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Re-examining Electoral Reform: The Role of Voters and Party Backbenchers

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Re-examining Electoral Reform: The Role of Voters and Party Backbenchers

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An abstract of
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Abstract

Re-examining Electoral Reform: The Role of Voters and Party Backbenchers
By Abigail L. Heller

Existing accounts of electoral reform often obscure three issues: 1) electoral reforms are more varied than the literature's focus on changes to the electoral formula (the way votes are translated into seats) suggests, 2) ordinary voters play an important role even when electoral reforms are passed through the legislature, and 3) parties are not unitary actors, as traditionally assumed. In my three-paper dissertation, I address each of these, largely overlooked, elements.

In "The Battle over Gerrymandered Districts: How Americans Balance Fairness and Partisanship," I explore public support for electoral reform — specifically, how voters balance competing preferences for fair elections versus elections that result in preferred policy outcomes. Using a survey experiment focusing on redistricting in the United States, I explore how elite-level polarization affects this tradeoff. I find that concerns about fairness affect the willingness of respondents to support redistricting proposals, but the effects are conditional on the level of party polarization and the strength of respondents' partisanship.

In "Public Support for Electoral Reform: The Role of Electoral System Experience," I consider how experience with different electoral systems affects citizen support for electoral reform. I leverage subnational electoral system variation in the United Kingdom and difference-in-differences designs to estimate the causal effect of experience with alternative electoral systems on support for changes to the electoral formula. I find that negative subnational experiences with an electoral system similar to that under consideration for national elections decrease support for reform.

In "Legislating Themselves Out of Office: Electoral Reform and Parties as Non-Unitary Actors," I examine the conditions under which rank-and-file legislators support electoral reform when they expect it to damage their reelection prospects. I explore this question using data on the careers of members of Parliament and the 1885 Redistribution of Seats Act in the United Kingdom. I find that those who supported reform, despite the reelection risks, are more likely to receive a peerage the following year and may be more likely to receive parliamentary office. This suggests that party leaders can use conventional inducements, but also extra-parliamentary rewards, to ensure support for reform among backbenchers.

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Chapter 1

Introduction

One question that has driven much of the literature on electoral reform[1] is: why would a party alter the electoral system that brought it to power? After all, it is typically parties in power that have the ability to alter the electoral system. Generally, the answer given is either that office-seeking parties propose electoral reform when they believe doing so will increase their legislative seat share (Benoit 2004; Benoit and Hayden 2004; Boix 1999; Colomer 2004) or that policy-seeking parties institute reform to increase their policy influence, either through maximizing their own seat share or that of allied or ideologically-aligned parties (Bawn 1993; Benoit 2004). However, this central question and the answers reveal several things about the traditional literature. First, electoral reform implicitly tends to refer to changes to the electoral formula (the way that votes are translated into seats) for legislative elections. Second, the literature is often focused on the role of political parties in the process of electoral reform. Finally, these parties are generally conceptualized as unitary actors. In this three-paper dissertation, I build on the existing literature while also a) considering a wider range of reforms, b) exploring ordinary citizens' role in and preferences about electoral reform, and c) recognizing that parties are not unitary actors and intra-party politics are

[1] By electoral reform I mean changes to the electoral system or the laws regulating "how votes are cast at elections for a representative assembly and how these votes are then converted into seats in that assembly" (Gallagher and Mitchell 2005, 3; Lijphart 1994), as well as those regulating "electoral competition between and within parties" (Cox 1997, 38).

important determinants of electoral reform.

Before turning to why it is important to address these particular gaps in the existing literature, one might first consider the importance of studying electoral reforms more generally. There is a perception (especially common in the older literature on electoral institutions) that electoral systems are rarely, if ever, altered (Duverger 1963; Grofman and Lijphart 1986; Katz 1980; Lijphart 1999; Nohlen 1984; Norris 1995; Rae 1971; Taagepera and Shugart 1989). In fact, electoral reform is relatively common. Between 1945 and 2010, there were 90 instances of changes to the electoral formulas used for national legislative elections in democracies worldwide (Bormann and Golder 2013). Moreover, if we also consider electoral reform attempts that fail (either in a referendum or the legislature), alterations to electoral systems beyond changes to the electoral formula (as I discuss in more detail below), and subnational electoral reform — all of which are often overlooked, especially in the quantitative electoral reform literature — this count of electoral reforms in democracies worldwide is almost certainly a significant underestimate

Not only is electoral reform surprisingly common, but it connects to core topics in political science. For example, the choice of electoral rules can affect democratic stability, political competition, participation, and representation. Given that electoral systems affect the party system (Benoit 2007; Duverger 1963; Shugart and Taagepera 2017), the type of government (minority or majority, single-party or coalition; Lijphart 1999; Shugart and Taagepera 2017), the behavior of legislators (Chang and Golden 2007; Pellicer and Wegner 2013), strategic incentives facing voters (Cox 1997), and policy outcomes (Bawn 1993), understanding when reforms are likely to occur and what form they will take is critically important.

