

Partisanship as Cause, Not Consequence, of Participation

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Abstract

In most democracies, citizens who identify with a political party are more likely than non-partisans to turn out to vote. But why is this the case? Does voting foster partisanship, as prominent models of political learning and cognitive dissonance postulate? Or does partisanship encourage voting, as expressive voting models and social identity theory suggest? I introduce the concept of *partisan duty* to capture the role of partisan social identities in the turnout decision, and present new empirical tests of the relationship between partisanship and voting. I leverage a unique institutional arrangement in Chile to establish the direction of causality with a regression discontinuity, and I implement a novel survey design with behavioral outcomes to identify causal mechanisms. Data from the US confirms that the main findings generalize beyond Chile. Electoral participation does not generate partisanship. Instead, partisanship mobilizes voters: it increases the expressive benefits to voting and generates a sense of duty to support one's partisan group.

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Partisan identification is intimately related to many central aspects of political behavior. It influences vote choice (Campbell et al. 1960), voters’ policy positions (Samuels and Zucco 2014), and the manner in which voters process new political information (Bartels 2002). Partisanship has the power to undermine politicians’ accountability (Besley 2006) and it can even trump voters’ democratic values in polarized societies (Svolik 2017).

Yet large gaps remain in our understanding of partisanship’s effect on another key subject of political behavior: voter participation. For decades, scholars have written at length about higher turnout rates among partisans. But until now, causal identification problems have hampered our ability to draw important theoretical conclusions. We know that partisans are more likely to participate than non-partisans. But the question remains, *why* are partisans more likely to turn out?

Are partisans simply people who are more interested in politics (and, therefore, more likely to participate)? Or is there a direct, causal relationship between partisanship and turnout? Prominent theories of political behavior provide the foundation for two distinct interpretations of the direction of causality in this relationship. According to the *mobilization hypothesis*, partisanship increases turnout. Those who identify with a political party place value on the opportunity to express and reaffirm this identity by voting (see, e.g., Fiorina 1976; Carlin and Love 2015). But according to the *identity-formation hypothesis*, the effect runs in the opposite direction: electoral participation generates partisanship (see, e.g., Leon and Rizzi 2016; Singh and Thornton 2013; Lundell 2007).¹ When citizens vote, they engage with the party system, learn about which parties best represent their preferences, and become more likely to adopt partisan identities (Converse 1969).

The evidence for these two accounts is inconclusive. Disentangling the causal connections between partisanship and turnout poses a major empirical challenge. Partisanship and turnout are closely related to many other political characteristics; this dynamic might produce spurious correlations that do not reflect an underlying causal relationship between partisanship and turnout. For example, a citizen’s level of interest in politics might drive both her decision to turn out and her identification with a party. Ideally, a researcher would induce exogenous variation in partisanship and observe whether rates of turnout change in response (and vice versa). But this is no simple task. Partisan identification develops over long periods of time; short-term experimental interventions would be unlikely to change such a fundamental political identity. Likewise, exogenous variation in turnout is rare in the real world.

In this paper, I present a series of tests that overcome these limitations and allow us to confidently conclude that partisanship mobilizes voters, but that repeated voting does not foster partisanship. These tests also allow us to discern the mechanisms linking partisanship to participation. I find evidence of two key mechanisms. The first — expressive utility —

¹The identity-formation hypothesis has received less scholarly attention than the mobilization hypothesis, but this argument has been advanced by many empirical studies, including Lundell (2007), Dalton and Weldon (2007), Singh and Thornton (2013), Leon and Rizzi (2016), Mackerras and McAllister (1999), Lupu and Stokes (2010), and Converse (1969). Festinger (1962) and Acharya, Blackwell and Sen (2018) (among others) provide strong theoretical foundations for the identity-formation hypothesis, highlighting the importance of cognitive dissonance and the ways in which behavior can influence preferences and identities.

is well-established in the theoretical literature. The second — partisan duty — is a new theoretical concept I introduce, which highlights the desire to do one’s part to contribute to a group success. I show that partisans are willing to pay material costs to express their partisan identities, and that they are further mobilized by a sense of duty to contribute to their party’s electoral success.

I present data from a regression discontinuity and a novel survey measurement strategy with behavioral outcomes, both conducted in Chile. Chile offers a compelling context for studying the relationship between partisanship and turnout. I demonstrate that a unique voting law generated exogenous variation in Chileans’ voting patterns over long periods of time. I leverage this variation to rigorously examine the effect of voting on partisan identification. In turn, original survey data provides evidence that partisans are willing to pay real costs to express their identification, even when doing so has no impact on electoral outcomes, policy implementation, or party reputation.

The evidence paints a compelling picture. The act of turning out to vote has, at best, a minimal effect on one’s likelihood of forming a partisan identity. But partisan identities deeply affect the incentives to turn out to vote. Partisans gain expressive utility from casting a vote to support their party, and they are willing to pay costs for these expressive benefits. The mobilization hypothesis is well-supported by the empirical evidence. The identity-formation hypothesis, by contrast, finds no support in this study.

The main empirical contribution of this paper is to compare the validity of competing theories in a context where we can precisely differentiate between the observable implications of each theory. As with any study, this one faces a tradeoff between precision and generalizability. By focusing the investigation on Chile, I am able to conduct well-identified tests of the various causal mechanisms and contribute to the theoretical debate over partisan turnout. However, this approach leaves open the question of whether we would find the same results in other contexts.

As a first step in assessing how general the findings are, I analyze panel data on partisanship and turnout from the United States. These data confirm that the main results travel beyond Chile. Partisans in the US are much more likely to turn out than non-partisans, but voting generates no measurable increase in partisan identification.

Partisanship and turnout are two of the most-studied phenomena in political behavior; understanding the relationship between them is important in its own right. But this study has broader implications as well. In the process of learning why partisans vote more frequently than non-partisans, we learn general lessons about the factors that encourage people to turn out to vote, including the intrinsic motivations inherent in expressive voting. The study also sheds light on the ways in which partisan identification changes people’s attitudes towards elections, and shapes a voter’s sense of her role in politics.

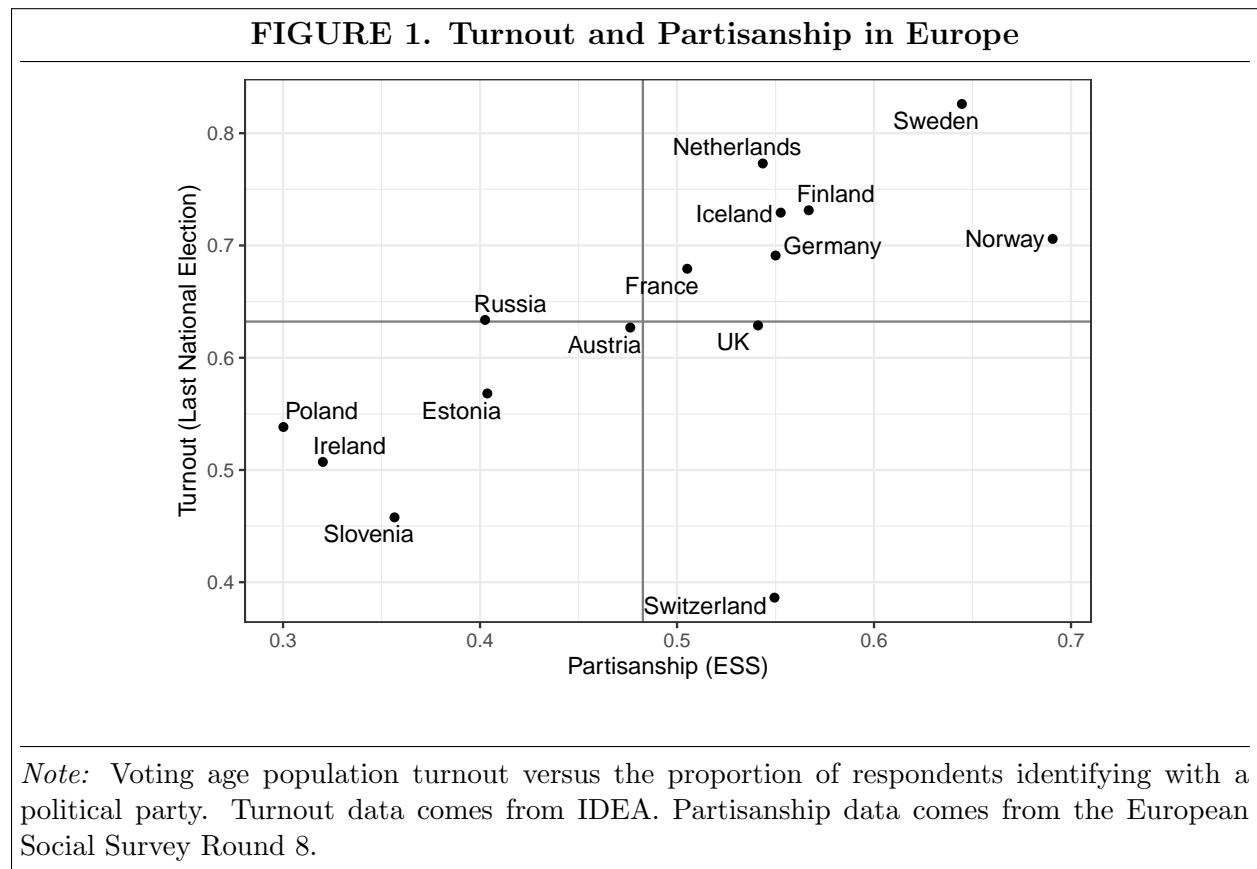
And the importance of this causal question extends beyond studies of voting behavior. The mobilization hypothesis has clear implications for party strategy; the identity-formation hypothesis underlies theories about the effects of compulsory voting (Singh and Thornton 2013) and the relationship between democratic stability and electoral volatility (Lupu and Stokes 2010). A more precise understanding of the microfoundations of voting behavior

contributes to better theorizing about elite behavior, institutional implications, and macro-level patterns in democracies.

Theories of Partisan Turnout

The robust relationship between partisanship and turnout is well-established in American politics (Campbell et al. 1960; Gerber, Huber and Washington 2010) and comparative politics (Powell 1986; Resnick and Casale 2014; Smets and van Ham 2013). Partisanship is a strong predictor of turnout at the individual level (Carlin and Love 2015; Carreras and Castañeda-Angarita 2014). And the pattern scales up to the aggregate level (Heath 2007; Vowles 1994).

In Chile, partisan identification has closely tracked with turnout since the return to democracy. In the early 1990's, approximately 75% of Chileans identified with a political party and more than 80% turned out to vote. Nearly three decades later, fewer than 25% identify with a party and fewer than 50% turn out to vote.² Cross-national comparisons in Europe reveal a similar pattern. Figure 1 plots turnout against the proportion of survey respondents identifying with a political party in fifteen countries included in the European Social Survey. It shows a strong positive correlation, with only one outlier: Switzerland, where frequent referendums make national elections less salient (Franklin 1996).



²See Fig. A.1 in the online appendix.

These simple correlations, however strong, can tell us only so much about the relationship between partisanship and turnout. We know that these variables move together, but we don't know how or why. Below, I outline two possible interpretations of this relationship. The identity-formation hypothesis maintains that turnout produces partisanship; the mobilization hypothesis maintains that partisanship generates turnout.

The Identity-Formation Hypothesis

A large body of literature postulates that the more someone votes, the more likely she is to become a partisan. Converse's (1969) social learning model claims that partisanship increases as a result of experience with a party system, particularly during the voting years. The act of voting forces citizens to think about the parties regularly, and they consider their relation to each of the parties when deciding how to vote. This cognitive engagement increases the likelihood that they will begin to identify with one of the parties (Mackerras and McAllister 1999; Singh and Thornton 2013).

And a burgeoning literature on cognitive dissonance in politics has questioned the traditional story that preferences are always causally prior to behavior; instead, people may behave a certain way out of habit or experimentation, and then adjust their preferences and self-conceived identities to justify this behavior (Acharya, Blackwell and Sen 2018). Cognitive dissonance simply refers to a state in which two or more pieces of information "psychologically do not fit together" (Festinger 1962: 93). People seek to avoid the psychological discomfort of cognitive dissonance:

"Cognitive dissonance is a motivating state of affairs. Just as hunger impels a person to eat, so does dissonance impel a person to change his opinions or his behavior." (Festinger 1962: 93)

Evidence from many psychological experiments shows that people often change their beliefs to alleviate cognitive dissonance (see, e.g., Festinger and Carlsmith 1959; Brehm 1956; Aronson, Fried and Stone 1991). More recently, political scientists and economists have tested the influence of cognitive dissonance on political beliefs. Empirical studies have found evidence of this dynamic in many aspects of political life, including presidential approval ratings, attitudes towards democratic norms, and general political interest (Mullainathan and Washington 2009; Quintelier and Van Deth 2014). Partisan identities, then, might be the result of accumulated political activity, such as voting for a party multiple years in a row (Lupu and Stokes 2010).

The theory of cognitive dissonance applies broadly, even to contexts in which one's behavior does not significantly deviate from one's prior beliefs or preferences:

"Even when people vote for a party they clearly prefer to others, actually doing so may still lead them to think more highly of this party than before the election. In other words, even if there is no obvious conflict between behavior and attitudes, there may be a slight dissonance that can be reduced by changing attitudes so that they even more clearly favor the behavior exhibited." (Bølstad, Dinas and Riera 2013: 432)

Thus, voters with no partisan identity might cast a ballot for the party that they weakly prefer; then, “the desire to resolve cognitive dissonance leads them to see themselves as partisans of this party” (Lupu and Stokes 2010: 92).

The identity-formation hypothesis has been advanced by many theoretical and empirical studies, but few have conducted direct tests of the theory. Singh and Thornton (2013), Lundell (2007), Mackerras and McAllister (1999), and Dalton and Weldon (2007) observe that more people identify with political parties when voting is compulsory, and argue that the identity-formation hypothesis explains this phenomenon. Turnout rises under compulsory voting and, according to the identity-formation hypothesis, voting makes people more likely to become partisans. Thus, compulsory voting increases partisan identification as a direct result of increasing turnout.

But other plausible explanations for this pattern exist. For example, political parties might target a broader base of support in compulsory voting systems because they know that a larger share of the electorate will turn out to vote. In this alternative account, widespread partisanship in compulsory voting systems is not a direct result of increased voting, but rather of party strategy.

The best test to date of the identity-formation hypothesis is Leon and Rizzi (2016). The authors used a discontinuity among young voters in Brazil to gain causal leverage, and found mixed results. Brazil mandates voting among citizens aged 18–70. Citizens who are 16–17 are eligible but not required to vote. In their study, the authors examine the political preferences and behaviors of Brazilians between the ages of 16 and 20. At the age of 18, citizens pass from a voluntary voting system to a compulsory one, producing a discontinuity in the costs of abstention (and, by extension, turnout rates). Leon and Rizzi (2016) leverage this discontinuity to assess the behavioral effects of electoral participation.

