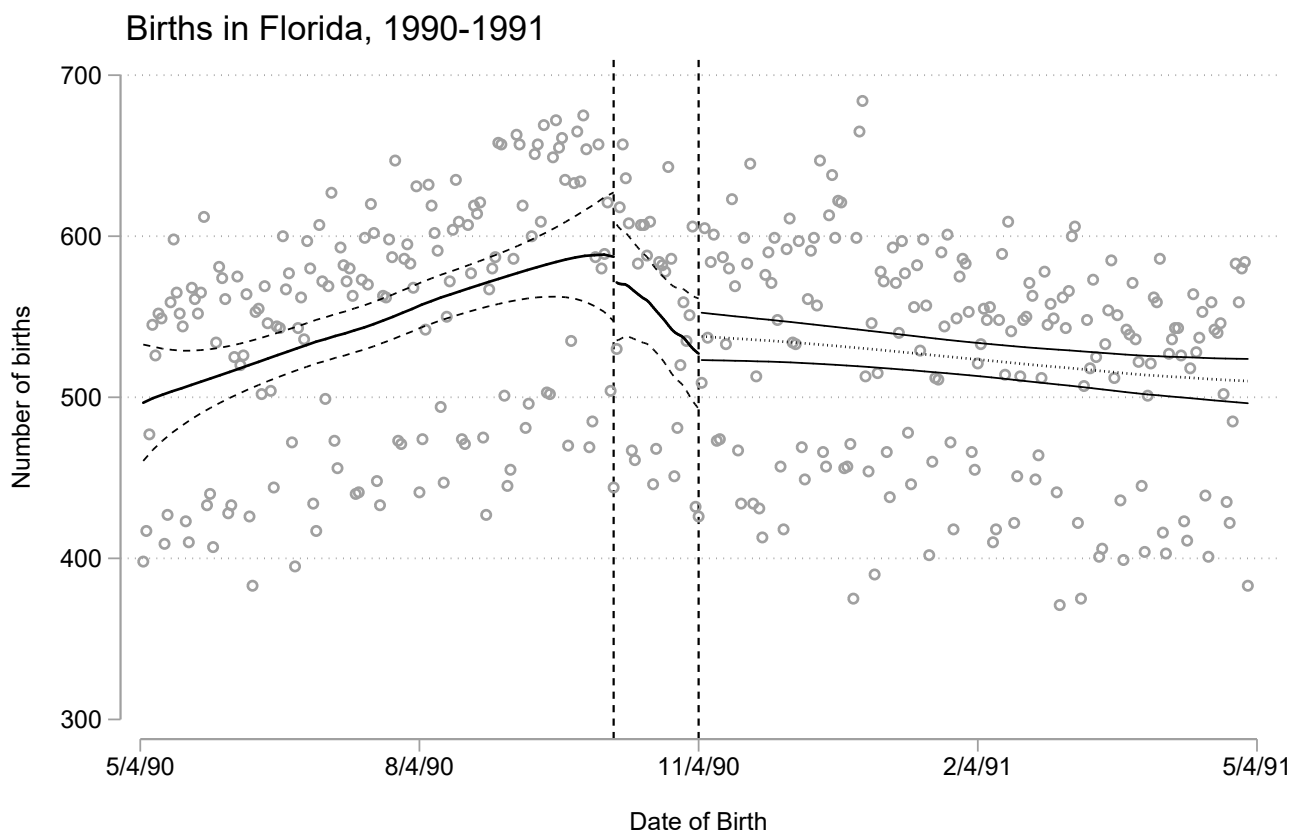
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Appendix A

Supporting Materials to Chapter 2

A.1 Smoothness of Density around Registration Deadline Cut-off

Figure A.1: Distribution of Births in Florida between May 5, 1990 and May 4, 1991



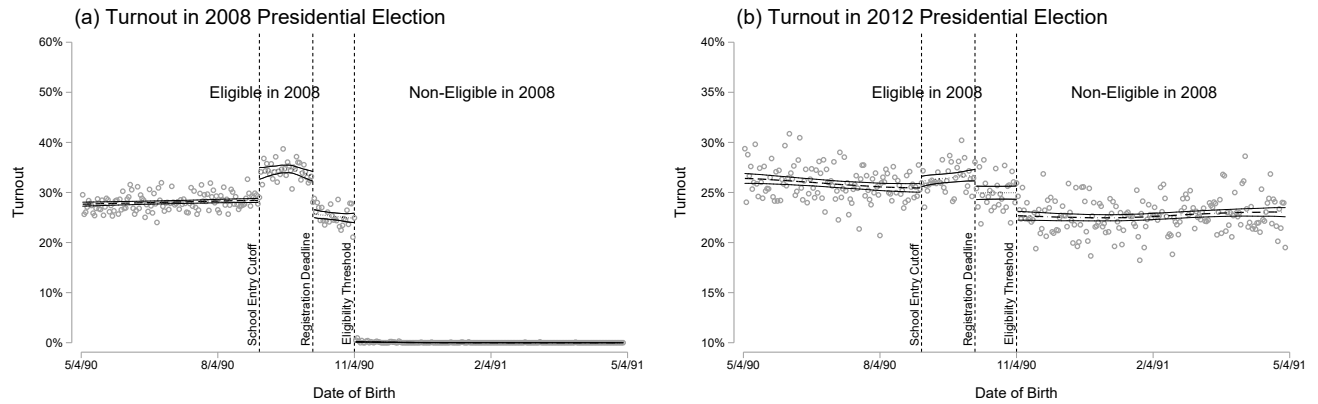
The vertical line to the left indicates the day and month of the registration deadline for the 2008 presidential election, the vertical line to the right the day and month of the Election Day eligibility cutoff for the 2008 election.

A.2 Registration Deadline Discontinuities in Texas

Discontinuities at the registration deadline are widespread across U.S. states. Figure A.2 plots turnout for the 2008 and 2012 U.S. presidential elections for young voters in Texas. As we can see in Figure A.2(a) for the 2008 presidential election, there clearly is a discontinuity around the registration deadline in Texas, with voter turnout being significantly lower in that election for those who turned 18 after the registration deadline. The analysis in Texas is somewhat complicated, however, by a third discontinuity around the school entry cutoff date of September 1. Young people to the left of this cutoff have started school a year earlier and thus, will usually just have finished high school shortly before the 2008 election. I expect that due to the changes in these people's lives (starting college or a job, leaving home) they are somewhat less likely to

vote than their peers who are slightly younger and still in high school at the time of the election. Figure A.2(b) shows that while there is a discontinuity in turnout for young Texans around the election day cutoff in the 2012 presidential election, we also have a small discontinuity around the registration deadline.

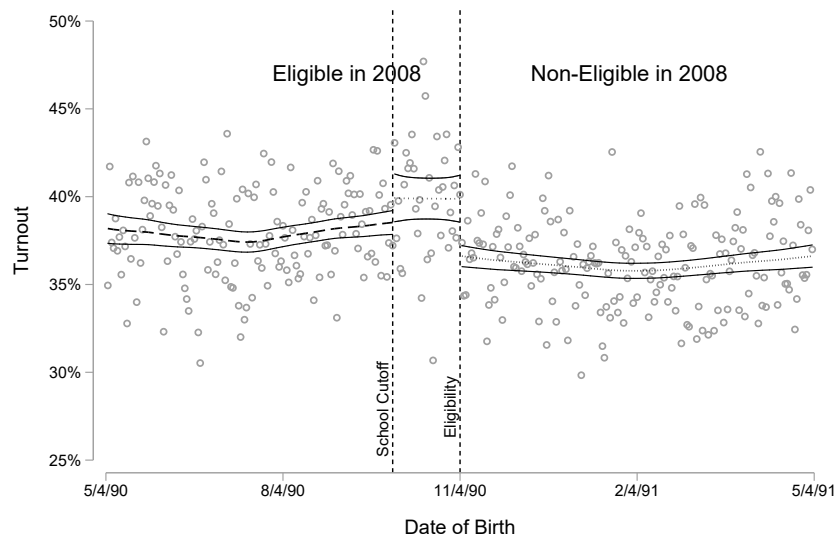
Figure A.2: Turnout in Presidential Elections in Texas



A.3 Separate Treatment Around School Entry Cutoff

In some states we can observe a slight drop in turnout to the left of the school entry cutoff dates of September 1 (Florida, Texas) or September 30 (Ohio). Given that those whose 18th birthday is shortly before this deadline will already have finished high school by the time of the 2008 election, they are more likely to have left their established social networks in order to start college or work. Having to vote during this short, but unstable time where young people transition from high school to the next stage in their lives seems to make them less likely to vote in their first few elections (Franklin 2004). As a result, we see a slight drop in turnout for these voters in both the 2008 and 2012 elections. Figure A.3 illustrates this for Ohio, Figure A.2 above does so for Texas. Pennsylvania did not specify a school entry cutoff date in its statutes in the early 1990s and New York's was after election day on December 1. Therefore youth turnout around the election day cutoff is unaffected by the school entry cutoff date in these two states.