However, it is especially important to expand the electoral reform literature with a specific focus on a more expansive definition of reform, the role of citizens, and the role of intra-party politics. First, there are many changes to electoral systems beyond changes to the electoral formula. Examples include changes to the district magnitude (the number of seats per district or constituency), the electoral threshold (the threshold of votes above which a party

is eligible for seats), the size of the legislature, voter registration requirements, ballot design, campaign finance, or the drawing of electoral districts (Gallagher and Mitchell 2005; Jacobs and Leyenaar 2011; Leyenaar and Hazan 2011; Lijphart 1994; McElwain 2008; Negretto 2015; Shugart and Taagepera 2017). These comparatively "minor" changes may be easier to pass than changes to the electoral formula (which may require supermajorities), may achieve similar goals in terms of seat maximization, and may be more predictable than reforms to the electoral formula which will also affect the party system, the type of government that forms, and the strategic incentives for voters (Benoit 2007; Cox 1997; Duverger 1963; Lijphart 1999; Shugart and Taagepera 2017). Moreover, these alternative types of reform, although largely overlooked in the electoral reform literature (with the exceptions noted above), are almost certainly more prevalent than changes to the electoral formula. In Japan, for example, of 73 reform proposals since 1889, only 16 (22%) involved changes to the electoral formula.[2]

Second, political parties are not the only actors that influence electoral reform. Much of the literature gives voters only an implicit role, essentially treating the distribution of policy preferences in the electorate as fixed, such that a party need only determine which electoral system will achieve a favorable translation of those preferences into seats. However, not only does electoral reform have the potential to affect the preferences of the electorate (i.e., through affecting the party system or the choices available to voters as well as the incentives they face for strategic voting), voters, courts, experts, and pressure groups can all directly affect the process of electoral reform (Benoit 2007; D'Alimonte 2005; Leyenaar and Hazan 2011; Renwick 2010; Vowles 2005). Indeed, in recent years there have been a number of both national and subnational cases — Italy in 1991 and 1993; New Zealand in 1993; British Columbia in 2005, 2009, and 2018; the United Kingdom in 2011; Maine in 2016; and Ontario in 2017 — in which the fate of electoral reform was directly determined by voters in initiatives or referenda (D'Alimonte 2005; Denemark 2001; Donovan 1995; Gambetta and Warner 2004; Katz 2001; Nagel 2004; Qvortrup 2012; Renwick 2010; Santucci 2018; Vowles

[2] Figures compiled by the author from Gallagher (1998), McElwain (2008), Reed (2005), Reed and Thies (2001), Renwick (2010), and Wada (2004).

2005). Moreover, even in the more prevalent cases in which electoral reform is decided in the legislature, we expect parties to be responsive to voters and take their preferences into account (Weingast 1997). Therefore, to have a complete picture of electoral reform, we must consider what affects voter support for reform.

Finally, while a convenient (and occasionally necessary) simplification, we know that parties are not unitary actors. Instead, political parties are groups of individuals that may behave in a more or less cohesive way depending on both the preferences of party members and the disciplinary incentives and tools that are available to and used by the party leadership (Aldrich 1995; Benedetto and Hix 2007; Bowler, Farrell, and Katz 1999; Cox 1987; Cox and McCubbins 1993; Duverger 1963; Eggers and Spirling 2014b; Hicken 2009; Kam 2009; Laver and Shepsle 1999). Even if a proposed electoral reform would benefit a party as a whole, there may be different consequences for individual legislators (Gandhi, Heller, and Reuter 2020; McElwain 2008; Remington and Smith 1996). Moreover, the conventional wisdom that assumes parties behave as if they are unitary actors cannot explain cases in which reforms that would have increased the overall seat share of the party were thwarted by party backbenchers (rank-and-file legislators) despite the wishes of party leaders — as in Japan and Russia (McElwain 2008; Remington and Smith 1996) — or cases in which parties knowingly passed reforms that *decreased* their seat share — as in the United Kingdom. Thus, to understand the full range of electoral reforms (and reform attempts that fail), it is necessary to consider what affects support for reform among individual legislators.