Voting made young people more likely to identify with one party — the Brazilian Social Democracy Party (PSDB) — in the short term, but not with any other party. And even the effect of PSDB support might be fleeting. The study looked only at Brazilians between the ages of 16 and 20. Voters of this age group are often still developing their political identities. Voting in their first election might give young adults a head start in developing their political identities, but this effect might dissipate over time as non-voters have more political experiences and develop their own identities. If this is the case, it would not produce the sustainable long-term effect necessary to explain major differences between partisanship in compulsory versus voluntary voting systems, or the persistent relationship between partisanship and turnout across age groups.

Although the identity-formation hypothesis has seen limited empirical testing, it builds on noteworthy developments in political behavior which show that actions often precede beliefs. With a strong grounding in social psychological theory and implications for institutional design, the identity-formation hypothesis warrants further empirical investigation. But it is not the only theoretically cogent causal pathway between voting and partisanship.

The Mobilization Hypothesis

An alternative approach to explaining the turnout-partisanship correlation considers how partisanship might change the calculus of voting. Social identity theory provides a strong foundation for the argument that partisanship increases the incentives to vote. Theorists in this tradition argue that partisan identification constitutes a “genuine form of social identification” (Green, Palmquist and Schickler 2002: ix). Citizens develop a sense of what types of people a party represents — the party’s “brand” (Lupu 2016) — and whether they fit into such groups. Many recent empirical studies support this interpretation, showing that party identification influences attitudes and behavior in a manner similar to other fundamental social identities, with effects that extend beyond the political arena to everyday social settings (Iyengar, Sood and Lelkes 2012; Mason 2015; Carlin and Love 2018).

When partisans internalize a sense of group membership, as social identity theory posits, they experience expressive benefits from voting. Votes operate as “a means to express political beliefs and preferences and, in doing so, to establish or reaffirm [one’s] own political identity” (Schuessler 2000: 88). A partisan enjoys expressive benefits from voting regardless of whether her vote is pivotal to the result: she gains utility from the act of voting, not just the electoral outcomes. Even if a voter does not have strong preferences over policy platforms in the current election, if she identifies with one of the parties, she still gains utility from voting and reaffirming her identity as a partisan. And she enjoys these expressive benefits even if her vote is not observed by others: by voting for her party, she is acting in congruence with her partisan identity and reaffirming this aspect of her sense of self. Through this mechanism, partisanship increases the payoffs to voting and mobilizes voters who might otherwise stay home.

Fiorina (1976) formalizes this concept in a classic rational choice model of voter turnout. Voters have preferences over parties’ platforms, but they also have partisan identities (which may or may not align with their preferences over party platforms in a particular election). Fiorina builds on the Riker and Ordeshook (1968) formulation, which models the utility of voting as an additive combination of the costs of voting, the benefits of fulfilling a sense of civic duty,³ and the expected policy payoff (the difference in utility from one party’s platform versus the other, multiplied by the perceived probability of casting a decisive vote that changes the outcome of the election). Fiorina adds a new term to the model: an “expressive factor” (395) that accounts for the “psychic satisfaction” (396) of voting in accordance with one’s partisan identity (or the “psychic cost” of casting a vote for a different party).

Fiorina’s model emphasizes that partisans don’t just prefer a given party’s platform or performance in office. They internalize a sense of partisan membership (Greene 2002). When a partisan votes for her party, she gains expressive utility from re-affirming her identity, separate from any instrumental benefit of increasing the likelihood of electing a government whose policies she prefers.

But another result of this internalized identity is that “the group’s failures and victories become personal” (Huddy, Mason and Aarøe 2015: 3). Membership in a partisan

³Riker and Ordeshook (1968) frame the D term more broadly than the common conception of civic duty, and even mention the benefits from affirming a partisan preference as among the factors contributing to D .

group, similar to membership in a sports team’s fanbase, generates an emotional investment in the outcomes of political competition. Turning out to vote is often a low-cost way of joining in a group effort to achieve a collective victory. Achen and Sinnott (2007: 9) draw parallels between turning out to vote and cheering for a favorite sports team: “Neither act is instrumentally rational, and thus they will seem mysterious to those whose explanatory repertoire is confined to self-interested motivations. But there is little doubt that people have a capacity for supporting inexpensive group efforts.”

This sense of contributing to a group success is not adequately captured by classic models of turnout, which focus on an individual’s (negligible) chance of changing electoral outcomes and on consumptive benefits of voting (utility, such as expressive utility, that comes from the act of voting and is independent of electoral outcomes). Even if partisanship generates a deeper investment in the party’s success, each voter still has an infinitesimal chance of casting a decisive vote that changes the electoral outcome.⁴ A partisan may care deeply about the social reputation of the party with which she identifies (Huddy, Mason and Aarøe 2015), but her one vote will hardly affect that reputation on its own.⁵

To fully understand the importance of partisanship for turnout, then, would require that we reformulate the theoretical framework. The classic approach to modeling turnout focuses entirely on the individual (What is the chance that I alone will change the outcome of the election? What is my individual consumptive benefit from voting?). But true social identification with a party does not just provide another additive utility benefit to voting; it changes the way that a voter defines or optimizes her utility. A model accounting for partisan social identification must accommodate group-level considerations, such as the concern for group-level outcomes in a rule-utilitarian framework or the optimization of group-level behavior in Kantian optimization approach.⁶ One of the primary reasons that partisanship is such a powerful mobilizing force is that it creates a sense of membership — partisans are “part of the team” and they behave as such.

To capture the key social-group dimension of partisanship, I introduce the concept of *partisan duty*. I define partisan duty as the sense of duty or obligation to do one’s part to

⁴Fiorina (1976) incorporates expectations about the closeness of elections into his comparative statics. However, his formulation is subject to the same problems as the Downsian model: the probability of affecting the electoral outcome is always so small that it should have no discernible effect on behavior, if voters are optimizing in the way that the classic models postulate.

⁵In the case of party reputation, increasing vote shares have an effect separate from winning or losing. The reputation of a party that loses by a very small margin will fare much better than one that loses by huge margins. An individual vote for the party, even if it is not decisive, contributes to the party’s reputation with certainty. But here too the effect is infinitesimal — the marginal effect of one additional vote on a party’s reputation is negligible.

⁶Whereas many benefits to voting have been modeled as additional parameters in the classic Downsian voting model (e.g., the Riker-Ordeshook (1968) formulation), group-level considerations require a different theoretical framework. Two such frameworks are rule-utilitarian models — where a rule-utilitarian optimizes over a group’s welfare rather than just her individual welfare — and Kantian optimization models — where an agent optimizes over her own individual welfare, but selects her strategy under the assumption that other agents who share her preferences will select the same strategy she selects. See Feddersen and Sandroni (2006), Roemer (2019), and Rau (2021*a*) for more detailed presentations of these modeling approaches and how they differ from the decision-theoretic framework of Downs (1957).

contribute to a partisan group’s success. Partisan duty can be modeled in the context of a group rule-utilitarian model (see Harsanyi 1980; Feddersen and Sandroni 2006; Coate and Conlin 2004).⁷ In a group rule-utilitarian model, elections are modeled as contests between distinct groups in which individuals “want to ‘do their part’ to help their group win” (Coate and Conlin 2004: 1476). Rule-utilitarian models of turnout provide a solution to the paradox of rational turnout, predicting high turnout rates where the classic Downsian model does not (Feddersen and Sandroni 2006).

Partisan duty is a natural extension of the rule-utilitarian framework. The stronger a voter’s party identification, the more likely she is to think about voting in terms of her role in a partisan group. And as rule-utilitarian models illustrate, this makes voters more likely to turn out. Even if a voter acknowledges that her individual vote will not change the outcome of the election, she votes because she wants to do her part to help her group win. And the rule-utilitarian model shows that this group-based thinking is perfectly at home in a rational choice model of turnout, albeit not the classic Downsian approach. And this approach has the added benefit of recovering the relevance of perceived closeness of the election and the importance of the outcome to the voter — partisans are more likely to turn out than non-partisans, and they are especially likely to turn out at high rates when they perceive that the election will be close or they care deeply about the outcome.⁸

Empirical Testing

Both the mobilization hypothesis and the identity-formation hypothesis are built on a strong foundation of theoretical literature, and several recent empirical studies have developed clever strategies for tackling the inherent obstacles to causal identification. Yet we still lack conclusive evidence for either hypothesis.

Gerber, Huber and Washington (2010) provide the strongest evidence to date of a causal relationship between partisanship and turnout. The authors conducted a field experiment in which they randomly assigned some subjects to receive a letter reminding them that they must register with a political party if they wished to vote in an upcoming primary. Subjects who received the letter were slightly more likely to report identifying with a party and more likely to turn out for the primary. However, we cannot determine the *direction* of this causal effect from their data; partisan identification was measured after the election. It is possible that the treatment induced feelings of partisanship which then caused higher rates of turnout. Alternatively, by providing information about the upcoming election, the treatment may have directly boosted turnout, and the act of voting in a partisan election

⁷Partisan duty can also be incorporated into a model of Kantian optimization. See Rau (2021*a*) for a more detailed development of the concept of partisan duty in the context of the group-rule-utilitarian model and an original Kantian model of turnout.

⁸In the classic Downsian model (and refinements such as Riker and Ordeshook (1968)), perceived closeness of the election and the importance of the outcome to the voter are both rendered irrelevant. Closeness only factors into the model in terms of p , the probability that the election is determined by a one-vote margin — a vanishingly unlikely possibility in a large election, even one that is expected to be very close. And B , the voter’s preferences over electoral outcomes, is multiplied by this vanishingly small probability, such that it drops out of the equation. But empirically, we know that both these factors influence voters’ decisions to turn out or abstain (see Aytaç and Stokes 2019).

may have increased partisanship. Although Gerber, Huber and Washington (2010) provide strong evidence of a causal relationship between partisanship and turnout, their study does not allow us to adjudicate between the two hypotheses.

And the strongest test of the identity-formation hypothesis (Leon and Rizzi 2016) is limited by the exclusive focus on young adults: is voting in one's first election an adequate proxy for the general effects of voting in the population at large? Or is it a stand-in for early political experiences more broadly?

Further empirical testing is necessary to adjudicate between the mobilization and identity-formation hypotheses. In the following sections, I present data from a series of novel tests that allow us to identify the direction of causality and test the mechanisms that underlie the relationship between partisanship and turnout.

I begin with a test of the identity-formation hypothesis. I leverage a unique voting rule in Chile to directly test the causal effect of turning out to vote on the development of partisan identities. I show that voting does not generate a sense of partisan identification, even over extended periods of time: decades of voting in every election made an individual no more likely to identify with a political party than if she had always abstained.

I then test the mobilization hypothesis with two original surveys in Chile. I use behavioral outcomes and an experimental design to impose a cost on expressing one's partisanship. I find strong evidence that expressive voting and partisan duty animate the relationship between partisanship and turnout, just as the mobilization hypothesis predicts.

Finally, I assess the generalizability of these findings with panel data from the United States. Once again, the evidence is inconsistent with the identity-formation hypothesis. Instead, any causal relationship stems from the mobilizing power of partisan identification.

Does Voting Create Partisans? Evidence from Chile

In 1988, after fifteen years of brutal dictatorship under General Augusto Pinochet, Chile held a plebiscite to determine whether Pinochet would remain in power or be replaced by a democratically elected president. Seventeen parties opposing the Pinochet regime joined to form the Coalition of Parties for NO (*Concertación de Partidos por el NO*, or simply *Concertación*). The Concertación parties believed that the majority of the population would agree with them in opposing Pinochet's continued rule. But they feared that many would stay home due to widespread skepticism and resignation (Tironi 1990: 47).

Many people originally did not plan to register to vote in the plebiscite: "they believed it would be futile to vote in a context that was so prone to fraud and coercion, and they did not want to legitimize an undemocratic exercise" (Roberts 1995: 518). The potential success of the No campaign would depend on turnout: it could only win if turnout was high. To this end, the parties of the Concertación devoted immense effort to a large registration drive (Roberts 1995). These mobilization efforts were effective. Ninety-two percent of the eligible population registered to vote and, in a shock to regime officials, 56% voted "No" on Pinochet and ushered in a new era of democratic governance.

Registering for the plebiscite constituted a long-term commitment to voting. From 1988 to 2011, Chile combined compulsory voting with voluntary registration. Voters could choose whether or not to register, but once they registered, they were obligated to vote in all future elections. The compulsory voting rule was enforced, and turnout among registered voters was about 90% through 2010 (Corvalan and Cox 2013). But registration rates fell rapidly. At the time of the 1993 presidential election (the first presidential election after the return to democracy), only 55% of newly eligible voters reported registering to vote.⁹

If voting were voluntary, many of those who registered for the plebiscite would have abstained in future, lower-stakes elections. In a 1992 survey, 34% of those who were registered said that they would abstain from the upcoming municipal election if voting were voluntary.¹⁰ But they were obligated to keep voting. Younger Chileans, on the other hand, faced a very different decision. Those who came of age after the plebiscite were not registering to oust an authoritarian regime. They were simply choosing to vote (or not) in democratic elections. This generated an unusually large age bias in turnout in Chile, which we can exploit to test the proposition that voting generates partisanship.

Consider a hypothetical individual who was born in December 1970. At the time of the plebiscite, she was younger than 18 and, therefore, ineligible to register for the vote. Once she did turn 18, suppose she chose not to register. She might have had no interest in voting in regular democratic elections. Or any interest she had might have been outweighed by the substantial cost imposed by the compulsory voting rule: she would be accepting a legal obligation to vote in every national and local election for the rest of her life (or until some point in the distant future when the rule might be changed). Thus, like many Chileans of her cohort, she chose never to register. By the time of the 2009 election, she would have been approaching age 40 having never voted in her life.

Yet if she had been born just three months earlier, in September 1970, her voting history might have looked very different. Having come of age just in time, she likely would have joined the 92% of Chilean adults who registered for the 1988 plebiscite. Whereas the stakes of regular elections between democratic candidates were not enough to compel her to register, the stakes of the plebiscite were high enough. Her desire to vote for or against the continuation of Pinochet's regime, combined with the mass registration drives conducted by the political parties, were sufficient to outweigh the costs of registering. Thus, she registered to vote in 1988 and, because of the compulsory voting rule, continued to vote in every national and local election through 2010.

The combination of a unique set of voting and registration rules, and the high stakes of the 1988 plebiscite, generate a rare case of exogenous variation in voting histories. Some citizens who came of age after the plebiscite, of course, registered to vote in later elections

⁹Newly eligible voters are those who were eligible to vote in 1993 but not in 1989. This estimate (55%) comes from the CEP November 1994 survey. In the November 1994 wave, only birth years were available (not exact birthdates, as are used in the regression discontinuity). The estimate, therefore, only includes those born between Jan 1, 1972 and Dec 31, 1974. The estimate of 55% overestimates registration among newly eligible voters. Social desirability bias generates higher reported turnout than official electoral data. But data from the Chilean electoral service is not granular enough to precisely measure this age group.

¹⁰Estimate comes from the CEP April 1992 survey.

(and a very small number who were eligible chose not to register for the plebiscite). But many citizens looked like the individual described above: they were willing to register for the plebiscite but, given the choice, would not register to vote in subsequent elections. Among this subset of the population, small shifts in one’s birthdate meant the difference between a lifetime of regular electoral participation, and complete abstention from all elections (through 2010).