Figure A.3: Turnout in 2012 Presidential Election in Ohio

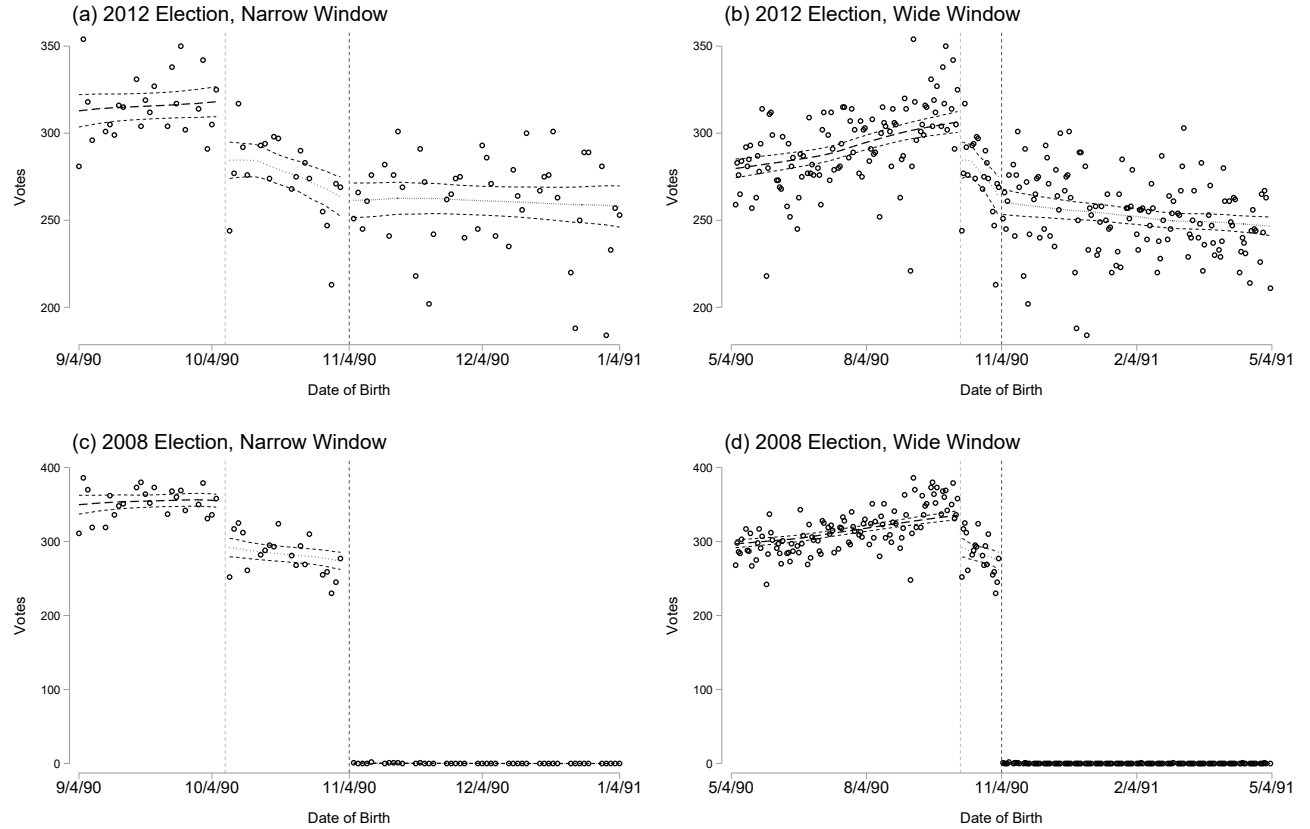


A.4 Discontinuity in Daily Vote Totals for Florida

Another way to test whether there is a discontinuity around the registration deadline cutoff is to plot the total number of votes by birthdate cohorts. This measure has the big advantage that it is not affected by migration in and out of a state. Thus, if I still find support for my claims about a discontinuity at the registration deadline, this will greatly increase the confidence in my findings. While this measure will be more non-linear than my turnout measure due to seasonal variation in the number of births and, as a consequence, in eligible voters, we would not expect to see any abrupt discontinuities in the total number of votes unless there is a treatment effect. Given that total births are much lower at weekends than for weekdays – something that makes the graphical inspection of discontinuities extremely difficult –, Figure [A.4](#) plots the votes by birthdate cohorts for the 2008 and 2012 U.S. presidential elections in Florida only for weekdays. Dropping all Saturdays and Sundays from these graphs also solves the problem that people directly to the left of both the registration deadline and election day cutoffs in 2008 were born on a weekend. Across all graphs, we can see the same pattern: there is a clear discontinuity in votes at the registration deadline cutoff, but there is no discontinuity at the election day cutoff.

Figure [A.5](#) is similar to Figure [A.4](#), but includes weekend days. In order to adjust for the much lower number of births and, as a consequence, of votes for these days, I multiply the number of votes for Saturdays and Sundays by a factor of 1.23 and 1.31 respectively, thus increasing the number of votes for these days to the average number of votes for weekdays. The overall pattern (i.e. a discontinuity around the registration deadline and no discontinuity at the election day cutoff) is unaffected by the inclusion of weekend days.