Each of the three papers in this dissertation address one or more of these critical areas that are sometimes neglected in the existing literature. Two of the papers (the first and third) address electoral reforms that tend to be overlooked because they are not changes to the electoral formula. Two of the papers (the first and second) address what factors affect citizen support for electoral reform, rather than focusing on the level of the political party. Finally, continuing the effort to shift the focus away from unitary parties, the third paper explores what affects individual legislators' support for electoral reform. I further summarize

each of the three papers and their contributions below.

1.1 Overview of the Dissertation

In the first paper, "The Battle over Gerrymandered Districts: How Americans Balance Fairness and Partisanship," I ask, to what extent do citizens value democratic institutions — particularly, free and fair elections — versus their preferred partisan and policy outcomes? Are citizens willing to sacrifice fair and competitive elections for preferred policy outcomes or is their commitment to democracy robust to elite-level ideological polarization? These are particularly important questions because citizens are supposed to be the last line of defense against democratic backsliding and especially against abuse of power by incumbents who seek to undermine free and fair elections (Bermeo 2003; Fearon 2011; Levitsky and Way 2010; Levitsky and Ziblatt 2018; Weingast 1997). Moreover, because citizens care about policy outcomes and not only democratic institutions, a large literature suggests that ideological polarization can undermine support for democracy and lead citizens to support anti-democratic incumbents who better reflect their policy preferences (Aghion, Alesina, and Trebbi 2004; Ahlquist et al. 2018; Berman 1997; Bermeo 2003; Carey et al. 2019; Graham and Svolik 2020; Levitsky and Ziblatt 2018; Linz 1978, 1990; Sani and Sartori 1983; Sartori 1976; Svolik 2020).

I explore these questions in the context of redistricting proposals in the United States using a conjoint survey experiment with over 2,100 U.S. residents. I argue that people's preferences regarding redistricting are dependent on their partisanship and policy preferences, but also their preferences regarding fair elections. How individuals balance these potentially competing preferences depends on the level of polarization between political parties. I find evidence that people's preferences regarding redistricting are influenced by partisan concerns as well as preferences regarding fair elections. Moreover, the effects are conditional on the level of polarization between political parties.

In addition to contributing to the literature on electoral reform by considering reforms that are often excluded by the existing literature (those having to do with the drawing of electoral boundaries) as well as better understanding citizen preferences about these reforms, this paper also contributes to several other literatures. First, I contribute to the comparative literature on support for democracy and democratic stability (e.g., Bermeo 2003; Linz 1978, 1990; Sani and Sartori 1983; Sartori 1976; Svolik 2020). However, rather than exploring abstract preferences for democracy, I focus on free and fair elections which are a necessary precondition for democracy. Second, I contribute to the literature on public opinion about redistricting in the United States (e.g., Fougere, Ansolabehere, and Persily 2010; Frankovic 2017; McDonald 2008; Tolbert, Smith, and Green 2009). However, I directly elicit opinions about different types of "independent" districting commissions, consider elite-level polarization as well as partisanship, and use an experiment to obtain causal estimates of the effect of preferences for fairness, partisanship, and polarization on public opinion.

In the second paper, "Public Support for Electoral Reform: The Role of Electoral System Experience," I explore what affects support for electoral reform among the public. Specifically, I ask, how does experience with multiple electoral systems affect people's willingness to support reforms to the electoral formula? Answering this question is critical because of the increasing use of initiatives and referenda to enact changes to electoral systems (e.g., D'Alimonte 2005; Denemark 2001; Donovan 1995; Gambetta and Warner 2004; Katz 2001; Nagel 2004; Qvortrup 2012; Renwick 2010; Santucci 2018; Vowles 2005). Moreover, experience with multiple electoral systems is common, even in places where the electoral system for national legislative elections has remained consistent over time. For example, Australia, Canada, Chile, France, Italy, Japan, Nepal, New Zealand, Russia, the United Kingdom, and the United States all use different electoral systems for elections to different national bodies or at different levels of government or have changed the system used for national legislative elections over time, exposing (at least a subset of) citizens to multiple electoral systems.