Data and Model

I use survey data collected between June 2006 and December 2011 to estimate a regression discontinuity.¹¹ In 2012, the legislature passed a new voting law that made registration automatic and voting voluntary. I focus on the last few years of the compulsory voting regime to conduct the most favorable test for the theory that turnout generates partisanship. By the time these data were collected (2006–2011), Chileans who registered for the plebiscite had voted in regular national and local elections for about two decades. Even if the causal effect of turnout on partisanship occurs slowly as voters accumulate experience with the political system, any potential effects should show up within this long timeframe.

Moreover, using data collected approximately 20 years after the plebiscite limits the possibility of a compound treatment. Those who voted in the plebiscite participated in a historic political event. If we were to use data from the first few years after the return to democracy, any effect we observed might stem from either (1) the general experience of voting or (2) the particular experience of voting in the plebiscite. But as we move forward in time, the experience of voting in the plebiscite grows more distant. At the same time, the treatment of voting in elections grows stronger, as registered voters were obligated to vote in many elections throughout these two decades.

The dataset comprises a representative sample of Chileans and includes four key variables for each individual. First, we have the exact birthdate of each respondent, allowing us to determine their eligibility for the plebiscite. Birthdate is the running variable for the regression discontinuity. Respondents were also asked whether they identify with any political party. Their response to this question serves as the main dependent variable. In addition, all respondents were asked whether they identify with a coalition. Since coalitions play a central role in Chilean politics, I use this as an alternative dependent variable for robustness tests.

Finally, we have the self-reported registration status of every respondent at the time of the survey. This variable is not included in the main equation: the identification strategy hinges on using date of birth (eligibility for the plebiscite) as the independent variable instead of reported voting history. I use self-reported registration status in a separate equation to estimate the proportion of compliers in the sample (those individuals who would register to vote if and only if they were eligible for the plebiscite) and provide a substantive interpretation of the main result.

For the main analysis, I estimate the following model:

¹¹The data was collected by the *Centro de Estudios Públicos* as part of their regular public opinion surveys.

$$y_i = \beta_0 + \beta_1 d_i + \beta_2(a_i - c) + \beta_3(a_i - c)d_i + \epsilon_i \quad (1)$$

Where y_i is a binary variable, indicating whether i identifies with any political party; d_i is a binary variable indicating whether or not i was eligible for the plebiscite; a_i is i 's birthdate; and c is the cutoff for plebiscite eligibility (October 5, 1970). The parameter of interest is β_1 : this tells us what effect plebiscite eligibility had on partisan identification, separate from the general effect of age. β_2 and β_3 estimate the effect of age on partisanship, conditional on a person's eligibility status. Here, we use the term $a_i - c$ (rather than just a_i) so that β_2 and β_3 drop out of the equation as $a_i \rightarrow c$. Therefore β_1 directly estimates the jump (or discontinuity) in predicted partisanship from plebiscite eligibility.¹²

With respect to the theoretical question at hand — does voting make people partisan? — β_1 is the intent-to-treat (ITT) estimate. The ITT estimate tells us the effect of plebiscite eligibility on future partisanship. I present the results primarily in this form, because we do not know the precise number of compliers in the sample. (In this context, a complier is an individual who would register to vote if and only if they were eligible for the plebiscite.) However, I also present a local average treatment effect (LATE) interpretation of the main result, using self-reported registration status to generate a conservative estimate of the number of compliers. The LATE directly estimates the effect of voting in an election on future partisanship.

Interpreting the Discontinuity Estimate

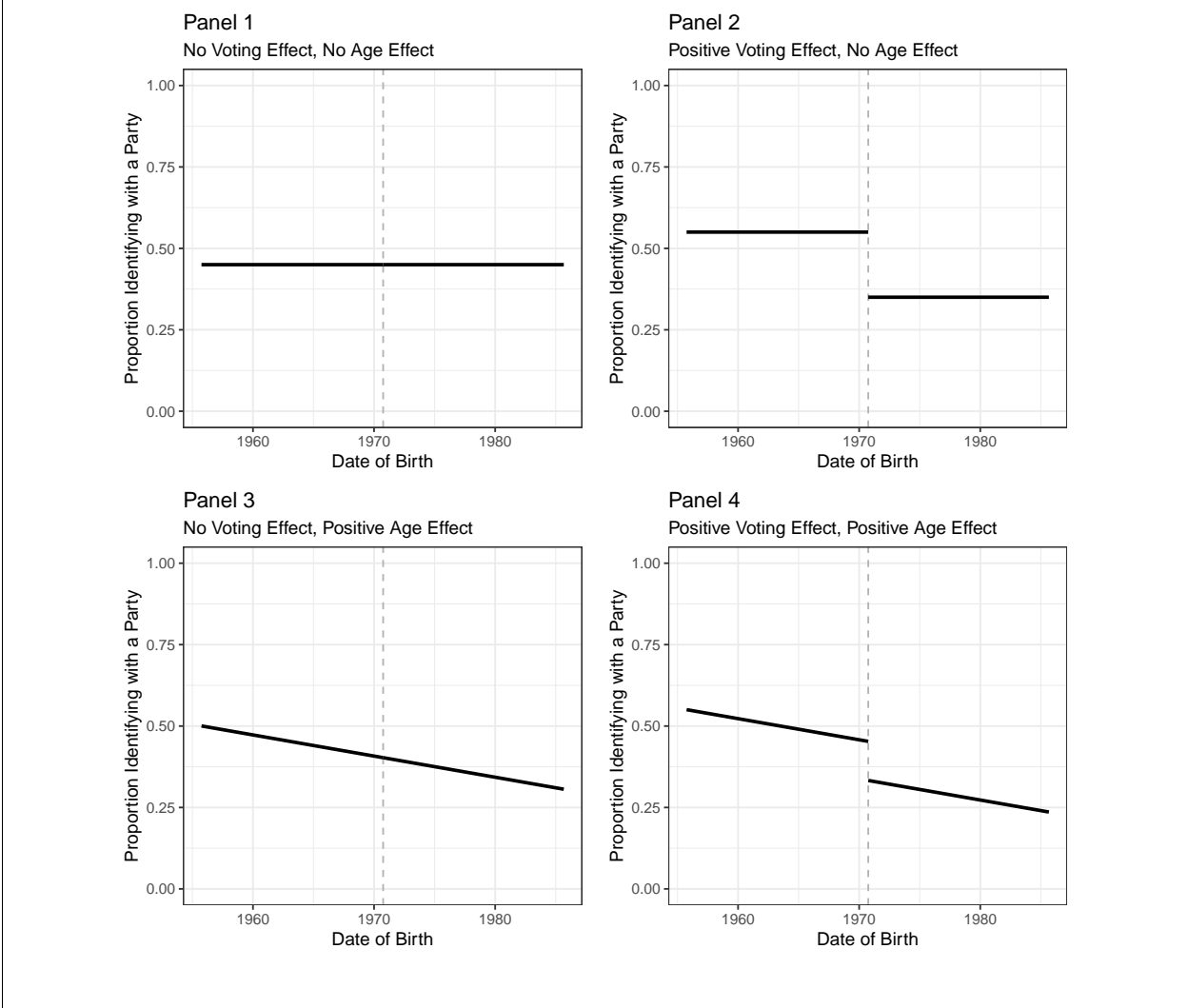
What kind of results would we observe if turnout had a causal effect on partisanship? Figure 2 illustrates the regression discontinuity with a series of hypothetical datasets. Each graph plots an estimate of Eq. 1. The vertical dashed line indicates the cutoff for eligibility to vote in the plebiscite: everyone to the left of the dashed line was eligible for the plebiscite; everyone to the right was ineligible.

Panels 1 and 3 are examples of the kind of results we could observe if voting does not affect partisanship. In panel 1, voters of all ages are equally likely to identify with a party. In panel 3, older voters are more likely to identify with a party, but their heightened partisanship is not a result of their eligibility at the time of the plebiscite. Panels 2 and 4, by contrast, are examples of data that strongly support the identity-formation hypothesis. In panel 4, like panel 3, age increases the likelihood of partisanship. But in contrast to panel 3, we observe a clear discontinuity around plebiscite eligibility. The experience of regularly voting in elections for two decades increased partisan identification, separate from the broader effect of aging on partisanship.

These graphs highlight the value of the regression discontinuity design. If partisanship is positively correlated with age, as in panel 3, a simple difference in means would yield a false positive result. Similarly, if partisanship were negatively correlated with age, we could end up with a false negative. The RDD allows us to control for any broader relationship

¹²See Imbens and Lemieux (2008) and Gelman and Imbens (2019) for discussions of why local linear regression is the ideal model for this discontinuity test.

FIGURE 2. Hypothetical Results and Interpretation



Note: Each graph plots an estimate of Eq. 1 from hypothetical data. The dotted line marks the cutoff for eligibility to vote in the plebiscite: anyone born to the left was eligible, anyone born to the right was ineligible.

between age and partisanship, and use the discontinuity in registration rates to isolate any effect of voting on partisanship.

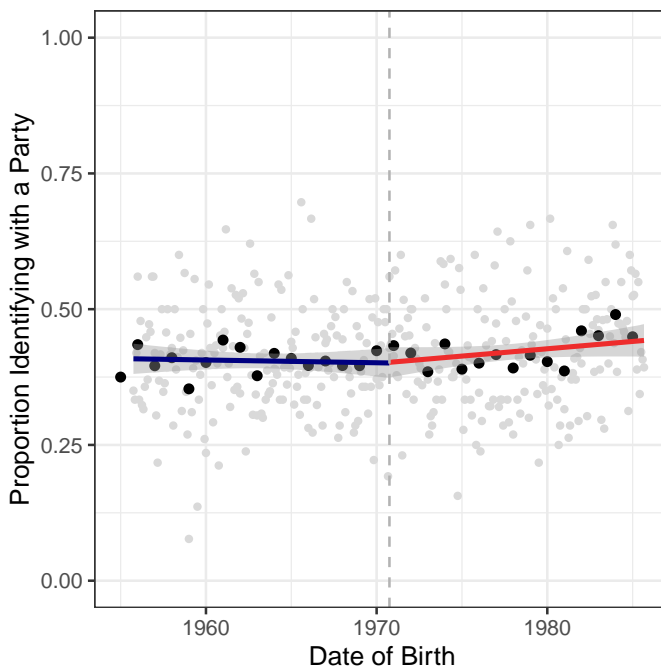
Results

In the dataset used for the analysis (collected from 2006–2011), 94% of those who were eligible for the plebiscite report that they are registered to vote. Among those who were ineligible for the plebiscite, only 42% report that they are registered to vote. Subject to a minimum sample size of 1219 observations, the discontinuity in self-reported registration is significant ($p < 0.05$) at all possible bandwidths.

Despite the large effect of eligibility on registration status and voting history, we observe

no discontinuity in partisan identification. Regardless of the bandwidth selected, we never observe a positive effect of plebiscite eligibility on partisan identification. Figure 3 displays the model fit with a 15-year bandwidth. Each black point represents the proportion of people born in a given year who identify with a political party (the gray points represent the proportion by month). These points are overlaid with the model fit from Eq. 1, presented with 95% confidence intervals. If turnout induced partisanship, then we would expect to see a significant discontinuity at the eligibility cutoff (Oct. 5, 1970). The predicted probabilities just to the left of this cutoff would be significantly higher than those to the right of it (as in panel 2 of Fig. 2). Instead, the estimate resembles a straight line, with no discontinuity.

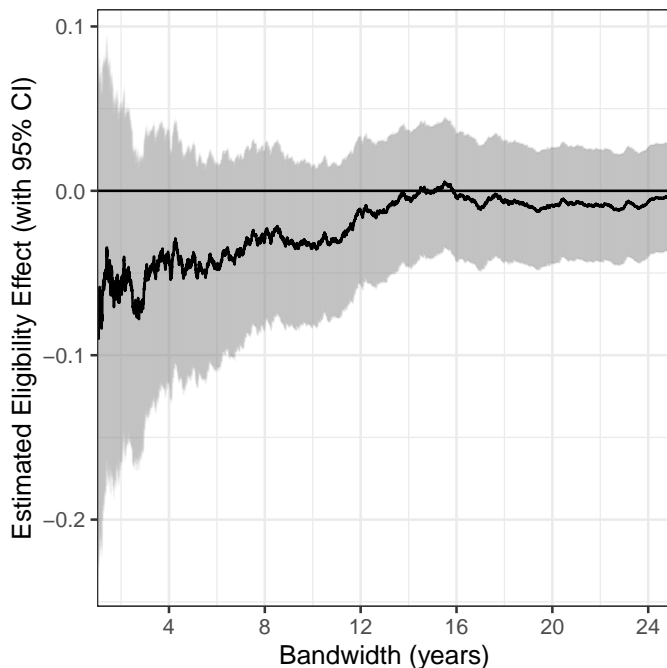
FIGURE 3. RD Test: Plebiscite Eligibility



Note: OLS estimate of Model 1 with a 15-year bandwidth and 95% confidence intervals (n=9699). Each point indicates the average rate of partisan identification for respondents born in a given year (black) or month (gray). The vertical dotted line indicates the cutoff point for eligibility to vote in the plebiscite (those to the left of the line were eligible to vote; those to the right were ineligible). Surveys were conducted from June 2006 to December 2011.

This finding is not specific to the 15-year bandwidth. Figure 4 presents the point estimate of β_1 with 95% confidence intervals for all possible bandwidths from one year to 25 years (increasing in increments of one day). We do not observe a discontinuity in any of these models. In all cases, β_1 is indistinguishable from zero. The failure to identify a statistically significant effect under any specification is striking, especially when considering the statistical power of the test. The Imbens and Kalyanaraman (2012) algorithm indicates that the optimal bandwidth is 8484 days. At this bandwidth, the study is well-powered to

FIGURE 4. RD Coefficient Plot



Note: Coefficient plot for each estimate of Model 1, using bandwidths from 1 to 25 years (increasing in intervals of one day). For each bandwidth, I plot the point estimate of β_1 with a 95% confidence interval. Surveys were conducted from June 2006 to December 2011.

detect an effect of $\beta_1 = 0.041$ or larger at the 95% confidence level.¹³

Power analysis allows us to confidently place an upper-bound on any possible effect and assess whether a result of this size would be substantively meaningful. For the substantive interpretation, it will help to convert the ITT estimate (β_1) to a LATE estimate. That is, rather than estimating the effect of eligibility for the plebiscite on partisan identification, we want to estimate the effect of voting in an election on partisan identification. Two key pieces of data are necessary to make this calculation. We need to know (1) how many elections occurred between the plebiscite and the survey collection and (2) the proportion of compliers in the sample.

The first data point is easily identified. Depending on the time of the survey, between 12 and 15 competitive elections had occurred after the plebiscite: 13.3 on average. The second data point — the proportion of compliers — is harder to identify. We do not know exactly how many individuals would register if and only if they were eligible for the plebiscite. But we can use survey data to estimate this figure.