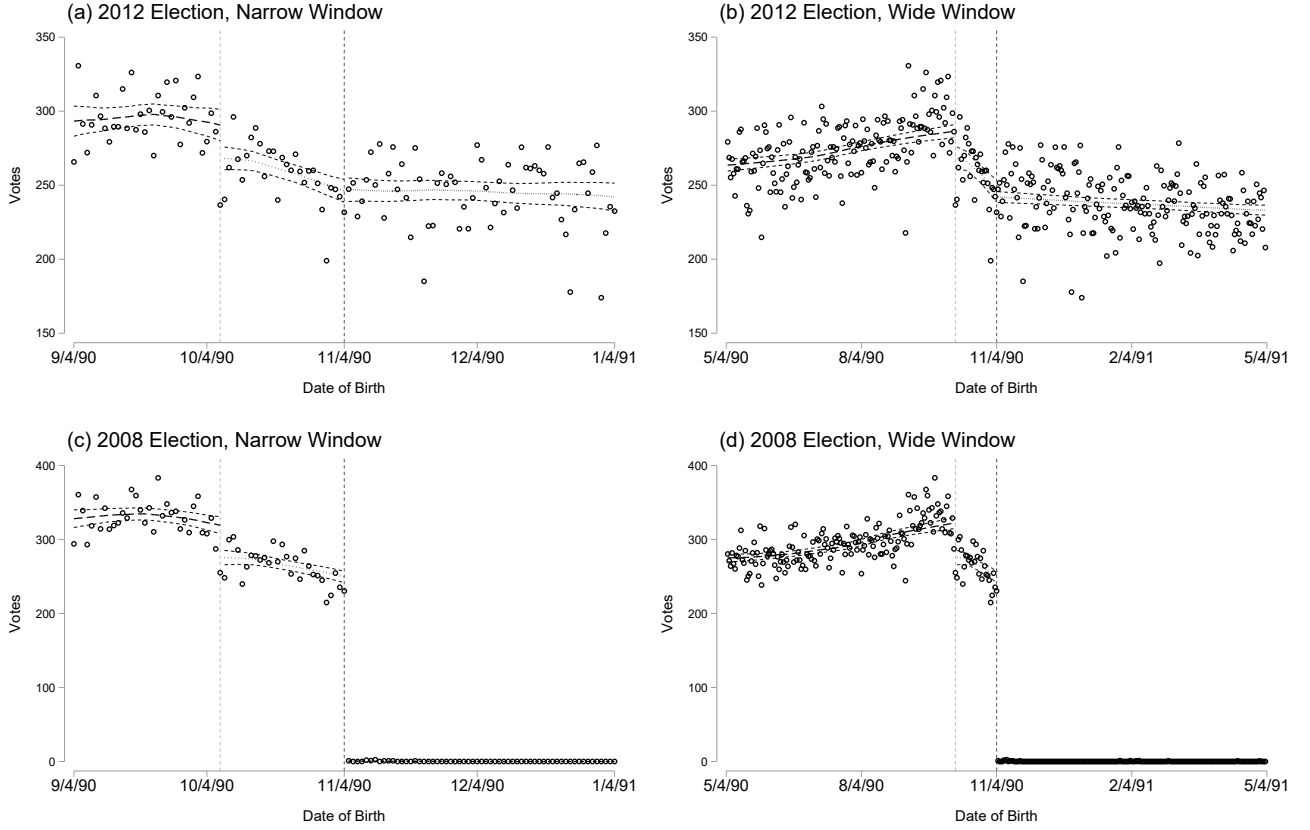
Figure A.4: Votes in 2008 and 2012 Presidential Elections in Florida - Excluding Weekends



A.5 Robustness of RD Estimates to Alternative Bandwidths

Table A.1 replicates the results from Table 2.1 by using a 10-day window on either side of the registration deadline cutoff instead of a 28-day window. Reducing the bandwidth to roughly a third of its original size produces results that are largely similar to those presented in Table 2.1 above, further bolstering our confidence in the robustness of these results.

Figure A.5: Votes in 2008 and 2012 Presidential Elections in Florida - Including Weekends



A.6 Robustness of RD Estimates to Alternative Model Specifications

To make sure that the main results from Table 2.1 above are not a product of the estimation strategy that is used in the main part of this article, I re-estimated the difference in turnout between preregistration eligibles and postregistration eligibles by using the following standard OLS model:

$$Y_j = \beta_0 + \beta_1 T_j + \beta_2 D_j + \beta_3 T * D_j + \epsilon_j \quad (\text{A.1})$$

Y_j is turnout measured as the number of people who voted over the number of people born on a given day, T_j is an indicator for whether people turn 18 after the registration deadline or

Table A.1: Turnout in U.S. Presidential Elections for 1990 Birth Cohorts

	(FL)	(TX)	(NY)	(OH)	(PA)
Registration Deadline Discontinuity					
2008					
Eligibles post-deadline	45.67	25.95	30.25	47.64	40.78
Eligibles pre-deadline	51.77	33.22	30.60	48.72	44.45
Difference	-6.10	-7.28	-0.34	-1.08	-3.67
	[-7.94, -4.27]	[-8.61, -5.96]	[-1.76, 1.07]	[-3.14, 0.99]	[-5.66, -1.68]
N	11,364	18,136	16,347	8,990	9,443
2012					
Eligibles post-deadline	45.86	24.96	30.55	41.47	36.83
Eligibles pre-deadline	49.00	26.79	28.85	38.81	37.21
Difference	-3.14	-1.82	2.66	1.50	-0.38
	[-4.97, -1.30]	[-3.10, -0.55]	[0.30, 3.10]	[0.63, 4.69]	[-2.33, 1.57]
N	11,364	18,136	16,347	8,990	9,443
2016					
Eligibles post-deadline	44.68				
Eligibles pre-deadline	47.53				
Difference	-2.85				
	[-4.68, -1.02]				
N	11,364				

Note: Table presents difference-in-proportions with 95% confidence intervals in brackets for young Americans born in 1990. Each estimation uses a 10-day-window on either side of the registration deadline in 2008.

not, D_j is the running variable indicating the distance (in days) from the registration deadline, and j indexes each birthday cohort. The interaction term $T * D_j$ allows for different slopes on either side of the registration deadline. Like the analysis in the main part of the text, I use a 28-day window on either side of the cutoff. Note that a major advantage of using an OLS model is that, assuming the model is correctly specified, it gives us an estimate of the treatment effect right at the cutoff point. This means that if the results from the OLS model are similar to the findings from the difference-in-proportions, this bolsters our confidence in our estimates and in the choice of a 28-day window on either side of the registration deadline cutoff.

Table A.2: Turnout in U.S. Presidential Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	FL	TX	NY	OH	PA	FL	TX	NY	OH	PA	FL
	2008	2008	2008	2008	2008	2012	2012	2012	2012	2012	2016
Registration Beliefs	-0.044* (0.016)	-0.066* (0.011)	0.0065 (0.011)	-0.011 (0.015)	-0.042* (0.016)	-0.027 (0.014)	-0.020* (0.008)	0.022* (0.009)	0.016 (0.016)	-0.0018 (0.015)	-0.029* (0.014)
Days	-0.043 (0.130)	-0.16 (0.085)	-0.20* (0.071)	0.21 (0.124)	0.15 (0.125)	0.13 (0.114)	0.034 (0.068)	-0.15* (0.060)	0.053 (0.130)	0.00055 (0.121)	0.19 (0.109)
Beliefs*Days	-0.22 (0.185)	0.020 (0.120)	0.012 (0.121)	-0.30 (0.176)	-0.20 (0.177)	-0.27 (0.161)	-0.025 (0.096)	-0.026 (0.102)	-0.14 (0.183)	-0.082 (0.171)	-0.35* (0.155)
Constant	0.52* (0.012)	0.33* (0.008)	0.30* (0.006)	0.49* (0.011)	0.44* (0.011)	0.49* (0.010)	0.27* (0.006)	0.29* (0.005)	0.39* (0.012)	0.37* (0.011)	0.48* (0.010)
Observations	56	56	56	56	56	56	56	56	56	56	56
R^2	0.58	0.84	0.38	0.06	0.29	0.24	0.23	0.19	0.06	0.03	0.27

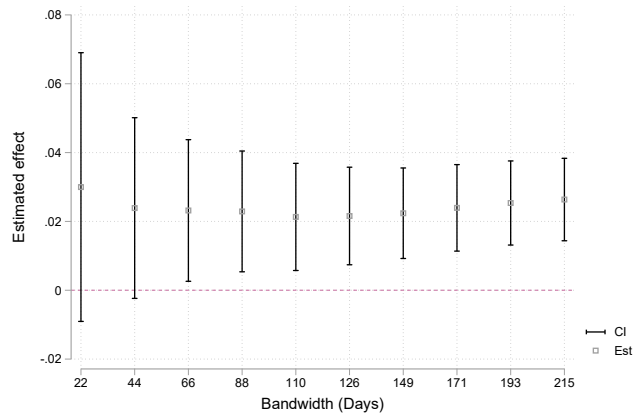
Table displays OLS estimates with a 28-day window (25 for NY) on either side of the cutoff. Standard errors in parentheses. * $p < 0.05$

Across the different models, the pattern is largely similar to the results from difference-in-proportions. For the 2008 elections, effect sizes are still large and statistically significant for Florida, Texas and Pennsylvania. However, we do no longer find an effect of registration beliefs for New York. Based on the OLS model, the effect size for 2008 is now somewhat smaller for both Florida and Texas. Registration beliefs decrease turnout for those who turn 18 right after the registration deadline in 2008 by 4.4 percentage points for Florida and by 6.6 percentage points for Texas. Note that the OLS estimates are smaller than the results from both the difference-in-proportions with the wide 28-day and the narrow 10-day window on either side of the registration deadline, potentially pointing to difficulties in appropriately estimating voter turnout with OLS if the running variable (i.e. birthdates) is discrete and the estimation window is narrow (Nyhan, Skovron and Titiunik 2017, 751). The effect of registration beliefs for the same group of voters in both Florida and Texas for the 2012 election are very similar in size to the estimates in the main text from difference-in-proportions.