I argue that experience with alternative electoral systems can affect support for elec-

toral reform because experience provides information and reduces the inherent uncertainty of reform proposals (Andrews and Jackman 2005). Importantly, I argue that the experience must be with a system similar to that which is being proposed because this provides the relevant counterfactual for people to evaluate the likely effects of the proposal. However, I also argue that the direction of the effect will be dependent on partisanship and the type of experience — individuals who have a positive experience (which I conceptualize as experience in which their preferred party benefited from the electoral system) will be more likely to support reform, while a negative experience will decrease one's likelihood of supporting reform. Additionally, I argue that experience will be especially important in the absence of clear and consistent party cues about the electoral system. I test this theory using observational data and leveraging subnational electoral system variation in the United Kingdom. I use difference-in-differences designs to estimate the causal effect of experience with different electoral systems at the subnational level on support for reform at the national level. I find evidence consistent with the theory. Specifically, I find that individuals who had a negative experience with an alternative electoral system and whose preferred party provided weak or confusing cues are less likely to support reform.

While I noted above that the traditional electoral reform literature often overlooks citizen opinions about reform and electoral systems, this is beginning to change. Thus, with this paper, I contribute to that important (and growing) segment of the literature that considers public opinion about reform (Banducci and Karp 1999; Norris 2011; Plescia, Blais, and Högström 2020; Renwick 2010, 2011). Additionally, the results suggest that people prefer electoral rules that benefit their preferred party (Lamare and Vowles 1996; Plescia, Blais, and Högström 2020), which implies that popular incumbents may find it relatively easy to institute or maintain electoral rules that keep themselves in power (Ahlquist et al. 2018; Graham and Svolik 2020). Therefore, this paper also has implications for the literature on democratic stability and support for democracy.

Finally, in my third paper, "Legislating Themselves Out of Office: Electoral Reform

and Parties as Non-unitary Actors," I consider why an individual politician — a party backbencher outside the party leadership — would support an electoral reform that he believes is likely to cost him his seat. As explained above, understanding what influences legislator support for electoral reform is critical because parties do not always behave as unitary actors, as often assumed (McElwain 2008; Remington and Smith 1996). Moreover, while the party leadership can usually use a variety of carrots and sticks to control members (Benedetto and Hix 2007; Bowler, Farrell, and Katz 1999; Cheibub 2007; Cox 1987; Cox and McCubbins 1993; Duverger 1963; Eggers and Spirling 2014b; Kam 2009), in the context of an electoral reform that legislators believe will damage their election prospects, some of these incentives lose their effectiveness. For example, offers of future committee assignments or cabinet posts or threats of withdrawal of election support are not credible if the politician believes they will lose their seat regardless.

In this situation, I argue that those who expect the party leadership to compensate them in the form of patronage (appointments) for their loss are more likely to support a reform that is likely to damage their own electoral fortunes. Importantly, I consider appointments that do not require recipients to hold elected office. Thus, the party leadership can compensate members of Parliament (MPs) who support the party and vote in favor of electoral reform but lose the election (note that this does not preclude the possibility that the leadership will also reward those who support the reform and retain their seats through the traditional parliamentary channels). I test this argument using detailed information on the career trajectories of Liberal Party MPs in the United Kingdom and leveraging the 1885 Redistribution of Seats Act. I find evidence that those who supported the reform were more likely to be rewarded with a new peerage in the following year and this also holds for the subset of MPs who ran and lost in the election after the reform. Moreover, there is also some evidence that those who supported reform and retained their seat may have been rewarded with parliamentary office.

This paper contributes to the electoral reform literature both by disaggregating parties

to consider individual legislators and by considering reforms other than those to the electoral formula (the 1885 Redistribution of Seats Act changed the district magnitude and boundaries of the constituencies). In addition, it builds on and engages with a large literature on party discipline (Benedetto and Hix 2007; Bowler, Farrell, and Katz 1999; Cheibub 2007; Cox 1987; Cox and McCubbins 1993; Duverger 1963; Eggers and Spirling 2014b; Kam 2009), while recognizing that the party leadership can also leverage incentives outside parliament that are rarely considered in this literature. Moreover, this paper also contributes to our understanding of a particularly important instance of electoral reform — this reform introduced the single-member districts which form the basis of today's electoral system in the United Kingdom — that is sometimes overshadowed by reforms to the franchise during this time period (Cox 1987; Hart 1992; Hawkins 1998; Mason 2015).