I estimate Eq. 1 with self-reported registration as the dependent variable. This gives

¹³I use the conventional power level of 80% and a one-tailed test of β_1 . See the online appendix (Figure A.8) for details on the statistical power of other bandwidths.

us a very conservative estimate of the proportion of compliers at 17% of the population. We know that the number of compliers is larger than this because there is significant over-reporting of registration status in the surveys, and the vast majority of this over-reporting comes from those who were ineligible for the plebiscite. When adjusting for over-reporting (using official data on aggregate registration rates), I find that the proportion of compliers is 71–144% higher than the raw numbers indicate.¹⁴

Using the most conservative estimate of compliers (17%), how can we interpret an ITT of $\beta_1 = 0.041$? It would be equivalent to finding that voting in an election has a 2.0% chance of making one a partisan.¹⁵ If we adjust for over-reporting in the complier estimate, this translates to an effect in the range of 0.76% to 1.1%. But even without this adjustment, it is clear that the results of the regression discontinuity calculations are substantively meaningful. If voting had even a 2% chance of making one a partisan, the test would be well-powered to detect it at the 95% confidence level. Yet we find no such effect.

We observe the same results if we look at a simple difference-in-means comparison across the treatment (eligible for the plebiscite) and control (ineligible for the plebiscite) groups. I also calculate the regression discontinuity with an alternative dependent variable: identification with a coalition. Coalitions in Chile are stable groups of parties that often function as “de facto parties” (Carey 2002). The data reveal no effect of voting on coalition identification.¹⁶

Despite the large effect of plebiscite eligibility on voting history, it had no effect on partisan (or coalition) identification. Many factors make this a favorable test-case for the identity-formation hypothesis. The treatment effects are sustained over decades. Political parties focused their efforts on reaching out to those who were registered, and not on mobilizing un-registered citizens,¹⁷ so there were many more opportunities for registered voters to interact with the parties at election time. The party system was stable throughout the decades examined, making the environment conducive to the long-term development of party identities.¹⁸ But even in this context, we observe no effect.

¹⁴See the online appendix (section 2.1) for further details.

¹⁵The LATE interpretation of the ITT depends on a model of the cumulative effect of voting in multiple elections. See section 2.3 in the online appendix for details.

¹⁶Full details for these robustness tests are presented in the online appendix (section 2.2).

¹⁷This strategy was explained in many interviews with elected representatives, their campaign managers, and party leaders (conducted by the author in March and October 2018). See also Venegas (2016).

¹⁸The set of competitive parties in the Chilean system was constant throughout the period studied, though the relative strength of these parties changed — most notably, the Christian Democrats (PDC) grew weaker while the Independent Democratic Union (UDI) grew stronger. Lupu (2018) observes recent convergence between the two major coalitions in Chile, drawing from party manifestos; though the coalitions remain distinct in voters’ minds. In a survey I conducted in March and April 2019 (described in greater detail later in this paper, and in the appendix), 76% of respondents indicated that there were differences between the coalitions and 58% explicitly indicated that the question of which coalition was in power was important to them (notably higher than the 47% turnout in the 2017 national elections). Convergence between coalitions in Chile might not map perfectly onto convergence between parties: as Londregan (2000) noted, the PDC aligned with other center-left coalition members on questions of human rights violations during the Pinochet dictatorship, but were closer to the right-wing parties on other issue dimensions. Whereas the regime dimension was paramount in the early post-Pinochet years, other issues have risen to the forefront of voters’ minds. Even in the absence of any policy movement within parties, this shift in issue importance would

One potential concern is that partisanship might not be as relevant in Chile, compared with other countries. After all, Chilean parties have been described as “stable” but “up-rooted” (Luna and Altman 2011), lacking strong ties with voters in the post-authoritarian era. At the time of data collection, only 41% of Chileans identified with a political party.¹⁹ A closer look, however, reveals that partisanship is essential to understanding participation in Chile. In the 2013 election — the first presidential election conducted with voluntary voting and automatic registration — partisans were 18 percentage points more likely to turn out than non-partisans.²⁰ And in the next section, I show that partisanship plays a central role in Chileans’ decisions to turn out or abstain, now that voting is voluntary.

Partisanship as Mobilization: Survey Evidence

The mobilization hypothesis contends that expressive incentives and partisan duty mobilize partisans who might otherwise stay home. The reduced-form prediction — that partisanship generates turnout — is not well-suited to experimental testing. In contrast to voting, partisanship is an identity, not an action; causal identification is even more challenging when partisanship is the independent variable. Whereas the previous section identified a rare instance of exogenous variation in voting history, it is not even clear what “exogenous variation” would mean with regard to an identity. To the extent that partisanship is a meaningful social identity, it is not easily manipulated. Intervention-induced partisanship tends to be a shallow label rather than a meaningful identity. For example, Burden and Greene (2000) show that state party registration laws in the United States produce higher rates of reported partisanship. But they also find that partisanship in party-registration states is “shallower” (Burden and Greene 2000: 72) and that partisanship in these states is less predictive of vote choice.

However, the theory of *why* partisanship mobilizes voters provides another key observable implication that is more readily testable: partisans gain utility from expressing their partisanship, even in a private context where their expression has no bearing on electoral or policy outcomes (e.g., an anonymous ballot in a large election where one has a near-zero probability of casting a decisive vote). This is the claim I will directly test, to assess the validity of the mobilization hypothesis.

I test this key prediction with two original surveys conducted in Chile. The first survey estimates the prevalence of partisan duty and expressive motivations for voting, using a quota-based sample of 598 eligible voters in Chile collected in March and April 2019.²¹ The second survey, conducted with a sample of 431 Chilean partisans in August and September

make the coalition divide less stark.

¹⁹This estimate comes from the CEP surveys that were used to estimate the regression discontinuity. Over the same time period, LAPOP surveys estimated the rate of partisanship in Chile at 20%. However, Baker and Renno (2019) show that the question wording used in the LAPOP surveys consistently under-estimates the number of partisans across countries.

²⁰See Table A.1 in the appendix.

²¹Quotas for gender, age, education, and region were determined from the most recent census data. Because the survey was conducted online, some populations were harder to reach, and the sample skews younger and more educated than the Chilean population at large. See the online appendix for additional details on the sample demographics.

2019, uses a novel design with behavioral outcomes to probe the real-world relevance of expressive utility.

Prevalence of Expressive Motivations and Partisan Duty

In March and April of 2019, I surveyed 598 voting-eligible Chileans about their attitudes towards parties and voting. Respondents were asked about their partisan identification, the concept of partisan duty, and their reasons for voting.

Each respondent indicated the party with which they identified most strongly. The strength of that identification was then measured using the scale that Bankert, Huddy and Rosema (2017) developed for multi-party systems. The scale consists of eight questions, designed to measure social identification with a political party. Responses to the eight items were aggregated and scaled, to create an index measuring the strength of identification. The variable ranges from -1 to 1. A score of 0 means that the individual agreed with statements indicating social identification with a party as frequently as they disagreed with such statements. The sample distribution for this variable is illustrated in Fig. A.11 in the online appendix.

Respondents were also asked whether they felt a sense of partisan duty to help their party succeed. Partisan duty was measured through agreement with the following statement:

“As a supporter of [PARTY], I have a duty to turn out and vote in elections to help my party win.”

Respondents expressed widespread agreement with this statement. Figure 5 illustrates the rate of agreement across different levels of partisan identification. As the theory predicts, the rate at which respondents agreed with this statement increased with the strength of partisan identification. Among respondents with a partisan identification score above zero, 85% agreed with the partisan duty statement and only 2% disagreed (the remainder said they neither agreed nor disagreed).

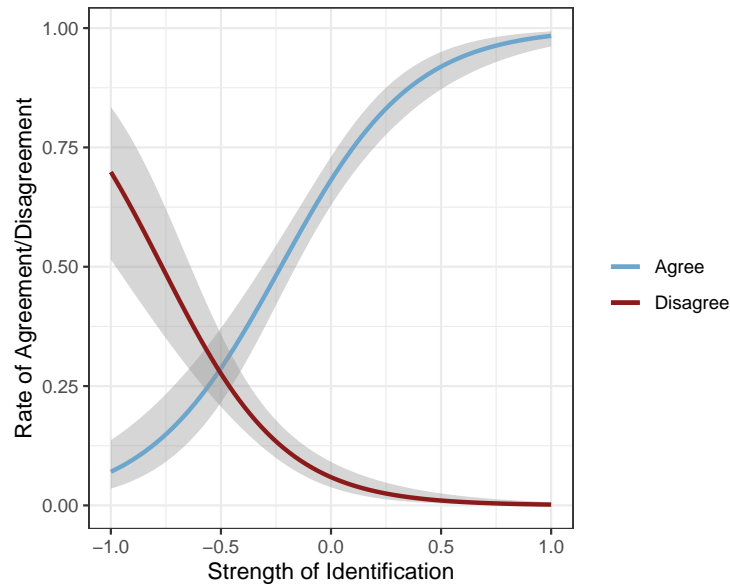
Respondents were also asked to indicate the primary reason why they vote (with the option to indicate that they do not vote, if applicable). Possible reasons included civic duty, partisan duty, expressive voting, social pressure, pivotality (“my vote could change the outcome”) and group pivotality (“together, my vote and the votes of people like me could change the outcome”).²²

Civic duty was the most commonly cited reason for voting. But among those who identify most strongly with political parties, both partisan duty and expressive voting rival civic duty in their importance. Figure 6 illustrates the relationship between strength of partisan identification and the importance of different reasons for voting. Twenty-four percent of respondents with partisan identification scores above zero cited either expressive voting or partisan duty as the *most important* reason they vote.

According to the survey evidence, partisan duty and expressive benefits are widespread

²²See the online appendix (Tab. A.6) for the complete wording of each option.

FIGURE 5. Partisan Duty



Note: Proportion of survey respondents who express a sense of partisan duty, by strength of party identification. The results were fitted with a logit regression, with 95% confidence intervals. The curves do not always sum to 1 because respondents could also select “neither agree nor disagree.”

and motivate turnout among partisans. But it is one thing for survey respondents to say that they value expression. It is another for voters to pay real costs in exchange for these expressive benefits. I now turn to evidence from a second survey, where I use a behavioral outcome to show that voters are willing to pay real costs to express their partisan identification, even when there are no electoral or policy outcomes at stake.

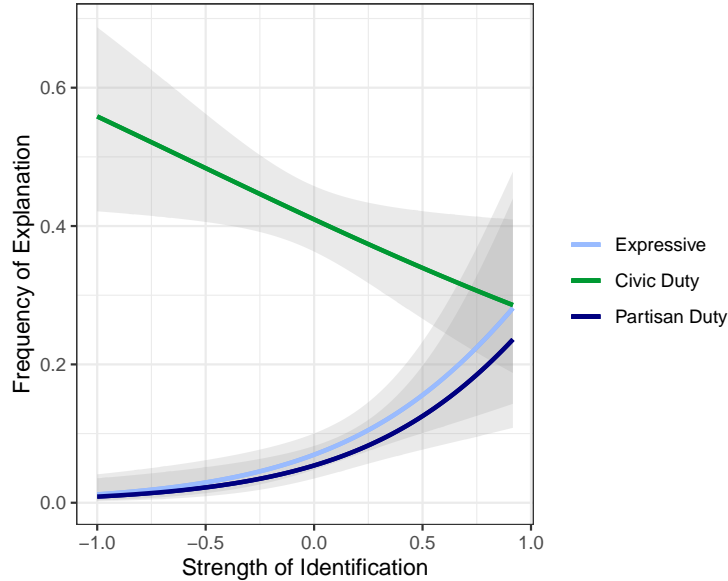
Measuring the Value of Expression

The second survey uses behavioral outcomes to more rigorously test whether partisans will pay material costs in the real world to obtain these expressive benefits. I measure respondents’ willingness to pay a cost, in terms of their time, to express their partisan identifications.

Participants were offered the choice between two surveys of differing lengths. They could either indicate which party they identified with, and complete a longer (10-minute) survey, or they could choose to skip the partisan identification question and complete a shorter (two-minute) survey. Participants were informed that regardless of which survey they completed, they would receive the same total payment. If they chose to express their partisan identification, they would have to devote five times as much of their time to earn the same payment they would receive from the shorter survey.

This choice of surveys was designed to impose a cost on expressing partisan identification. But we can’t simply assume that the longer survey is costly to all respondents. For

FIGURE 6. Primary Reason for Voting



Note: Proportion of survey respondents who indicated that the primary reason they vote is either civic duty, partisan duty, or expressive voting, plotted by strength of partisan identification. The results were fitted with a logit regression, with 95% confidence intervals.

many respondents, surveys are an opportunity to earn money, and they prefer to maximize their wage (money earned per time spent completing a survey). For these respondents, expressing their partisanship comes at the cost of an 80% wage cut for this survey. But others genuinely enjoy taking surveys and sharing their thoughts (Graham and Huber 2021). For these respondents, a longer survey is an opportunity to spend more time doing something they enjoy. Measuring the proportion of respondents who opt in to the longer survey would surely over-estimate the proportion who are willing to send a costly signal of their partisanship.

To mitigate this threat to inference, I use an experimental design. Participants were randomly assigned to one of two treatment groups. In the expressive treatment group, as described above, survey length is tied to the opportunity to express one's partisan identity. Participants may choose to express a preference for a party and complete a 10-minute follow-up survey, or choose not to express a preference for any party and complete a two-minute survey.

But in the baseline treatment group, survey length is the only differentiating factor between the two choices. Participants simply choose between a two-minute survey and a 10-minute survey; they do not indicate their partisan identification in either case. This baseline treatment allows us to estimate the proportion of the sample population that prefers longer versus shorter surveys. We can compare the proportion of participants who opt in to the longer survey in the expressive treatment group versus the baseline treatment group

to estimate the proportion who view the longer survey as costly but are willing to pay that cost to express their partisan identification.

The key to this design is that it maintains the opportunity for expressive benefits but removes potential instrumental benefits of voting in an actual election. As in a real election, participants have the opportunity to express their partisanship by anonymously checking a box next to their party’s name, and the cost is one’s time and cognitive effort. But in the experiment, there are no electoral or policy outcomes to influence, and participants don’t experience pressure to participate from their friends or family. Removing these other incentives isolates the potential expressive benefits and enables us to determine whether they add to the payoffs of voting.

Of course, spending time filling out a survey is by no means identical to the decision to turn out to vote. But by introducing real costs, this measurement strategy improves upon the common approach to testing theories about partisanship in surveys. Most often, we measure effects on turnout by simply asking respondents whether they would vote in an election — a costless response that is subject to well-known social desirability bias.²³ By implementing a behavioral outcome where indicating partisan preference is costly, we elicit more credible data about hypothetical behavior. This, in conjunction with direct questions about voting behavior and reasons for turning out, presents a compelling picture of the role that partisanship plays in mobilizing voters.

Design

The experiment was conducted in August–September 2019, using a Qualtrics panel of 431 respondents from Chile. Pre-treatment screening questions ensured that all participants were eligible to vote in Chile and identified with a political party (but did not give participants an opportunity to indicate which party). After completing the screening questions, each participant was randomly assigned to one of the two treatment groups (using simple randomization, with equal probability of assignment to each group). Members of each treatment group saw a prompt, asking them to select one of two surveys to complete. Each prompt is reproduced in Table 1.