A.7 Robustness of Persistence of Effect to Varying Bandwidths

In this section I show that the discontinuity in turnout that we still find four years later between those who turn 18 before and those who turn 18 after the registration deadline is robust to varying bandwidth specifications around the RD cutoff for Florida. Figure A.6 plots the size of the discontinuity at the registration deadline for 22 year-olds in the 2012 presidential election for different bandwidths. We can see that the size of the difference in turnout between these two groups is largely unaffected by the choice of the bandwidth.

Figure A.6: Varying Bandwidths Around Registration Deadline Cutoff



Note: Effect of being born before the registration deadline of October 7, 2008 on 2012 voter turnout in Florida.

A.8 Information on Registration Requirements Across U.S. States

While every U.S. state with voter registration allows people under 18 to register if they will be 18 by Election Day, the precise age that is required to register varies across states. In some states, young people can register as soon as they turn 16 or 17, in others, they have to wait until they are 17 years and 10 months old, and for many states, people are eligible to register if they will be 18 by the next general election. However, the precise age requirement in a state seems less important than the clarity by which these rules are conveyed. While only experimental evidence can test the causal effect of the clarity of information on a person's decision to register and vote in an election, the clarity with which this age requirement is conveyed in the election code or on registration forms may serve as a proxy for the degree to which voters may be uncertain about the exact age requirement to register in a given state. Among states analyzed in more detail in this article, some election codes and/or their registration forms (Florida, Oklahoma, Texas) are somewhat less clear about the exact age requirement to register in that state, suggesting that in these states there might potentially exist more uncertainty among voters about whether they have to be 18 by the registration deadline or by Election Day. The election codes in these states read as follows (emphasis added):

Florida: 97.041 Qualifications to register or vote. “(1)(a) A person may become a **registered voter** only if that person: 1. Is at least **18 years of age**; 2. Is a citizen of the United States; 3. Is a legal resident of the State of Florida; 4. Is a legal resident of the county in which that person seeks to be registered; and 5. Registers pursuant to the Florida Election Code. (b) A person who is otherwise qualified may preregister on or after that person's 16th birthday and may vote in any election occurring on or after that person's 18th birthday.”

Oklahoma: 26.A1.VI.Section 4-101 - Persons Entitled to Become Registered Voters “Every person who is a **qualified elector** as defined by Section 1 of Article III of the Oklahoma Constitution shall be entitled to become a registered voter in the precinct of his residence”

Oklahoma Constitution, Article 3, paragraph 1 “Subject to such exceptions as the Legislature may prescribe, all citizens of the United States, **over the age of eighteen (18) years**, who are bona fide residents of this state, **are qualified electors** of this state.”

Texas: Sec. 13.001. ELIGIBILITY FOR REGISTRATION. “(a) To be **eligible for registration** as a voter in this state, a person must: (1) **be 18 years of age or older**; (2) be a United States citizen; (3) not have been determined by a final judgment of a court exercising probate jurisdiction to be: (A) totally mentally incapacitated; or (B) partially mentally incapacitated without the right to vote; (4) not have been finally convicted of a felony or, if so convicted, must have: (A) fully discharged the person’s sentence, including any term of incarceration, parole, or supervision, or completed a period of probation ordered by any court; or (B) been pardoned or otherwise released from the resulting disability to vote; and (5) be a resident of the county in which application for registration is made. (b) To be eligible to apply for registration, a person must, on the date the registration application is submitted to the registrar, be at least 17 years and 10 months of age and satisfy the requirements of Subsection (a) except for age.”

Table A.3: Voter Registration Information Across Seven U.S. States

Potentially Ambiguous Registration Information:

Florida: “To register in Florida, you must be a U.S. citizen, a Florida resident, **at least 18 years old** (you may pre-register at 16 or 17, but you cannot vote until you are 18).”

Texas: “To be eligible for registration as a voter in this state, a person must be **18 years of age*** or older; be a United States citizen; not have been determined by a final judgment of a court exercising probate jurisdiction to be [...]” (Harris County)

Oklahoma: “Who can register – You **can register to vote** if you are both a citizen of the United States and a resident of the State of Oklahoma, and **18 years old or older.**”

Idaho: “To register to vote in Idaho you must:

be a U.S. Citizen,

have resided in Idaho and in the county for thirty (30) days prior to the day of election, and

be at least 18 years old.

Clear Registration Information:

New York: “To register you must [...] be 18 years old by the end of this year; [...]”

Ohio: “You are qualified to register to vote in Ohio if [...] you will be at least 18 years old on or before the day of the general election [...]”

Pennsylvania: “I declare that [...] I will be at least 18 years old on the day of the next election. [...]”

Sources: Voter registration forms, websites of Departments of State or of County Commissioners for Election.

Emphasis added.

* In a footnote on the Harris County commissioner of elections’ website and in apparent contradiction to the above statement, it is stated that people who are 17 years and 10 months old are eligible to register in this county.

A.9 Voter Registration Form from Florida

The election code and voter registration forms in Florida state that citizens have to be 18 years old in order to *register* in this state (see Figure A.7). While the election code and registration forms mention the possibility to preregister, they fail to explain how preregistration works or what precise steps citizens have to take to preregister. In personal communication with the Secretary of State’s office and several election commissioner of election offices in Florida on December 12, 2018, they confirmed to me that to preregister in Florida, a person simply needs to submit their voter registration form before turning 18. No other steps are required and no specific forms for voter preregistration exist.