Chapter 2

The Battle over Gerrymandered Districts: How Americans Balance Fairness and Partisanship

> "We are in the business of rigging elections."
> — Mark McDaniel, former North Carolina State Senator, referring to redistricting (quoted in Winburn 2011).

Free and fair elections are the foundation of democracy.[1] For elections to be free and fair, there must be competition — the rules must provide multiple parties and candidates a chance to win, such that alternation in power is a possibility (Anderson et al. 2005; Birch 2008; Przeworski 1991, 2015). Fundamentally, elections are unfair if those in power are able to change the rules to ensure that they remain in power.[2] Since incumbents are one of the

[1] This project was approved by Emory University's Institutional Review Board under IRB00100275. Funding for this project was provided by the Emory University Laney Graduate School's Professional Development Support Funds.

[2] Although any rule (regardless of its architect) that artificially advantages the incumbent may be problematic, at the extreme, occupying office would grant incumbents the power to shape the rules in such a way as to deny others the opportunity to occupy office in the future.

biggest threats to maintaining a level playing field, it is essential that their power is contained to ensure free and fair elections (Levitsky and Way 2010; Levitsky and Ziblatt 2018).

Other democratic institutions, such as independent courts, are supposed to serve as a check on incumbents. However, citizens are the final line of defense (Fearon 2011; Weingast 1997). Citizens are supposed to guard against incumbent abuse of power by exercising their own power at the ballot box (Bermeo 2003). Alternatively, a robust civil society may be important for mobilizing citizen protest to contain incumbents (Bermeo 2003). Yet we know little about whether citizens would actually be willing to play this role. And history provides strong circumstantial evidence that they may not.

One reason citizens may not play this role as defenders of democracy is that they have preferences over more than just democracy. While citizens may care about democratic institutions such as free and fair elections, they also care about the policies these institutions produce. Elections determine not only who occupies office, but also which policies are implemented. People have (potentially very strong) preferences about policy outcomes and free and fair elections may or may not lead to preferred outcomes. When citizens in a democracy have strong policy disagreements, they may occasionally be willing to sacrifice their commitments to democratic institutions to ensure preferred policy outcomes. Specifically, strong ideological polarization can lead citizens to support anti-democratic movements or incumbents who represent their policy interests (Linz 1978, 1990; Sani and Sartori 1983; Sartori 1976).[3] Citizens with strong policy preferences or partisanship may dislike the alternative policy so much that they are willing to support incumbents who bend or even break the rules of democracy to maintain their power (Aghion, Alesina, and Trebbi 2004; Ahlquist et al. 2018; Graham and Svolik 2020; Svolik 2020).[4] Indeed, even with an active citizenry, democracy may be undermined by extreme polarization, as in Weimar Germany or 1970s Chile (Berman 1997; Bermeo 2003). So how much do citizens value democratic institutions

[3] The literature is often unclear about whether the relevant polarization is at the elite or citizen level.

[4] In a related experiment, Ahlquist et al. (2018) find that people use "partisan-motivated reasoning" to inform their preferences about institutional changes and that partisan cues cause opposition supporters to view competition-reducing reforms instituted by the incumbent more negatively.

versus the policies that democracy produces? Are citizens willing to sacrifice core democratic institutions, such as competitive elections, in order to obtain preferred policy outcomes or is their commitment to democracy robust to ideological polarization?

The negative effect of polarization on citizens' support for democracy has primarily been examined in developing democracies. However, recently these themes from the comparative democratic backsliding literature have been applied to the United States (Carey et al. 2019; Graham and Svolik 2020; Levitsky and Ziblatt 2018). Given that gerrymandering is one way for incumbents to consolidate their hold on power, an area of particular concern regarding the state of democracy in the U.S. is redistricting — the process of redefining boundaries for political representation. In 2017, the U.S. Supreme Court heard two partisan gerrymandering cases — regarding whether Wisconsin Republicans and Maryland Democrats had drawn unconstitutional partisan gerrymanders.[5] These are only the latest in a long series of gerrymandering cases and in each, the Court has declined to rule on the underlying question of when partisan gerrymandering becomes unconstitutional.[6] These events underscore that both parties engage in gerrymandering and it is an ongoing issue.