The main outcome measure is survey selection: whether the respondent chose to complete a short survey or a long survey. This outcome variable is designed to mimic the kinds of costs people pay to turn out to vote. The costs of completing the longer survey are temporal and cognitive. So too are the most significant costs of voting: time to travel to the polls and wait in line, and time and cognitive effort to gather information and decide how to vote. And this outcome measure, as I show below, is a significant predictor of self-reported turnout.

The baseline treatment provides a reference point: it tells us how many people derive utility from filling out longer surveys. If we find that participants in the expressive treatment group are significantly more likely to opt in to the longer survey, this would be strong evidence of the internal incentives for expression: partisans want to reaffirm their identities, regardless

²³Direct questions about turnout in real elections are also used in the survey experiment to check the validity of the results.

TABLE 1. Experimental Conditions

Baseline

We are conducting a study about elections and political parties in Chile. You may opt in to this study in one of two ways.

1. You may select “Survey A” to complete a **10-minute** survey.
2. You may select “Survey B” to complete a **2-minute** survey.

If you complete either survey, you will receive the same payment, regardless of which survey you select.

Your responses will be completely anonymous.

Expressive

We are conducting a survey about elections and political parties in Chile. You can participate in this project in one of two ways.

1. You may select one of the parties listed below to indicate your support and complete a **10-minute** follow-up survey about political parties and elections in Chile.
2. You may select “choose not to support any party” and complete a **2-minute** survey about elections in Chile.

If you complete either survey, you will receive the same payment, regardless of which survey you select.

Your responses will be completely anonymous. The total number of people who express identification with each party will not be made public.

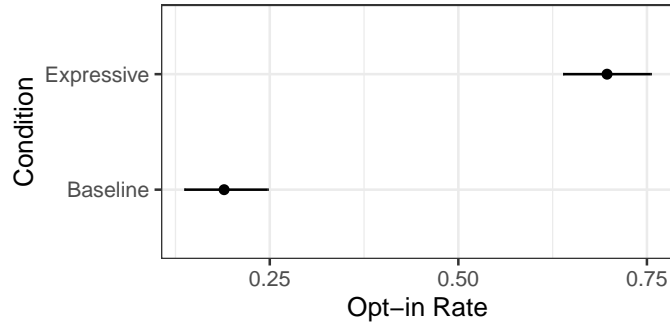
of who (if anyone) can see. And they will undertake costly actions to do so. By contrast, if voters are only motivated by instrumental motivations, we would not expect the expressive condition to have an effect.

Results

I use a difference-in-means calculation to test the hypothesis that partisans are willing to pay a cost to express their partisan identification. The outcome measure is a binary variable, indicating whether the participant opted in to the longer survey ($y = 1$) or chose to complete the shorter survey ($y = 0$). I compare the opt-in rate for the expressive treatment with the opt-in rate in the baseline group, and use randomization inference to test for statistical significance. Figure 7 presents the rate at which respondents in each group opted in to the longer survey.

The expressive treatment has a large and statistically significant effect: participants in the expressive treatment group were 51 percentage points more likely to opt in to the longer survey, compared with the baseline group ($p < 0.0001$). In other words, 51% of participants view the longer survey as a cost, but are willing to pay that cost to express their partisan identification.

FIGURE 7. Experimental Results



Note: Each point indicates the proportion of participants who opted in to the longer survey for a given treatment group. Each mean is presented with a 95% confidence interval. $n = 431$.

A series of additional tests further validate the substantive interpretation of these results, that partisans are motivated to turn out to vote by expressive benefits. The following results compare “expressive respondents” with “non-expressive respondents.” Expressive respondents are those that are willing to pay the cost of completing the longer survey to express their partisan identification. Non-expressive respondents are unwilling to pay the cost to express their identification. A third type of respondent exists: one who prefers long surveys and therefore does not perceive a cost to expressing their identification in the expressive treatments. We do not know how willing this type of respondent is to pay a cost to express their identification, so I exclude them from the comparison. We can never identify the exact set of individuals for all three types because we do not observe the full set of potential outcomes. But the experimental design allows us to estimate the group means for each respondent type (see the online appendix for details on the procedures for identifying these group means).

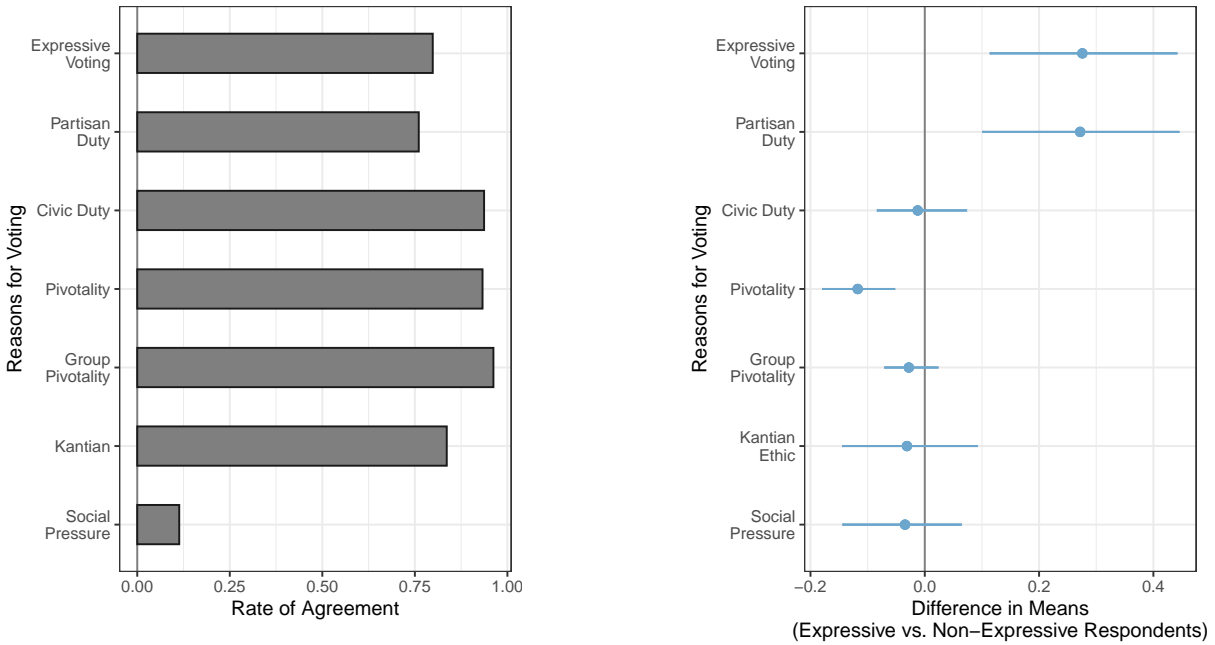
Expressive respondents are more likely to report that they always vote in elections, compared to non-expressive respondents. Prior to treatment, respondents were asked about their voting histories. Expressive respondents were 22 percentage points more likely to report always voting than non-expressive respondents ($p < 0.05$).

Expressive respondents are also more likely to report feeling a sense of partisan duty and voting for expressive purposes. Respondents were given a series of possible reasons for voting and asked to indicate the extent to which they agreed or disagreed with each statement.²⁴ These reasons included expressive voting and partisan duty, as well as civic duty, pivotality (“my vote could change the electoral outcome”), group pivotality (“together, my vote and the votes of people like me could change the electoral outcome”), a Kantian ethic (“I vote because it is what I would want others like me to do”), and social pressure.

The results are presented in Figure 8. The left-hand graph presents the overall rate

²⁴See the online appendix for the full text of each reason (Table A.2).

FIGURE 8. Reasons for Voting



Note: Both graphs present data from participants who report that they vote at least occasionally ($n=431$). The left-hand graph illustrates the rate at which participants agree with each reason for voting (in reference to their own decision to vote). The right-hand graph illustrates the difference in means for each reason, comparing those who are willing to pay the cost of completing a longer survey to express their identification, with those who are unwilling to pay the cost (with 95% confidence intervals).

of agreement with each statement. The right-hand graph illustrates the differences between expressive and non-expressive respondents. Expressive respondents are 27 percentage points more likely to report voting out of a sense of partisan duty and 28 points more likely to report voting for expressive benefits. Non-expressive respondents are 12 points more likely to say that they vote because their vote might change the outcome of the election. In all other categories, expressive and non-expressive respondents are indistinguishable. This pattern of results conforms to expectations: those who are willing to pay a real cost to express their party identification in a survey are also more likely to see expressive utility as a reason to vote, and to feel a sense of duty to support their party. Their decision to turn out is less likely to hinge on the perception that their one vote might change the outcome.

In sum, many people who identify with a political party are motivated by expressive utility. The majority of participants in the experiment are “expressive respondents”: they are willing to pay a cost to express their partisan identification. As predicted, these expressive respondents are significantly more likely to indicate that partisan duty and expressive utility compel them to participate in elections. They also report voting more frequently than non-expressive respondents, a result that highlights the real-world importance of expressive utility for mobilizing voters.

Do the Results Travel? Evidence from the US

Chile is a useful case for studying the causal relationship between partisanship and turnout. Its unique electoral institutions and history allow for a rare, well-identified test of the identity formation hypothesis. But any single-case study must grapple with the possibility that its findings are specific to that case alone. More broadly, any characteristic of the Chilean electoral system might be an unknown scope condition. For example, if voting fosters partisanship through a cognitive dissonance mechanism, as opposed to a political learning mechanism, the multi-party system in Chile might dampen the effect compared to a two-party system. In a multi-party system, voters might not always vote for their most-preferred party, and instead cast strategic ballots. Or, the widespread negative attitudes towards parties in Chile might work against the formation of partisan identities among those who are politically active.

To allay these concerns, I analyze panel data from the US. The US was selected because it differs from Chile in important ways: partisanship is more widespread in the US; fewer Americans than Chileans distrust political parties; and the US has a two-party system, rather than a large multi-party system. If we find evidence that voting fails to encourage partisanship in the US, as it also failed to do in Chile, this would allay concerns that the Chilean results might be particular to a low-partisanship electorate. The US data do not allow us to discern the extent to which partisanship might mobilize American citizens to go to the polls. But they do provide an estimate of how much more likely partisans are to turn out (compared with non-partisans), and to examine whether in the US, as in Chile, voting fails to foster partisanship.

The Cooperative Congressional Election Study (CCES) conducted pre- and post-election surveys in every even-numbered year from 2006 to 2018.²⁵ Respondents were asked whether they identified with a political party in both the pre- and post-election wave. In addition to collecting pre- and post-election identification, CCES has the benefit of providing validated turnout data. Respondents were matched with official voter files to verify whether or not they actually voted in that year’s election.

I present two results from each survey. First, I regress post-election partisanship (using a binary measure)²⁶ on validated turnout:

$$\text{PostElectionPID}_i = \beta_0 + \beta_1 \text{Vote}_i + \epsilon_i \quad (2)$$

The coefficient β_1 is simply the correlation between partisanship and turnout that we would observe in cross-sectional studies, and gives us a frame of reference for the main result. I then run the same regression, but I control for partisan identification in the pre-election survey:

$$\text{PostElectionPID}_i = \beta_0 + \beta_1 \text{Vote}_i + \beta_2 \text{PreElectionPID}_i + \epsilon_i \quad (3)$$

²⁵The pre-election surveys were conducted from late September to late October. The post-election surveys occurred in November.

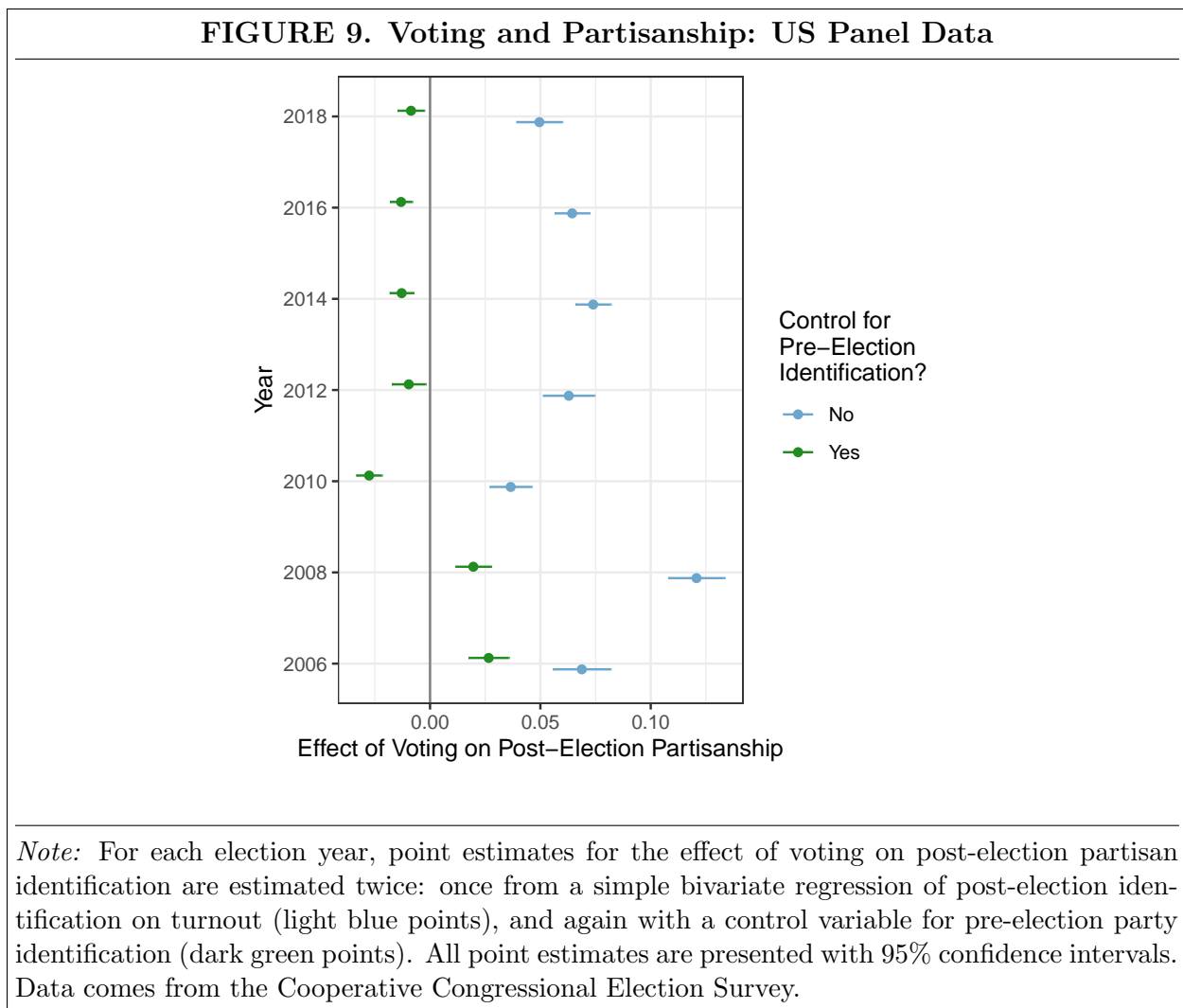
²⁶Respondents were asked, “Generally, do you think of yourself as a Democrat, Republican, Independent, or Other?” Respondents who indicated any party were classified as partisans; respondents who declared themselves “independents” or stated they were “not sure” were classified as non-partisans. Anyone who did not respond was excluded from the analysis.