Figure A.7: Voter Registration Form from Florida

Florida Voter Registration Application Part 1 – Instructions (DS-DE 39, R1S-2.040, F.A.C.)(eff. 10/2013)		Información en español: Sírvase llamar a la oficina del supervisor de elecciones de su condado si le interesa obtener este formulario en español.	
To Register in Florida, you must be: <ul style="list-style-type: none"> • a U.S. citizen, • a Florida resident, • at least 18 years old (you may pre-register at 16 or 17, but cannot vote until you are 18). <p>If you have been convicted of a felony, or if a court has found you to be mentally incapacitated as to your right to vote, you cannot register until your right to vote is restored.</p> <p>If you do not meet any ONE of these requirements, you are not eligible to register.</p> <p style="text-align: center;">Questions?</p> <p>Contact the Supervisor of Elections in your county: dos.myflorida.com/elections/contacts/supervisor-of-elections Visit the Florida Division of Elections' website at: dos.myflorida.com/elections</p> <p>CRIMINAL OFFENSE: It is a 3rd degree felony to submit false information. Maximum penalties are \$5,000 and/or 5 years in prison.</p> <p>PUBLIC RECORD: Once filed, all information including your phone number and email address as provided become public record except for the following which can only be used for voter registration purposes: your FL DL#, FL ID#, SSN, where you registered to vote, and whether you declined to register or to update your voter registration record at a voter registration agency. Your signature can be viewed but not copied. (Section 97.0585, Fla. Stat.)</p>		Where to Register: You can register to vote by completing this application and delivering it in person or by mail to any supervisor of elections' office, office that issues driver's licenses, or voter registration agency (public assistance office, center for independent living, office serving persons with disabilities, public library, or armed forces recruitment office) or the Division of Elections. Mailing addresses are on page 2 of this form. Deadline to Register: The deadline to register to vote is 29 days before any election. You can update your registration record at any time, but for a Primary Election, party changes must be completed 29 days before that election. You will be contacted if your new application is incomplete, denied or a duplicate of an existing registration. Your Voter Information Card will be mailed to you once you are registered. Identification (ID) Requirements: New applicants must provide a current and valid Florida driver's license number (FL DL#) or Florida identification card number (FL ID#). If you do not have a FL DL# or FL ID#, then you must provide the last four digits of your Social Security number (SSN). If you do not have any of these numbers, check "None." If you leave the field and box blank, your new registration may be denied. See section 97.053(6), Fla. Stat. Special ID requirements: If you are registering by mail, have never voted in Florida, and have never been issued one of the ID numbers above, include one of the following with your application, or at a later time before you vote: 1) A <u>copy</u> of an ID that shows your name and photo (<i>acceptable IDs</i> —U.S. Passport, debit or credit card, military ID, student ID, retirement center ID, neighborhood association ID, or public assistance ID); or 2) A <u>copy</u> of an ID that shows your name and current residence address (<i>acceptable documents</i> —utility bill, bank statement, government check, paycheck, or other government document). The special ID is not required if you are 65 or older, have a temporary or permanent physical disability, are a member of the active uniformed services or merchant marine who is absent from the county for active duty, or a spouse or dependent thereof, or are currently living outside the U.S. but otherwise eligible to vote in Florida. Political Party Affiliation: Florida is a closed primary election state. In primary elections, registered voters can only vote for their registered party's candidates in a partisan race on the ballot. In a primary election, all registered voters, regardless of party affiliation, can vote on any issue, nonpartisan race, and race where a candidate faces no opposition in the General Election. If you do not indicate your party affiliation, you will be registered with no party affiliation. For a list of political parties, visit the Division of Elections' website at: dos.myflorida.com/elections . Race/Ethnicity: It is optional to list your race or ethnicity. Boxes: Please check boxes () where applicable.	
Numbered rows 1 through 7 and 12 must be completed for a new registration.			
Florida Voter Registration Application Part 2 – Form (DS-DE #39, R1S-2.040, F.A.C.)(eff. 10/2013)		The downloadable/printable online form is available at: dos.myflorida.com/elections/for-voters/voter-registration	
This is: <input type="checkbox"/> New Registration <input type="checkbox"/> Record Update/Change (e.g., Address, Party Affiliation, Name, Signature) <input type="checkbox"/> Request to Replace Voter Information Card			
1	Are you a citizen of the United States of America? <input type="checkbox"/> YES <input type="checkbox"/> NO	OFFICIAL USE ONLY	
2	<input type="checkbox"/> I affirm that I am not a convicted felon, or if I am, my right to vote has been restored.		
3	<input type="checkbox"/> I affirm that I have not been adjudicated mentally incapacitated with respect to voting or, if I have, my right to vote has been restored.		
4	Date of Birth (MM-DD-YYYY) / / - - - 	FVRS No:	
5	Florida Driver License (FL DL) or Florida identification (FL ID) Card Number - - - - 	If no FL DL or FL ID, then provide	Last 4 digits of Social Security Number
6	Last Name	First Name	Middle Name
7	Address Where You Live (legal residence-no P.O. Box)	Apt/Lot/Unit	City
8	Mailing Address (if different from above address)	Apt/Lot/Unit	City
9	Address Where You Were Last Registered to Vote	Apt/Lot/Unit	City
10	Former Name (if name is changed)	Gender <input type="checkbox"/> M <input type="checkbox"/> F	State or Country of Birth
11	<input type="checkbox"/> Email me SAMPLE BALLOTS if option is available in my county. (See <i>Public Record Notice</i> above) My email address is:		
12	Party Affiliation <i>(Check only one. If left blank, you will be registered without party affiliation)</i> <input type="checkbox"/> Florida Democratic Party <input type="checkbox"/> Republican Party of Florida <input type="checkbox"/> No party affiliation <input type="checkbox"/> Minor party (print party name):	Race/Ethnicity <i>(Check only one)</i> <input type="checkbox"/> American Indian/Alaskan Native <input type="checkbox"/> Asian/Pacific Islander <input type="checkbox"/> Black, <i>not of</i> Hispanic Origin <input type="checkbox"/> Hispanic <input type="checkbox"/> White, <i>not of</i> Hispanic Origin <input type="checkbox"/> Multi-racial <input type="checkbox"/> Other:	<i>(Check only one if applicable)</i> <input type="checkbox"/> I am an active duty Uniformed Services or Merchant Marine member <input type="checkbox"/> I am a spouse or a dependent of an active duty uniformed services or merchant marine member <input type="checkbox"/> I am a U.S. citizen residing outside the U.S.
Oath: I do solemnly swear (or affirm) that I will protect and defend the Constitution of the United States and the Constitution of the State of Florida, that I am qualified to register as an elector under the Constitution and laws of the State of Florida, and that all information provided in this application is true.		SIGN/ MARK HERE 	<input type="checkbox"/> I will need assistance with voting. <input type="checkbox"/> I am interested in becoming a poll worker.
			Date

Appendix B

Supporting Materials to Chapter 3

B.1 Within-State Analysis of Treatment Effects

Table B.1: Effects of Compulsory Voting Using Within-State Variation in Treatment Intensity
- Non-Labor Candidates

	Δ Vote Share of Non-Labor Coalition Parties						
	NSW	QLD	SA	TAS	VIC	WA	All States
	1927- 1930	1912- 1915	1941- 1944	1928- 1931	1924- 1927	1936- 1939	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Turnout	-.405 (.357)	.460 (.365)	.120 (.251)	-.149 (.372)	-.509 (.616)	.626 (.484)	.195 (.136)
N	77	61	24	5	25	29	221
R^2	.020	.058	.007	.004	.026	.060	.017

Note: Table presents difference-in-differences estimates of treatment intensity (change in turnout as a result of compulsory voting) on change in ALP vote share. Robust standard errors in parentheses. Commonw.: Commonwealth; Elect.: Elections.

Table B.2: Effects of Compulsory Voting Using Within-State Variation in Treatment Intensity
- Independent Candidates

	Δ Vote Share of Independent Candidates						
	NSW	QLD	SA	TAS	VIC	WA	All States
	1927- 1930	1912- 1915	1941- 1944	1928- 1931	1924- 1927	1936- 1939	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Turnout	-.144 (.470)	-.298 (.534)	.070 (.525)	2.637 (.471)	.219 (.535)	-.113 (.511)	-.098 (.133)
N	77	61	24	5	25	29	221
R^2	.002	.028	.001	.750	.007	.002	.003

Note: Table presents difference-in-differences estimates of treatment intensity (change in turnout as a result of compulsory voting) on change in ALP vote share. Robust standard errors in parentheses. Commonw.: Commonwealth; Elect.: Elections.