Do citizens support this gerrymandering? Generally, many people profess to oppose gerrymandering, if they have any opinion (Fougere, Ansolabehere, and Persily 2010; Frankovic 2017). More specifically, the literature has focused on public opinion about the body that draws the districts or the interaction of partisanship and redistricting opinions. Although a majority of states give state legislatures control over the redistricting process, fourteen states now use what are generally known as "independent districting commissions" in some capacity when drawing districts for the U.S. House of Representatives and twenty-three use them for state legislature redistricting (NCSL 2015a, 2015b). Scholars have found that those who support the party benefited by gerrymandering accept it, while those who identify with

[5] Partisan gerrymandering is drawing electoral districts to benefit a particular political party (Galderisi and Cain 2005). A closely related concept is incumbent-protection gerrymandering or drawing electoral districts to protect incumbents (McDonald 2004; Winburn 2011). In this paper, "gerrymandering" refers to both types (I do not consider racial gerrymandering).

[6] See *Benisek v. Lamone*, 585 U.S. (2018) and *Gill v. Whitford*, 585 U.S. (2018).

the "victimized" party support removing redistricting power from state legislatures (Fougere, Ansolabehere, and Persily 2010; McDonald 2008; Tolbert, Smith, and Green 2009). However, to the best of my knowledge, no studies examine the interaction of preferences for fairness, partisanship, and elite-level polarization in the context of redistricting.[7]

I argue that to understand public opinion about redistricting, we must account for people's preferences regarding both *fairness* and policy (*partisanship*). While people differ in their ideal points on a left-right policy or ideology dimension, they may also differ on an orthogonal dimension capturing their preferences for fair elections. Additionally, I distinguish between two types of fairness — fairness regarding the process of redistricting and fairness regarding the outcome — over which people may have preferences. Moreover, elite-level *party polarization* conditions responses to redistricting proposals. As party polarization increases, redistricting proposals that shift the balance of power in the legislature are more likely to result in large shifts in policy outcomes. Therefore, it is more likely that support for redistricting proposals is dominated by fairness concerns only when no such shift is expected. However, when a redistricting proposal will shift the legislative balance of power and therefore policy, some people face a tradeoff between their preferences regarding fairness and their policy preferences. Those with extreme policy preferences (strong partisans) will be more likely than moderates to support a redistricting plan that reduces fairness if their preferred party benefits. For those with extreme policy preferences, a shift in the legislative power balance away from their preferred party would shift policy outcomes far away from their ideal point and they are willing to sacrifice fairness to avoid such a shift.

I test this theory with a survey experiment utilizing a conjoint design and 2,116 respondents living in the United States. I find that fairness (in terms of the process and outcome) and the strength of an individual's partisanship affect redistricting preferences. Moreover, I find some evidence that these effects depend on the level of party polarization.

[7] There has been recent work on how partisanship and polarization condition preferences for democracy (Graham and Svolik 2020; Svolik 2020), but these works use a broader conception of support for democracy. By focusing specifically on election fairness, I am able to precisely operationalize the concept and focus the empirical tests on this critical aspect of democracy.

This paper contributes to several literatures. First, the theory relates to the comparative literature on democratic stability and support for democracy. Although the idea that polarization may be problematic for democracy is prominent in the literature (e.g., Bermeo 2003; Linz 1978, 1990; Sani and Sartori 1983; Sartori 1976; Svolik 2020), I argue that polarization has a nuanced effect on public opinion (and thus, democratic stability). In particular, there is a distinction between societal and elite-level polarization and we also have to consider the existing balance of power between competing political forces. Additionally, by clearly operationalizing elite polarization and fair elections rather than exploring abstract preferences for democracy, I am able to carefully test how these complicated concepts affect opinions.

Second, this paper contributes to the literature on public opinion about redistricting. Much of this literature focuses on preferences for redistricting by "independent districting commissions" rather than state legislatures. However, the "independent" label almost never refers to politically independent, but to the fact that these commissions are independent of the state legislature.[8] In existing surveys, respondents are often asked to choose between redistricting by the state legislature or by an "independent districting commission" and may (erroneously) infer they are politically independent. One contribution of this paper is that I explore the effect of the type of commission on the choice of a redistricting proposal. Moreover, I account for other aspects of fairness beyond the proposer, unlike much of the literature. Additionally, by considering elite-level polarization in addition to partisanship, I add nuance to the literature that examines how partisanship affects redistricting opinions. Finally, while existing studies of redistricting opinions generally do not attempt causal identification, I empirically test the theory using a survey experiment to obtain causal estimates of the effect of concerns about fairness, partisanship, and polarization on public opinion. This allows me to probe the causal mechanisms of support for different redistricting schemes and better understand the tradeoffs faced by individuals.