In this equation, β_1 estimates the extent to which participation in an election generated new partisanship that did not exist prior to the election.

If the results from Chile travel to the US, β_1 will be a large positive number in Eq. 2, but it will be close to zero in Eq. 3. These results would indicate that partisanship and turnout are closely related, but that turning out did not generate partisanship.

Results

The results from the CCES data are presented in Figure 9. When we do not control for pre-election partisan identification, we observe that voters are, on average, 7 percentage points more likely to identify with a party after the election than non-voters. But controlling for pre-election partisanship, voters were actually 0.4 percentage points *less* likely to identify with a party than those who abstained.



If voting had a causal effect on partisan identification, we would expect a consistent increase in partisanship among voters, after controlling for pre-election identification. Al-

though post-election partisanship is consistently correlated with turnout across all seven election years, this effect generally disappears once we control for pre-election partisanship. In 2006 and 2008, there is a small positive effect. In the next five election years (2010–2018), the effect turns slightly negative. The small magnitude and inconsistent direction of the effect casts serious doubt on the theory that voting makes people more likely to identify with parties in the United States.

This doubt is amplified by the space for upward bias that the panel design introduces. The panel design eliminates many, but not all, confounders. There are still short-term events that may simultaneously mobilize voters and generate a sense of partisan identification. Party workers go door to door ahead of election day, urging potential supporters to get out to vote while appealing to their connections to the candidate’s party. And the converse can also occur: unfavorable news stories about one’s preferred party may leave supporters disenchanted, questioning their identification with the party and less motivated to vote. Any such event that occurs between the two survey waves would create the appearance of a causal effect.

Nonetheless, we observe no consistent effect of voting on partisanship. Even in the US, a country in which partisanship is more widespread and trust in parties is higher than in Chile, electoral participation does not increase one’s likelihood of adopting a partisan identity.

Conclusions

Partisanship is often discussed in negative terms. Theoretical and empirical studies tell us that it dampens electoral accountability (Eggers 2014), takes precedence over democratic values (Svolik 2017), enables corruption (Anduiza, Gallego and Muñoz 2013), generates discrimination against non-copartisans (Iyengar and Westwood 2015), and creates biases in processing factual information (Jerit and Barabas 2012).²⁷

But the effects of partisanship are not all bad. Strong partisan attachments serve as insurance for parties against short-term retrospective punishments (Lupu 2016). This insurance can enable parties to enact policies that are unpopular but necessary, or painful in the short-term but beneficial in the long-term. This study points to an additional, positive effect of partisanship: it encourages people to turn out to vote.

Prior research laid the foundation for two distinct causal interpretations of the relationship between partisanship and turnout. In the empirical tests presented in this paper, I show that only one of these theories holds up. The experience of voting does not make one more likely to identify as a partisan. But partisanship mobilizes people who might otherwise abstain by intensifying the expressive benefits to voting.

In light of the extensive theorizing that underlies the identity-formation hypothesis, why did voting fail to foster partisan identities? The explanation might lie in the distinction between attitudes and identities. Prior political studies have shown that cognitive dissonance is important to understanding attitude development: voting appears to affect approval ratings,

²⁷But see Bullock et al. (2015) for a strong critique of the evidence on partisan bias in factual beliefs.

attitudes about democratic norms, and interest in politics (Mullainathan and Washington 2009; Quintelier and Van Deth 2014). But partisanship is not merely a positive attitude towards a party — it often forms a meaningful social identity. Although one’s own voting experience does influence future attitudes and beliefs, the evidence presented here suggests that it does not hold the same power over identity formation.

These findings clearly contribute to our understanding of political behavior. But this causal distinction — whether (1) partisanship mobilizes voters or (2) the act of voting creates partisans — also has important implications for party strategy. Researchers have argued that parties have an incentive to maintain strong party brands: widespread partisan identification insulates parties from negative short-term retrospective voting, which could lead to party collapse (Lupu 2016). The argument highlights the risk that voters with weak party preferences may switch their vote choice. In this paper, I show that the strategic benefits of strong partisanship extend even further. Partisanship generates additional motivations for voters who prefer a given party’s policies to actually show up to the polls. Thus, strong partisan identification protects parties from the risk of abstention-based collapse as well.

This study also suggests new substantive questions worth exploring. Given the role of partisanship in mobilizing voters, what are the implications for party strategy? What does it imply about party strategy that partisan citizens are considerably more likely to turn out? When voting is compulsory, will parties invest fewer resources in cultivating strong partisan identities among their supporters? After all, a turnout boost is one of the major payoffs a party gets from cultivating partisanship. But parties operating under well-enforced compulsory voting laws don’t need to worry about their supporters staying home. In the absence of concerns about turnout, a party might optimize their electoral prospects by allocating more resources toward convincing independent or unaffiliated voters to support their platform, rather than building a sense of genuine social identification among existing supporters. Studies exploring the implications for party strategy can help us understand the second-order effects of institutional changes, such as the implementation of compulsory voting.

Future research might also test the identity-formation hypothesis in other contexts and elections. Although voting does not seem to have the long-term effect that would explain higher rates of partisanship in compulsory voting systems, it might shape partisanship in the short term in specific contexts. In founding elections, where party systems are new, or among young voters who are just starting to learn about their position in the political system, voting might jump-start the formation of partisan identities. Further testing will be necessary to discern whether this is the case.

Further methodological research will also help contextualize some of the results presented here. I introduce a novel survey-experimental design to establish the mobilizing power of partisanship. Implementing a design that differs substantially from prior studies yields new and valuable data, but it also generates new questions about the properties of the data. How exactly does the survey-participation cost translate to the cost of voting? The costs encountered in the survey experiment (time, cognitive effort) are similar in nature to the costs voters often encounter to cast a ballot; yet voting occupies a unique role in democracy that differentiates it from other forms of political participation (Chapman 2019).

The survey experiment showed that partisans will pay material costs to express their partisanship; but a precise understanding of how these costs translate to voting — both in nature and in magnitude — calls for further examination.

This study also calls attention to remaining gaps in our understanding of expressive voting. The experiment reported here indicates that expressive voting is an empirically verifiable phenomenon. But what kinds of voters place greater value on these expressive benefits? In what contexts is the expressive value of voting more or less important? Future studies probing these questions will help us to better understand how expressive voting factors into the broader calculus of voting across contexts.

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Supporting Information for “Cause, Not Effect: Partisan Identification and Voter Turnout”

August 28, 2021

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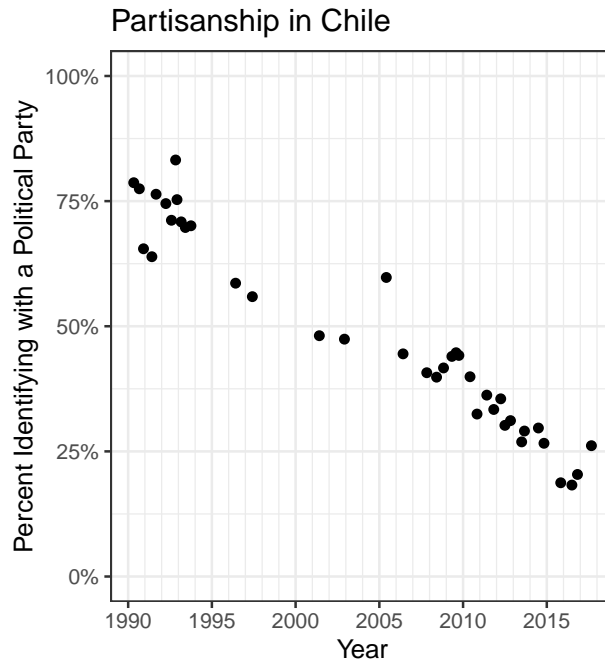
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FIGURE A.1. Partisanship in Chile



Note: Points indicate the proportion of respondents identifying with any political party in regular public opinion polls conducted by CEP (Centro de Estudios Públicos).

TABLE A.1. Partisanship and Turnout

	<i>Dependent variable:</i>	
	Voted in 2013 Election	
	<i>OLS</i>	<i>logistic</i>
	(1)	(2)
Party ID	0.175*** (0.028)	0.889*** (0.149)
Constant	0.632 (0.015)	0.539 (0.066)
Observations	1,350	1,350

Note: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$
Data: CEP November 2014 (self-reported turnout)

1 Survey Experiment

Participants in the survey experiment were recruited by Qualtrics. Surveys were conducted between August 23 and September 5, 2019. Compensation for all respondents was based on the rate for a survey of 10–15 minutes, even if they selected the 2-minute survey. Pilot results revealed that many survey respondents preferred to complete a longer survey, even in the baseline condition. While the design accounts for this (by including the baseline condition and measuring the outcome as a difference in means), I added an additional screening question to sort respondents into two groups (those who prefer long surveys and those who prefer short surveys) for the sake of optimizing the experiment’s power. The only respondents included in the experiment analyzed in this paper are those who (1) indicated that they identify with a political party and (2) indicated that, all else equal, they prefer shorter surveys.

Survey length preference was measured with the following question:

Suppose that you had the option to choose which survey to complete: a 2-minute survey or a 10-minute survey. You would receive the same payment for either survey. Which would you prefer to complete?

This question was posed at the beginning of the survey, and participants responded to a series of 4–6 demographic and political questions after this question but before treatment (to distance this question from the treatment). Among those who qualified on all other measures, 52% (635 of 1214) indicated that they prefer shorter surveys and were therefore included in the experiment. Among those 635, a total of 431 (selected at random) were included in this experiment (the other 204 were screened into a separate survey that is not part of this paper).

Supplemental Analysis

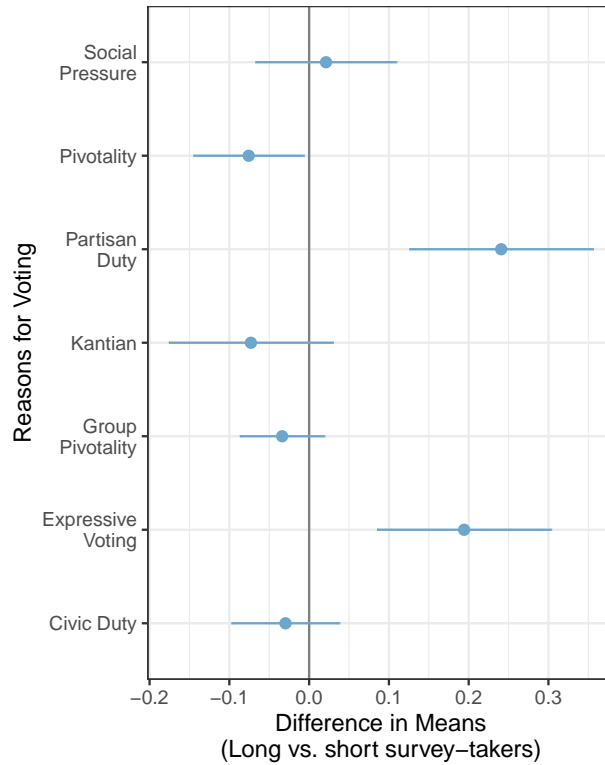
In the supplemental analysis, I compare covariates among expressive versus non-expressive respondents. Expressive respondents are those who perceive the long survey as a cost, but are willing to pay that cost to express their PID. Non-expressive respondents are those who perceive the long survey as a cost, and are unwilling to pay that cost to express their PID. These groups are defined in terms of potential outcomes. Expressive respondents will select the long survey in an expressive treatment ($y(1) = 1$) and the short survey in the baseline treatment ($y(0) = 0$). Non-expressive respondents will always select the short survey, regardless of treatment ($y(0) = 0$ and $y(1) = 0$). A third group exists: those who do not perceive the long survey as a cost ($y(0) = 1$ and $y(1) = 1$). We will call these types “survey lovers.”

I assume that we do not have “defiers”: respondents with $y(1) = 0$ and $y(0) = 1$. This would be an individual who prefers the long survey. In the expressive conditions, they are given the choice between completing their preferred survey (long) while getting the opportunity to express their identification, or completing their less-preferred survey (short) and not having the opportunity to express their identification. This individual would choose the latter option: completing their less-preferred survey and forgoing the opportunity to express their identification. I assume that respondents are not of this type. All respondents were screened to ensure that they identify with a political party prior to treatment. If a respondent did not want to share her party identification, she would likely respond “no” when asked if she identifies with a party (as “which party” is the natural follow-up question). Moreover, respondents could always select the “other” option if they really wished to avoid sharing the particular party with which they identify.

We never observe both potential outcomes for any individual, but we can estimate the group means for each of the three types (assuming we have no defiers). In the baseline condition, all respondents who select the long survey ($y(0) = 1$) are survey lovers. In the expressive condition, all respondents who select the short survey ($y(1) = 0$) are non-expressive respondents. The remaining groups that we observe empirically provide weighted averages for two types: the $y(0) = 0$ group is a mix of expressive respondents and non-expressive respondents; the $y(1) = 1$ group is a mix of expressive respondents and survey lovers. The $y(1) = 0$ and $y(0) = 1$ groups provide estimates of the sample proportions of non-expressive respondents and survey lovers, as well as group means for any covariate. Using these estimates, we can algebraically solve for the proportion of expressive respondents, and the group mean for any covariate among expressive respondents.

In the absence of the no-defiers assumption, these quantities are unidentifiable. We could, alternatively, compare those who opt in to the long survey versus those who select the short survey, within the expressive treatment. While this does not require the no-defiers assumption, it also sacrifices the main benefit of the experimental design: it simply compares respondents who opt to express their identification and complete the long survey with those who refrain from expressing their partisan identification and complete the short survey. But it does not distinguish between those who express their identification at a cost (expressive respondents) and those who enjoy long surveys. This approach just allows us to measure how

FIGURE A.2. Reasons for Voting



Note: Estimates come from bivariate regressions (reason for voting regressed on survey selection).

well the outcome measure (completing a long survey) correlates with other characteristics, such as self-reported turnout. I present the results for these calculations below, which are broadly consistent with the previously computed differences between expressive and non-expressive respondents.

With respect to self-reported turnout, those who opt in to the longer survey (in the expressive treatment) are 20 points more likely to report always voting ($p < 0.01$). Figure A.2 illustrates the difference in means estimates for each reason for voting, with 95% confidence intervals.

TABLE A.2. Reasons for Voting (Experiment)

Label	Reason
Expressive Voting	To express my support for my party
Partisan Duty	To contribute to my party's electoral success
Civic Duty	To fulfill my civic duty
Pivotality	My vote could change the outcome of the election
Group Pivotality	Together, my vote and the votes of people like me could change the outcome of the election
Social Pressure	If I didn't vote, people would judge me

Note: Respondents were asked to indicate the extent to which they agreed/disagreed with each reason when thinking about their own decision to vote. The sample was limited to those who indicated that they vote at least occasionally.