B.2 Within-State Analysis of Changes in Turnout and Labor Seat Share

Figure B.1: Change in Turnout and ALP Vote Share in Queensland, 1912-1915

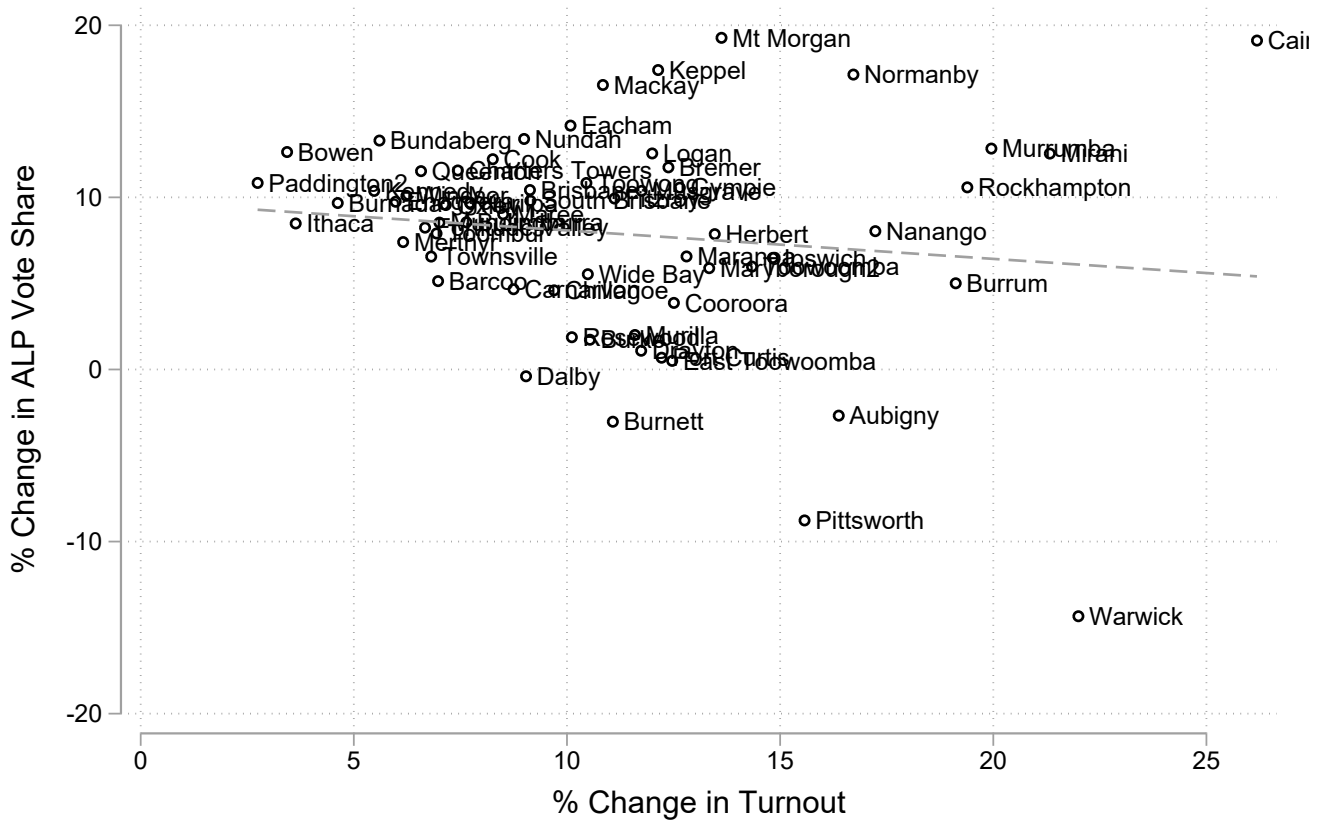


Figure B.2: Change in Turnout and ALP Vote Share in South Australia, 1941-1944

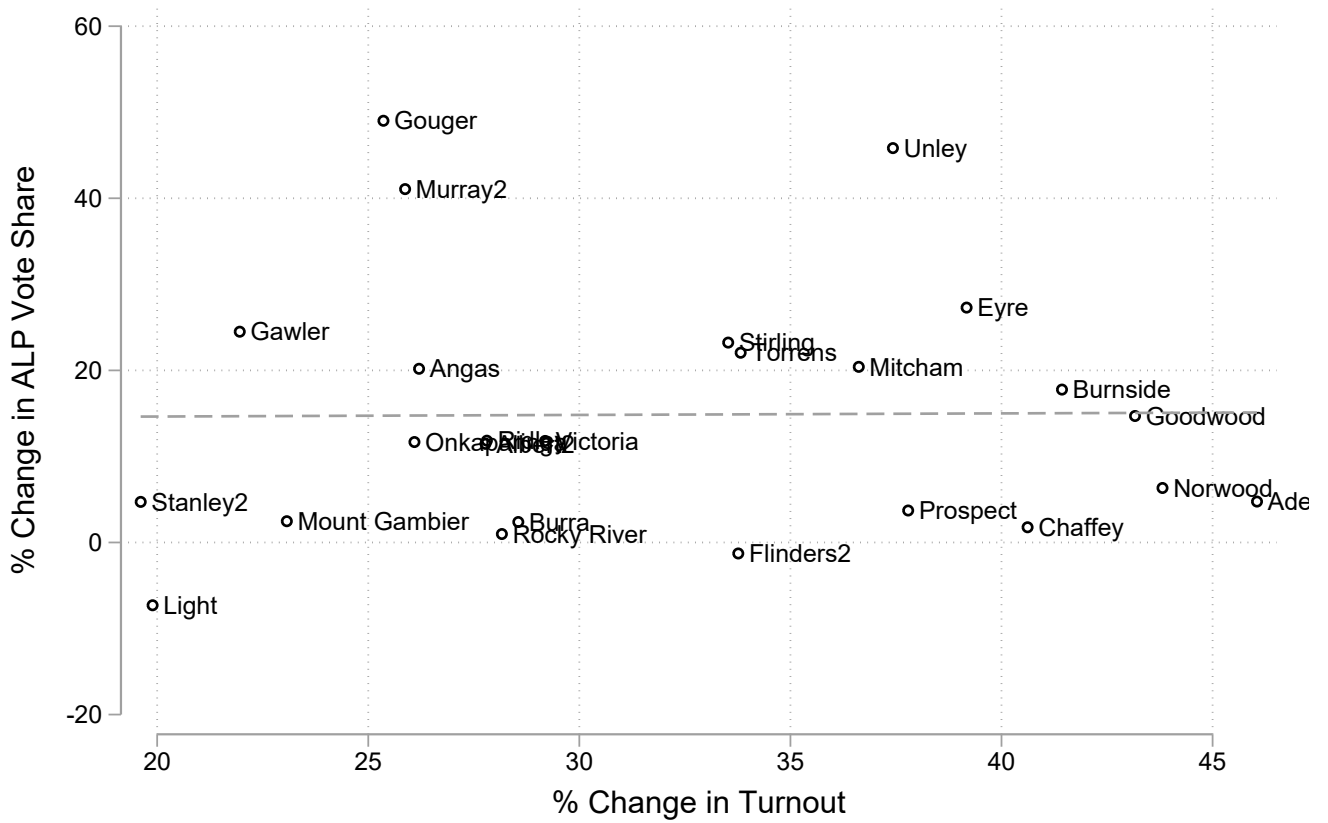


Figure B.3: Change in Turnout and ALP Vote Share in Victoria, 1924-1927

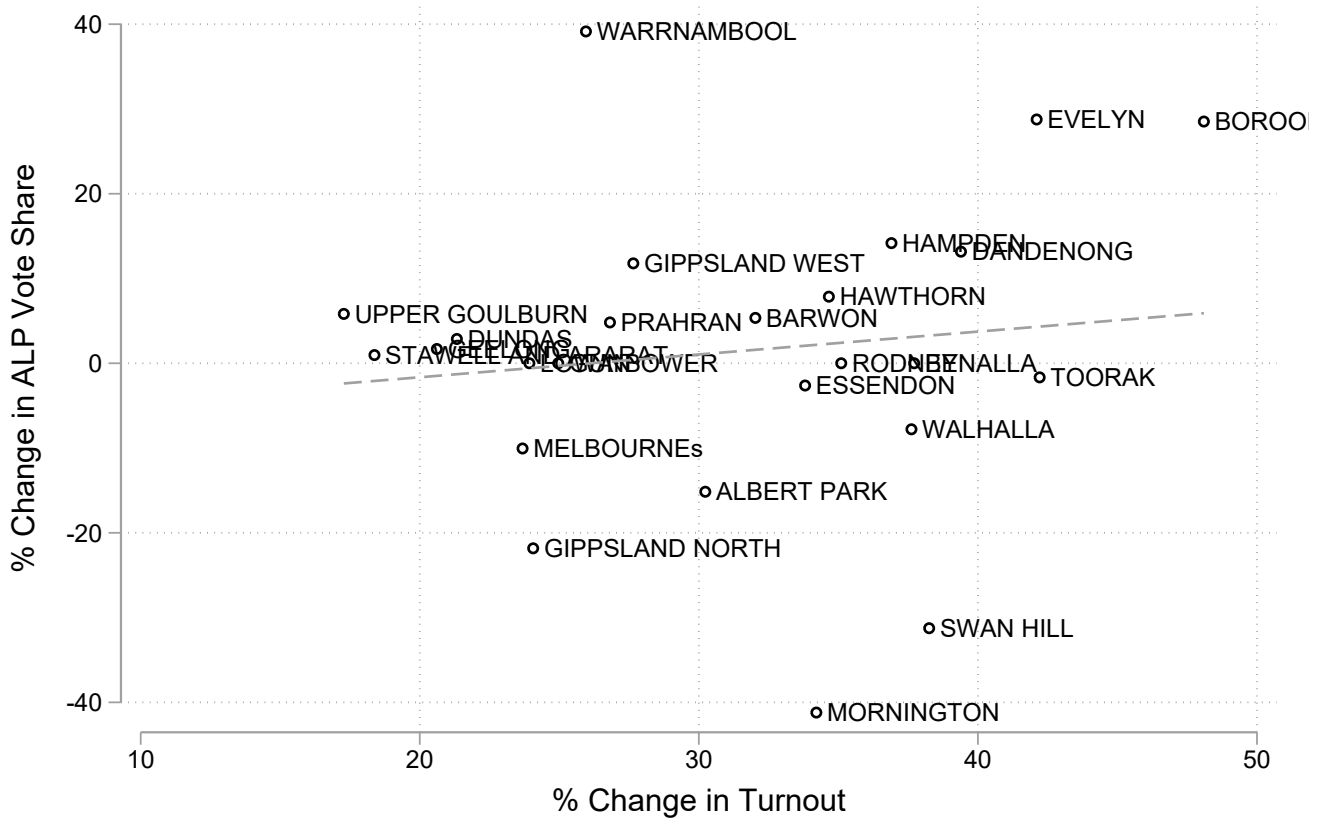
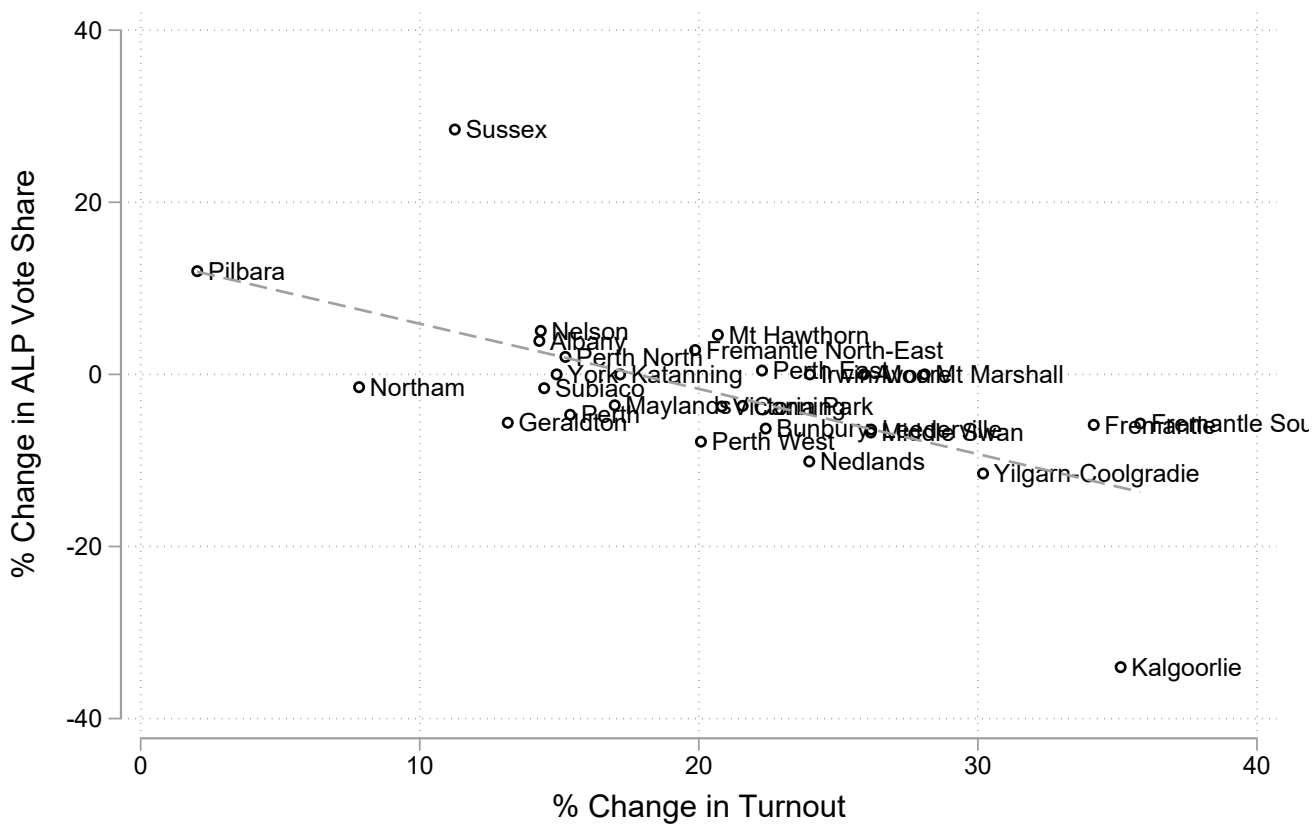


Figure B.4: Change in Turnout and ALP Vote Share in Western Australia, 1936-1939



Appendix C

Supporting Materials to Chapter 4

C.1 Question Wording of Treatments (English Translation)

A. [Control:] Which party would you vote for if there was a federal election this Sunday?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, other party, don't know/no answer (DK/NA)

B. [Treatment 1 (economic voting):] The German economy is doing very well, with unemployment at a historic low. The upcoming election will shape Germany's economic prospects for the next years. Which party would you vote for if there was a federal election this Sunday?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, other party, DK/NA

C. [Treatment 2 (strategic voting):] The CDU/CSU is very likely to win the federal election and form the new government. Whoever wants political change has only one option: to make the FDP or the Greens as strong as possible in order to avoid another four years of a Grand Coalition. Which party would you vote for if there was a federal election this Sunday?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, other party, DK/NA

D. [Treatment 3 (uncertainty/leadership characteristics):] These are uncertain times and the

world order is unstable. In times like these many think it is important for Germany to have an experienced leader. Which party would you vote for if there was a federal election this Sunday?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, other party, DK/NA

E. [Treatment 4 (social inequality):] In the upcoming election the SPD wants to make Germany fairer, provide more support to people who are struggling, and have the rich pay more in taxes. The CDU/CSU and the FDP oppose those changes. Thus, the outcome of the election is likely to have a direct impact on the extent of social inequality in Germany. Which party would you vote for if there was a federal election this Sunday?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, other party, DK/NA