In the next section, I present the theory and hypotheses. I then describe the experimental

[8] In practice, there are three different types of redistricting commissions — partisan, bipartisan, or independent/nonpartisan (McDonald 2004, 2008; Winburn 2011).

design, sample, and estimation strategy. After analyzing the results and evaluating their robustness, I conclude with directions for future research.

2.1 Preferences Over Electoral Reform

The democratic backsliding literature focuses on general support for democracy in times of polarization. However, democracy itself is a contested concept (Cheibub, Gandhi, and Vreeland 2010; Coppedge et al. 2016). Thus, I focus on elections as an important aspect of democracy and specifically examine redistricting for two reasons. First, it touches on both critical components of democracy — participation and contestation (Dahl 1971). Second, gerrymandering is a clearer violation of democratic norms than something such as voter identification laws, about which reasonable people could disagree regarding how they affect democracy. Given this focus, it is necessary to carefully conceptualize fairness in this context.

Fair elections require competition and the possibility of alternation in power (Anderson et al. 2005; Przeworski 1991, 2015). In the context of redistricting, fairness requires that partisans and incumbents are not advantaged by the way electoral districts are drawn. Specifically, I operationalize this in three ways. First, redistricting may be unfair if it results in, on average, less competitive races. Given what we know about incumbent advantages, less competitive races are likely to benefit incumbents. Second, redistricting that results in more uncontested seats may be unfair as it is likely to lead to more unchallenged incumbents.[9] Finally, the process of redistricting may be unfair. Elections are fundamentally an opportunity for citizens to choose their representatives, so a redistricting process that allows incumbents to choose their own electors by drawing their own electoral districts may be unfair. The first two operationalizations focus on the fairness of the outcome of redistricting, whereas the last focuses on what might be called process fairness. Both types may affect

[9] Less competitive races and uncontested seats are related to the concept of gerrymandering by "packing" partisans in a few districts. By filling a district with as many voters who identify with one party as possible, it is more likely that party will win that district by a large margin, but none of the surrounding districts. Effectively, this gerrymandering strategy relies on creating several uncompetitive districts.

opinions about redistricting.

I argue people have preferences regarding election fairness. There are at least two ways to think about these preferences. First, people might have what could be called an instrumental preference for fairness — they prefer fair elections and redistricting because they recognize that in the future, they may be in the minority and are therefore better off long-term if redistricting is fair. To the extent this would be a rational position for any individual, this is similar to treating fairness as a valence issue — one in which all individuals prefer and are better off when the electoral rules are fair. However, these preferences regarding fairness could also be more normative in nature. Some people may simply prefer fair redistricting because they believe this is good for democracy. To the extent that not everyone agrees on the normative importance of fair redistricting, it is possible that individuals differ in the strength (or potentially direction) of their preferences regarding fairness.

Regardless of their origin, these preferences regarding fairness are conditioned by peoples' partisanship or policy preferences and the extent of elite-level polarization between parties (party polarization). People care about policy, which can be represented on a left-right dimension on which individuals have differing ideal points or preferences. I conceptualize partisanship as attachment to a political party in part because of that party's policy positions.[10] Given the well-established two-party system in the U.S., strength of partisanship and extremism on the policy dimension are closely related. The more extreme policy positions an individual holds, the more likely they are to strongly identify with the party on their preferred side of the ideological spectrum.

How individuals balance these potentially competing preferences about fairness and partisanship, however, depends on the level of party polarization. Here, party polarization is the difference between the average ideal points of party elites; when the parties hold the same ideological (policy) position, there is no polarization, but polarization increases as the ideal points of the parties become further apart. Party ideal points can differ on one or both of

[10] This conceptualization of partisanship as tied to party policy does not rule out affective partisanship; rather, it suggests that partisanship is not entirely devoid of policy considerations.

at least two dimensions — social and economic policy. In the U.S., these dimensions largely collapse down to a single left-right dimension. However, in the experiment, I disaggregate them to understand if one type of polarization is more important for respondent preferences.

To understand how party polarization shapes how individuals evaluate the tradeoff between fairness and partisanship, consider the U.S. First, imagine there is no party polarization — both parties are aligned at the same place (it need not be the center) on the left-right policy dimension. In this situation, a redistricting proposal will not affect policy outcomes, even if it results in a shift in the balance of power between parties in the legislature. Therefore, individuals should be primarily concerned with the proposal's impact on fairness and their partisanship should not affect their support. In other words, in the absence of party polarization, preferences regarding fairness should dominate partisan preferences regardless of the strength of an individual's partisanship — whether they are strong partisans, weak partisans, independents who lean towards one party (leaners), or independents.[11]

Hypothesis 2.1. In the absence of party polarization, on average, concerns about fairness will outweigh partisanship when strong partisans, weak partisans, leaners, and independents evaluate redistricting proposals.