2 Regression Discontinuity

The regression discontinuity was estimated using survey data from the Centro de Estudios Públicos (CEP) survey project. Table A.3 lists the survey waves that were used in the RD calculations, along with the dates of data collection and the number of observations. Table A.4 lists the date and type of each election held after the 1988 plebiscite (through the 2010 election). Each respondent was surveyed after 12–15 post-plebiscite elections had occurred (with an average of 13.3 elections).

Wave	Dates	Observations
52	Jun–Jul 2006	1417
54	Dec 2006	1438
55	Jun 2007	1426
56	Nov–Dec 2007	1397
58	Nov–Dec 2008	1417
59	May–Jun 2009	1069
60	Aug 2009	1438
61	Oct 2009	1428
62	Jun–Jul 2010	1417
63	Nov–Dec 2010	1322
64	Jun–Jul 2011	1446
65	Nov–Dec 2011	1473
Total		16,688

A McCrary sorting test revealed no apparent sorting of the running variable (recorded birth date) around the cutpoint for the RD. The test was conducted using the `DCdensity` function from the `rdd` package in R. (See Fig. A.3.) The test yielded $\theta = -0.046$, $\sigma = .051$, $p = 0.372$.

TABLE A.4. Elections (post-plebiscite)

Date	Type
Dec 14, 1989	Presidential
Jun 28, 1992	Municipal
Dec 11, 1993	Presidential
Oct 27, 1996	Municipal
Dec 11, 1997	Parliamentary
Dec 12, 1999	Presidential (first round)
Jan 16, 2000	Presidential (second round)
Oct 29, 2000	Municipal
Dec 16, 2001	Parliamentary
Oct 31, 2004	Municipal
Dec 11, 2005	Presidential (first round)
Jan 15, 2006	Presidential (second round)
Oct 26, 2008	Municipal
Dec 13, 2009	Presidential (first round)
Jan 17, 2010	Presidential (second round)

2.1 Estimating Compliance

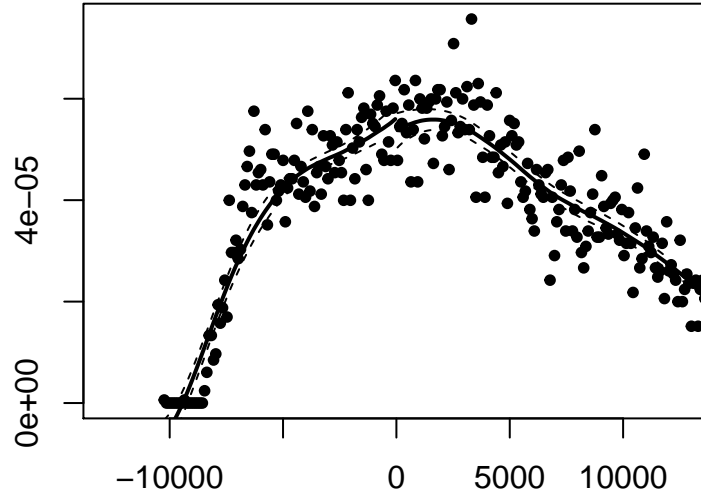
Using the self-reported registration data, we can estimate the proportion of compliers in the sample. But we know that respondents over-report electoral participation, due to social desirability bias. Thus, Figure A.4 presents three separate estimates of the discontinuity in registration rates. The first graph simply uses the self-reported registration status of respondents. The second and third graphs, however, use official registration numbers to estimate over-reporting and adjust the data accordingly.

When adjusting for over-reporting, I limit the sample to surveys conducted after the 2009-2010 election ($n=5511$). We know the actual registration rate among the entire population in 2010, and among those eligible for the plebiscite in 1988. We also know the proportion who were eligible for the plebiscite. Using these three pieces of information, we can calculate the over-reporting rate among citizens eligible for the plebiscite, and among citizens ineligible for the plebiscite.

Ninety-four percent of those eligible for the plebiscite report that they were registered to vote in the 2010 election, but only 92% of the population registered for the plebiscite. I assume that if someone was eligible for the plebiscite and chose not to register in 1988, then they did not register in later years. Previous empirical studies of registration in Chile have shown that this is a reasonable assumption (Corvalan and Cox 2013). It is also a conservative assumption: it uses the lower-bound on registration within the treatment group (where the registration rate within the treatment group represents the sum of compliers and always-takers).

Across the full sample, 74% of respondents report registering to vote, but overall registration for the 2009–2010 elections was only 68%. Sixty-two percent of respondents were

FIGURE A.3. McCrary Sorting Test



Note: The x-axis indicates the number of days before or after the registration cutoff that an individual was born.

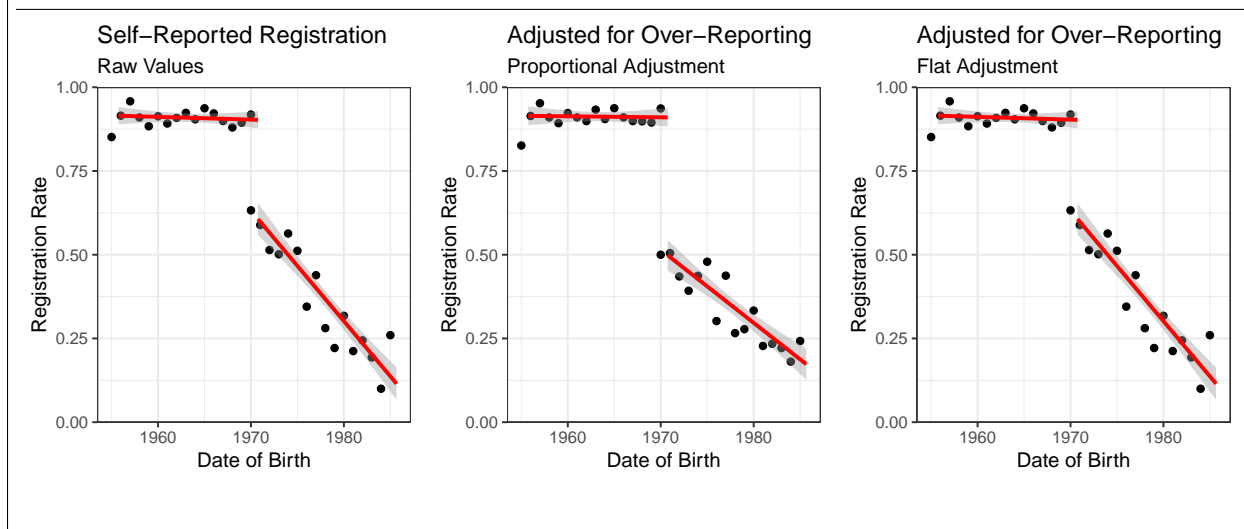
eligible for the plebiscite. If plebiscite-eligible voters registered at a rate of 92%, then plebiscite-ineligible voters must have registered at a rate of 29% to yield an aggregate registration rate of 68%. Those who were ineligible for the plebiscite report that they registered at a much higher rate: 42%.

We can adjust the self-reported registration rates to account for this over-reporting in either of two ways. First, we can apply a proportional adjustment. If 42% of plebiscite-ineligible citizens report registering to vote when only 29% did, then there is a 31% chance that any particular self-reported registrant is actually un-registered. So to adjust the registration rates, I randomly assign 31% of self-reported registrants (in the plebiscite-ineligible group) to un-registered status. Similarly, 2.1% of self-reported registrants in the plebiscite-eligible group are assigned to un-registered status ($0.02/0.94$).

Second, we can apply a flat adjustment: we take the trend line from the self-reported registration data and simply shift it down by the over-reporting rate. For plebiscite-eligible voters, this rate is 2% ($0.94-0.92$). For plebiscite-ineligible voters, this rate is 13% ($0.42-0.29$).

Using the raw, unadjusted numbers, we get the most conservative estimate of the discon-

FIGURE A.4. Discontinuity in Registration



Note: Effect of plebiscite eligibility on registration for the 2009–2010 presidential election. Surveys were conducted from June 2010–December 2011 ($n=5511$). The left-most graph presents self-reported registration data, with no adjustments. The other graphs adjust the self-reported registration rates, using official data to estimate social desirability bias.

tinuity: approximately 16 percentage points. Applying the proportional adjustment yields an estimated effect of 40 percentage points. And applying the flat adjustment yields an estimated effect of 28 percentage points. Recall that these figures are estimates of the proportion of compliers in the sample—those citizens who would register to vote if eligible for the plebiscite, but would not register otherwise.

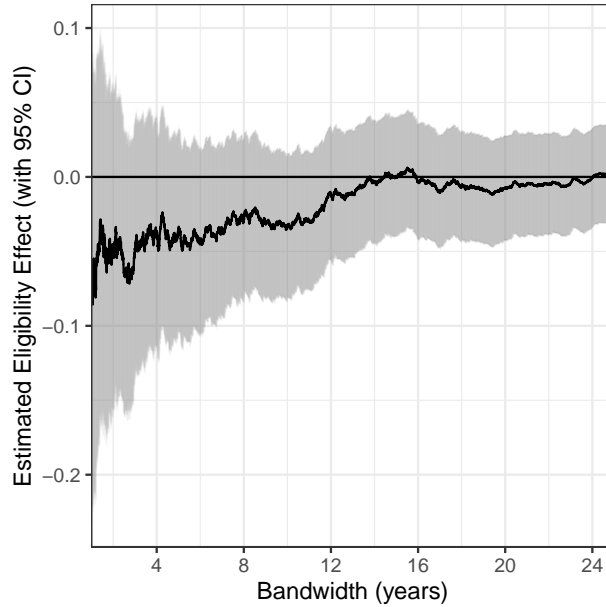
2.2 Additional Robustness Tests

Figures A.5 and A.6 replicate the main analysis (Fig. 4) with alternative specifications. Figure A.5 uses the same model as the main analysis, but introduces fixed effects for the survey wave. Figure A.6 uses the same model as the main analysis, but uses identification with a coalition as the dependent variable (in place of identification with a party).

Figure A.7 illustrates the difference in means for all bandwidths up to 8 years. Across all of these bandwidths, we never observe a positive treatment effect. When we use the difference-in-means comparison, rather than the RD setup, we must make assumptions about similarity in relevant covariates across the sample. Treatment assignment (a birthdate before or after the plebiscite cutoff) must be orthogonal to other factors that affect partisan identification. Within treatment groups, we do not observe any significant relationship between age and partisanship (see Fig. 3). Nonetheless, the necessary assumptions become stronger as the bandwidth increases, so I focus on smaller bandwidths here than the RD optimal bandwidths.

At the smallest bandwidths, we observe noisy estimates due to small sample sizes, with a handful of large negative (generally insignificant) effects. Of the 2917 bandwidths tested,

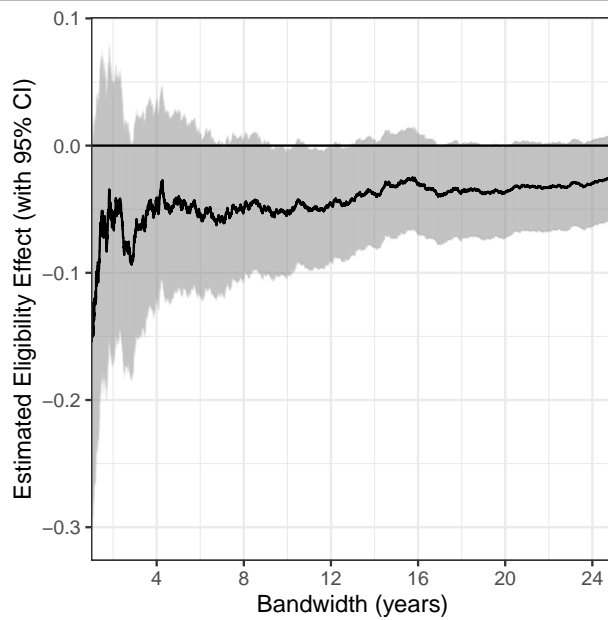
FIGURE A.5. RD Coefficient Plot: Survey Wave Control



Note: Coefficient plot for each estimate of Eq. 1, using bandwidths from 1 to 25 years (increasing in intervals of one day). For each bandwidth, I plot the point estimate of β_1 with a 95% confidence interval. In this model, I control for survey wave (using a dummy for each different survey wave).

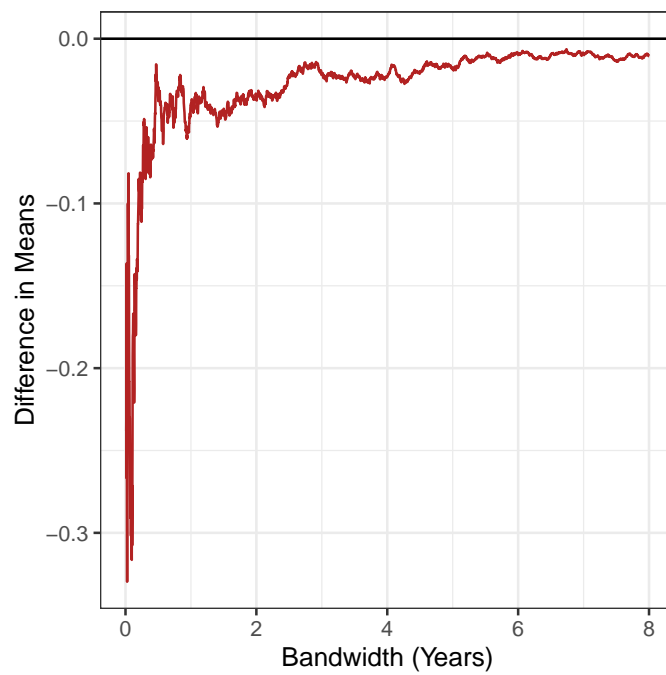
16 produce statistically significant effects (all negative). These occur at bandwidths from 27 to 49 days (with 51 to 102 observations). The effect quickly trends towards zero as the sample size increases. This test is well-powered to detect ITT effects smaller than 0.04 at about a 5-year bandwidth (see Fig. A.9 for power estimates across bandwidths).

FIGURE A.6. RD Coefficient Plot: Coalition Identification



Note: Coefficient plot for each estimate of Eq. 1, using bandwidths from 1 to 25 years (increasing in intervals of one day). For each bandwidth, I plot the point estimate of β_1 with a 95% confidence interval. In this model, I use coalition identification as the dependent variable (instead of party identification).