C.2 Question Wording of Treatments in Survey (German Original)

A. [Control:] Welche Partei wuerden Sie waehlen, wenn am kommenden Sonntag Bundestagswahl waere?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, Sonstige, weiss nicht/keine Angabe

B. [Treatment 1 (economic voting):] Der deutschen Wirtschaft geht es momentan sehr gut und die Arbeitslosigkeit ist auf einem historischen Tiefstand. Der Ausgang der Bundestagswahl wird Deutschlands wirtschaftliche Zukunft fuer die naechsten Jahre entscheidend mitpraegen. Welche Partei wuerden Sie waehlen, wenn am kommenden Sonntag Bundestagswahl waere?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, Sonstige, weiss nicht/keine Angabe

C. [Treatment 2 (strategic voting):] Die CDU/CSU wird sehr wahrscheinlich als staerkste Partei aus der Bundestagswahl hervorgehen und die neue Regierung bilden. Wer einen politischen Wechsel moechte, hat im Prinzip nur eine Option: die FDP bzw. die Gruenen so stark

wie moeglich zu machen, um weitere vier Jahre einer Groen Koalition zu verhindern. Welche Partei wuerden Sie waehlen, wenn am kommenden Sonntag Bundestagswahl waere?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, Sonstige, weiss nicht/keine Angabe

D. [Treatment 3 (uncertainty/leadership characteristics):] Wir leben in unsicheren Zeiten und die Weltordnung ist instabil. In Zeiten wie diesen ist vielen wichtig, dass Deutschland von einer Person mit weltpolitischer Erfahrung regiert wird. Welche Partei wuerden Sie waehlen, wenn am kommenden Sonntag Bundestagswahl waere?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, Sonstige, weiss nicht/keine Angabe

E. [Treatment 4 (social inequality):] Die SPD verspricht, Deutschland gerechter zu machen, Beduerftige staerker zu unterstuetzen und die Reichen staerker zur Kasse zu bitten. Die CDU/CSU und die FDP lehnen diese Aenderungen ab. Der Ausgang der Bundestagswahl wird also direkten Einfluss auf das Ausmass sozialer Ungleichheit in Deutschland haben. Welche Partei wuerden Sie waehlen, wenn am kommenden Sonntag Bundestagswahl waere?

CDU/CSU, SPD, Gruene, FDP, Die Linke, AfD, Sonstige, weiss nicht/keine Angabe

C.3 Summary Statistics

	Obs	Mean	St. Dev.	Min	Max
female	1786	0.52	0.50	0	1
East	1786	0.24	0.42	0	1
educRS	1786	0.46	0.50	0	1
educGY	1786	0.33	0.47	0	1
income1500_2499	1786	0.25	0.44	0	1
income2500_3999	1786	0.23	0.42	0	1
income4000pl	1786	0.10	0.30	0	1
incomemiss	1786	0.16	0.37	0	1
unionmbr	1786	0.12	0.32	0	1
age18_29	1786	0.09	0.29	0	1
age30_44	1786	0.22	0.41	0	1
age45_59	1786	0.39	0.49	0	1
age60pl	1786	0.30	0.46	0	1
mit_imm	1786	0.57	0.49	0	1
mit_terr	1786	0.13	0.33	0	1
mit_crime	1786	0.05	0.22	0	1
pid_afd	1786	0.21	0.41	0	1
pid_cdu	1786	0.18	0.38	0	1
pid_spd	1786	0.14	0.35	0	1
pid_fdpgr	1786	0.07	0.25	0	1
pid_no	1786	0.30	0.46	0	1

C.4 Results with Alternative Dependent Variable: Party Favorability

The following table reports results with the alternative dependent variable that records respondents' favorability ratings towards each of the different parties. Favorability is measured on an 11 point scale, with 0 meaning that a respondent does not like a party at all and 10 that she likes it a lot. In contrast to the vote intention question, the party favorability was not asked at the same time (i.e. on the same screen) as the treatment message that respondents read, but two questions later in the online questionnaire. It was also not directly about the upcoming election, but asked respondents to rate their party favorability in more general terms. Based on the results reported in Table [C.1](#), none of the treatment coefficients is statistically significant at conventional levels, possibly suggesting that the treatments were not strong enough to produce significant shifts in respondents' general party favorability.

C.5 Results with Continuous Political Knowledge Variable

Table C.1: Party Favorability Rating in 2017 German Federal Election

	(1) AfD	(2) CDU/CSU	(3) SPD	(4) FDP/Greens
Economic Voting	-0.014 (0.252)	-0.098 (0.247)	-0.339 (0.231)	-0.230 (0.187)
Strategic Voting	0.173 (0.258)	-0.038 (0.244)	-0.241 (0.232)	-0.004 (0.188)
Uncertainty	-0.194 (0.254)	0.002 (0.242)	-0.161 (0.232)	-0.109 (0.187)
Social Inequality	-0.153 (0.250)	-0.265 (0.241)	-0.123 (0.229)	-0.311 (0.187)
Constant	4.567 (0.438)	4.012 (0.421)	5.678 (0.413)	5.365 (0.324)
Controls	Yes	Yes	Yes	Yes
Observations	1673	1687	1679	1669
R^2	0.42	0.33	0.34	0.18

Note: Dependent variable is party favorability in 2017 German federal election for the party indicated at the top of each column. OLS regression with control variables: indicators for region (East vs. West), age group, gender, income group, education level, party identification, and most important topic (immigration, crime, terrorism). Models control for 122 respondents who are less anti-immigrant (indicator and interaction with treatments). Coefficients of primary interest for hypothesis tests are in bold.

Figure C.1: Effect of Economic Voting Treatment on AfD Support

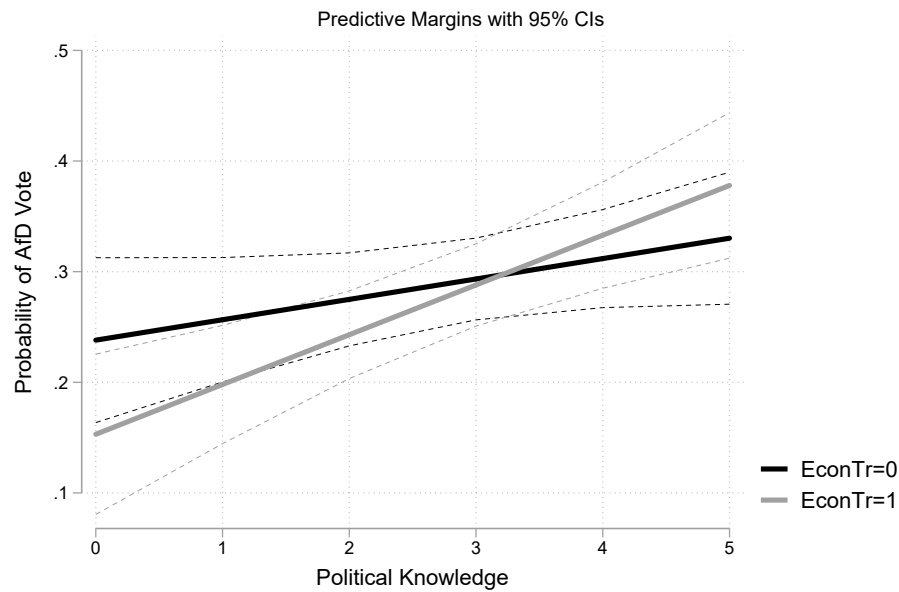


Table C.2: Models for 2017 Vote Intention with **Continuous** Political Knowledge Variable

	(5)	(6)	(7)	(8)
	AfD	CDU	SPD	FDP/Greens
Economic Voting	-0.085 (0.052)	0.0025 (0.039)	0.046 (0.044)	-0.044 (0.028)
Strategic Voting	-0.033 (0.050)	0.084 (0.040)	-0.042 (0.044)	-0.019 (0.037)
Uncertainty	-0.040 (0.054)	0.067 (0.041)	0.017 (0.044)	-0.047 (0.029)
Social Inequality	-0.10 (0.047)	-0.0044 (0.037)	0.061 (0.043)	-0.0037 (0.036)
KnowledgeContinuous	0.018 (0.011)	-0.0021 (0.008)	0.011 (0.008)	0.0046 (0.007)
EconTr*KnowContinuous	0.027 (0.016)	0.020 (0.012)	-0.021 (0.012)	-0.00053 (0.009)
StratTr*KnowContinuous	0.013 (0.016)	-0.015 (0.012)	0.00030 (0.013)	0.0043 (0.012)
UncertTr*KnowContinuous	0.014 (0.015)	-0.0095 (0.011)	-0.011 (0.012)	0.0034 (0.009)
SocInTr*KnowContinuous	0.035 (0.015)	0.0033 (0.011)	-0.019 (0.012)	-0.0022 (0.011)
Constant	0.16 (0.051)	0.020 (0.041)	0.037 (0.047)	0.081 (0.038)
Controls	Yes	Yes	Yes	Yes
Observations	1786	1786	1786	1786
R^2	0.47	0.41	0.42	0.27

Note: Dependent variable is vote intention in 2017 German federal election for the party indicated at the top of each column. OLS regression with control variables: indicators for region (East vs. West), age group, gender, income group, education level, party identification, and most important topic (immigration, crime, terrorism). Models control for 122 of the 1,786 respondents who are less anti-immigrant (indicator and interaction with treatments).

Figure C.2: Effect of Social Inequality Treatment on AfD Support