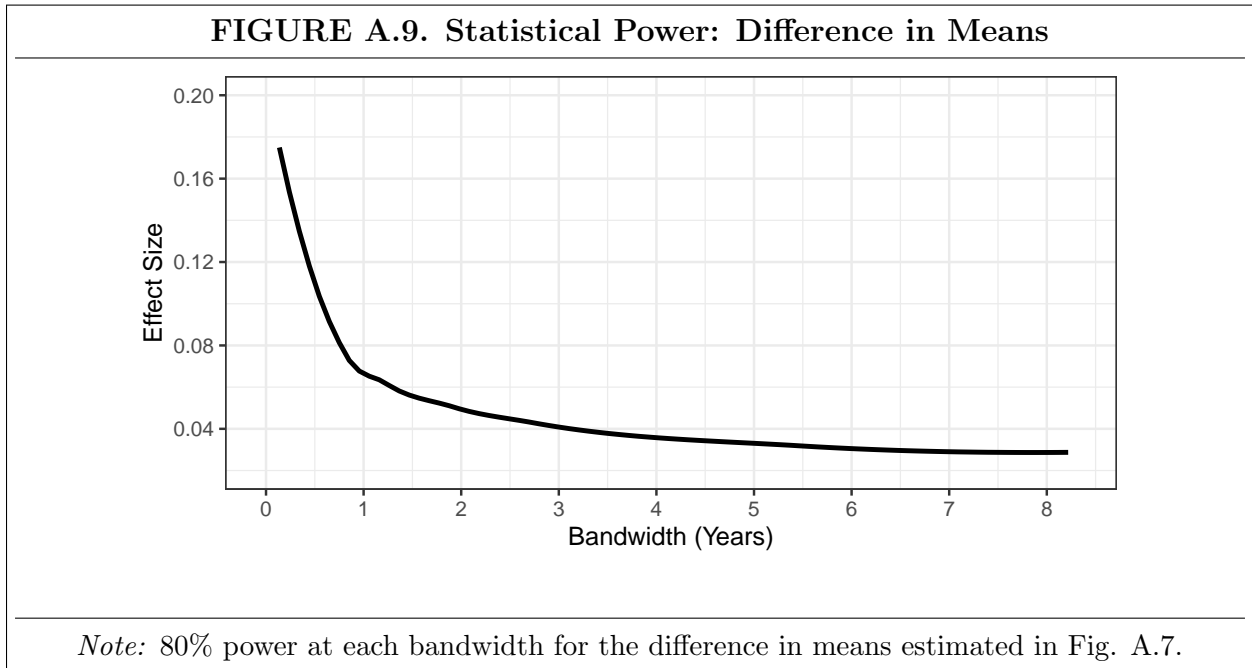
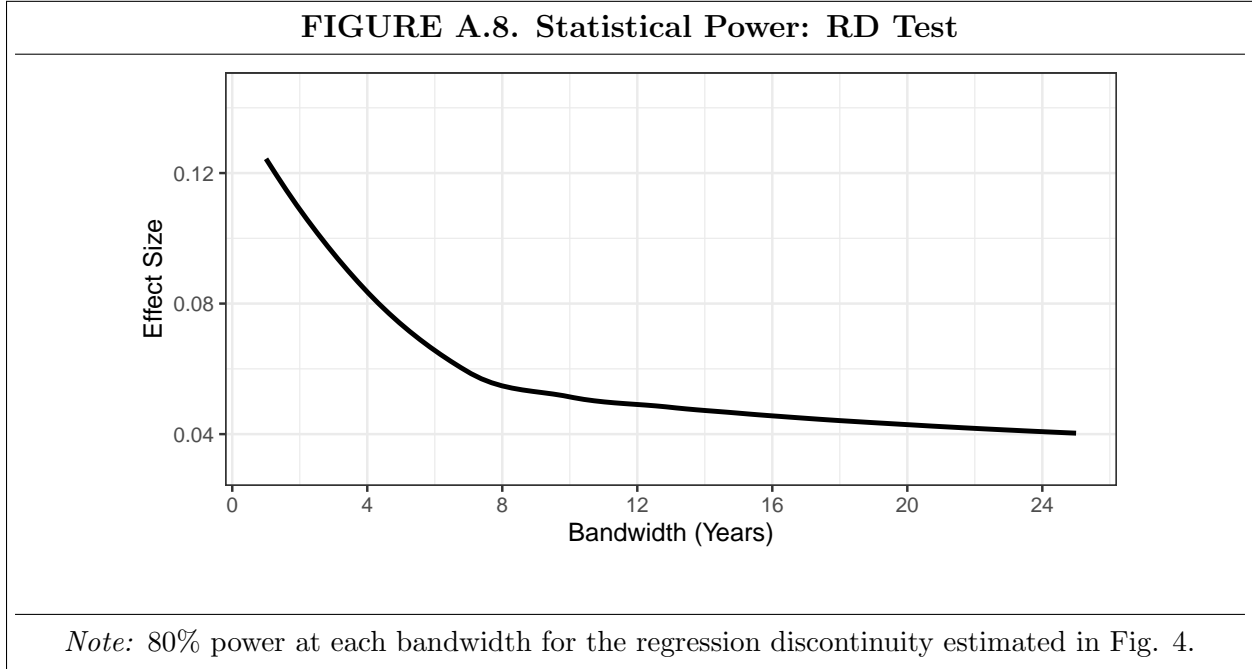
FIGURE A.7. Effect of Eligibility on PID



Note: Effect of plebiscite eligibility on partisan identification using the raw difference in means. All possible bandwidths (increasing by one-day intervals) up to 8 years are presented here.

2.3 Power Analysis

Figures A.8 and A.9 illustrate the statistical power of the regression discontinuity test using local linear regression and the difference in means. The curve plots the minimum β_1 effect size that the test can detect at 80% power for each bandwidth ($p < 0.05$, one-tailed tests). Each curve was generated through simulations.



Recall that β_1 (the effect size referenced in these power calculations) is the ITT, measuring the effect of plebiscite eligibility on partisanship. The LATE interpretation of β_1

— the effect of *voting* on partisanship — depends on assumptions about the model of the cumulative effects of voting in multiple elections. The LATE interpretation presented in the text assumes a simple model where a non-partisan has a certain probability of adopting a partisan identification each time she votes as a non-partisan. In other words, when she votes in her first election, she adopts a partisan identity with probability π . If she adopts a partisan identity after the first election, she remains a partisan after voting in the second election. If she does not adopt a partisan identity after the first election, she again has probability π of adopting a partisan identity once she votes in the second election. Thus, after voting in two elections, her probability of being a partisan is $\pi + \pi(1 - \pi)$. Alternative models might consider a waning effect: perhaps if a voter participates in many elections without developing a partisan identity, she becomes very unlikely to develop one from voting in future elections; or a more cumulative process: perhaps voting in a single election rarely leads to partisanship, but voting in three elections has a big effect, with the repetition of experience generating a sort of tipping point. Any such alternative model will affect the LATE interpretation of these ITT power calculations.

3 Observational Survey

The survey was conducted online from March 21–April 10, 2019 on a sample collected through Qualtrics. The sample included 598 total respondents.

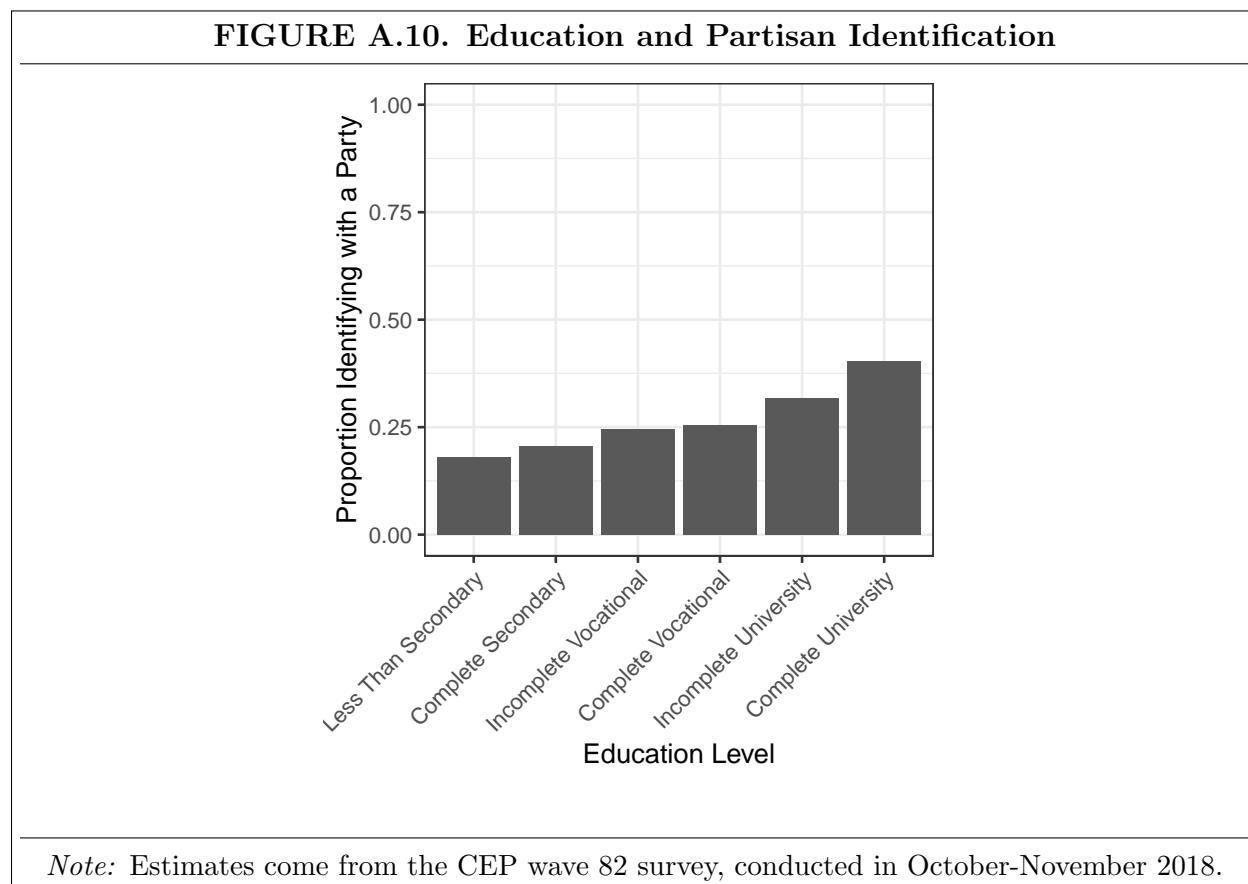
Initially, I determined quotas for age, education, gender, and region, using the most recent census data. Partway through fielding the survey, Qualtrics determined that they did not have sufficient access to older respondents and respondents with low education. We relaxed and eventually removed the quotas to fill the sample, which skews younger and more educated than the Chilean population at large. Table A.5 presents detailed information on the sample demographics in comparison to the census data.

Category	Census data	Sample count	Sample proportion
Age			
18–24	14.25%	105	18%
25–34	20.79%	163	27%
35–44	18.07%	133	22%
45–54	17.62%	115	19%
55–64	14.23%	47	8%
65+	15.05%	35	6%
Gender			
Male	48.95%	251	42%
Female	51.05%	347	58%
Education			
Less than secondary	37.78%	10	2%
Complete secondary	31.97%	89	15%
Incomplete vocational	1.58%	52	9%
Complete vocational	7.48%	116	19%
Incomplete university	6.00%	99	17%
Complete university	15.18%	232	39%

With any online survey, it is not possible to collect a traditional probability sample of the general population. We cannot state with certainty how the survey population differs from the population at large, because there may be other unobserved characteristics on which the survey respondents differ from the broader population. However, using well-studied characteristics like age and education, we can draw inferences about how the sample might differ.

As illustrated with the CEP data used in the regression discontinuity, age does not bear any strong relationship to partisanship in Chile. It does correlate with voting experience and propensity to turn out to vote (now that voting is voluntary), with younger people being less likely to turn out to vote. A younger sample, then, likely has a lower electoral participation rate than the population average.

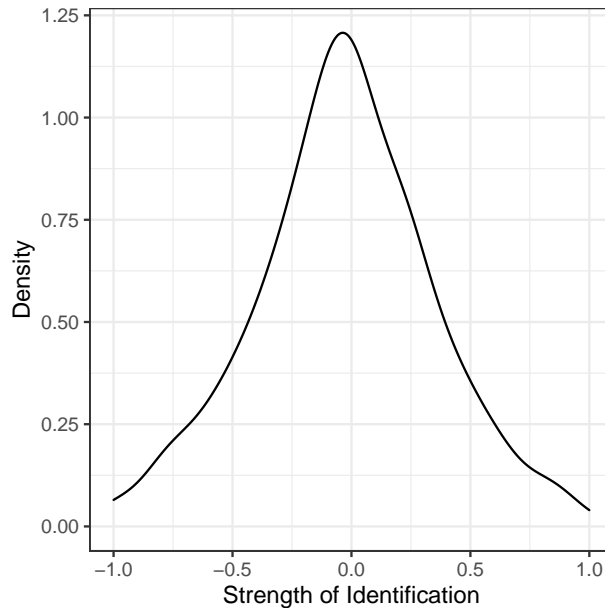
The education skew pushes in the opposite direction on participation: turnout rates rise with education (see, e.g., Corvalan and Cox 2013). Education is also positively correlated with partisan identification. Figure A.10 illustrates the relationship between education and partisanship according to wave 82 of the CEP surveys, conducted in October and November 2018.



In line with these patterns, respondents indicate higher than average rates of partisanship: 55% of respondents in my survey indicated a party identification in the first question (that is, the standard question asking which party the respondent feels closest to), compared to 23.3% in the CEP wave 82 survey. Although the sample has a greater number of partisans than a probability sample would yield, the implications for representativeness among partisans are unclear — conditioning on having a partisan identification, it is not clear whether partisanship is stronger or weaker among partisans in my survey, compared to the broader population of partisans. Establishing such claims of broader representativeness would require implementing the more detailed partisanship questions used here within a survey conducted on a probability sample of the general population.

Figure A.11 presents the distribution of scores on the partisan strength scale. All respondents were asked which party they identified with most strongly. Those who indicated that they do not identify with any political party saw a follow-up question, asking which party they feel a little closer to. Some respondents (169) still refused to select a party, so they did not see the follow-up questions for the partisan scale.

FIGURE A.11. Strength of partisan identities.



Note: Distribution of partisan strength scores (n=429).

The partisan scale was constructed by adding together the responses from eight questions, and re-scaling to a 0 to 1 scale. For each question, response options included “disagree strongly,” “disagree somewhat,” “agree somewhat,” and “agree strongly.”

Question wording: You indicated earlier that you identify most strongly with [PARTY]. Please indicate the extent to which you agree with the following statements, thinking about [PARTY].

- When I speak about this party, I usually say “we” instead of “they.”
- I am interested in what other people think about this party.
- When people criticize this party, it feels like a personal insult.
- I have a lot in common with other supporters of this party.
- If this party does badly in opinion polls, my day is ruined.
- When I meet someone who supports this party, I feel connected with this person.
- When I speak about this party, I refer to them as “my party.”
- When people praise this party, it makes me feel good.

TABLE A.6. Reasons for Voting (Observational)

Label	Reason
Expressive Voting	To express my support for my party
Partisan Duty	To contribute to my party's electoral success
Civic Duty	To fulfill my civic duty
Pivotality	My vote could change the outcome of the election
Group Pivotality	Together, my vote and the votes of people like me could change the outcome of the election
Social Pressure	If I didn't vote, people would judge me
NA	I don't vote

Note: Respondents were asked to indicate the most important reason why they vote, from the options listed under "Description."