However, the individual's calculation changes as party polarization increases. Increasing polarization makes some people more likely to trade fairness for redistricting proposals that produce preferred partisan (or policy) outcomes. As polarization increases, redistricting proposals that shift the balance of power between parties have increasingly large effects on policy outcomes. Thus, unlike in the no polarization scenario, under party polarization, a redistricting proposal that changes the legislative balance of power is more likely to shift outcomes considerably — possibly far away from the individual's ideal point. To avoid this unappealing policy outcome, the individual may be more likely to accept unfair proposals that benefit his preferred party.

[11] Independents need not be exactly in the center of the ideological spectrum. They are simply indifferent between the two parties.

Moreover, conditional on party polarization, preferences will differ depending on the strength of an individual's partisanship (or extremism on the policy dimension). To illustrate, compare two individuals in the context of extreme party polarization: one a strong Democrat (whose policy ideal point is far to the left) and one an independent (whose ideal point is roughly in the middle of the policy spectrum). Suppose both are asked to evaluate a redistricting proposal that decreases fairness and changes the legislative balance of power such that the likelihood of a Democratic majority (or a policy outcome on the left) is increased. The independent is likely to oppose the proposal, but the strong partisan may support it. When strong partisanship is combined with party polarization, the strong partisan so dislikes the alternative policy option that he is willing to sacrifice fairness for a higher likelihood of obtaining his preferred policy. In contrast, the independent is more likely to be indifferent between the two policy outcomes (assuming relatively symmetric party polarization) and is therefore less likely to support a proposal that decreases fairness. In other words, as party polarization increases, strong partisans are more likely to trade fairness for preferred partisan outcomes, while independents are more likely to prefer fair redistricting, even if the proposed reform would change the distribution of power in the legislature.

More generally, as party polarization increases, certain groups face a tradeoff if the redistricting proposal decreases fairness and affects the legislative balance of power. Independents and those whose partisanship is strong, but unaligned with the party benefited by the balance of power shift will not support the proposal. For independents, the proposed decrease in fairness is sufficient to induce them to object. For strong partisans, the proposal both decreases fairness and shifts the balance of power (and policy outcomes) away from their preferred party (or policy) — both incentives combine to induce this group to object to the proposal. Conversely, those whose party will benefit from the redistricting proposal face a tradeoff. Those with strong partisan attachments (or extreme policy preferences) are more likely than weak partisans or leaners to resolve this tradeoff by accepting a decrease in fairness to secure a favorable shift in the legislative balance of power. It is strong partisans who

benefit most (or, conversely, stand to lose the most) by a shift in the balance of power and thus policy. In other words, when a proposed redistricting plan has the potential to affect the balance of power in the legislature, some people (those with strong partisan attachments who expect their party to benefit) will be willing to sacrifice fairness.

Hypothesis 2.2. As party polarization increases, on average, strong partisans who benefit from the proposal will be more likely to prioritize partisanship over fairness when evaluating redistricting proposals. Weak partisans, leaners, and independents will, on average, be more likely to prioritize fairness.

2.2 Experimental Design

The United States is an ideal context in which to test this theory. First, the U.S. is clearly a two-party system, making empirical testing straightforward. Second, although there has been increasing polarization nationally (e.g., Abramowitz and Saunders 2008), there is variation in the extent of polarization at the state level due to the federal structure (Shor 2018). Third, there have recently been increasing concerns about the state of democracy with multiple groups documenting decreases in the democracy or freedom scores of the U.S. (Carey et al. 2019; Freedom House 2018; Lührmann et al. 2018). Finally, as discussed previously, gerrymandering in the U.S. is a prominent and salient issue.

Specifically, I use a conjoint experiment to test the hypotheses. Conjoint designs allow for a) causal inferences, b) evaluation of the relative influence of multiple treatment components on the same scale, c) increased external validity as respondents face complex choices similar to realistic situations, and d) reduced concerns about social desirability bias because there are multiple rationales for a given choice (Hainmueller and Hopkins 2015; Hainmueller, Hopkins, and Yamamoto 2014). The experiment consists of three tasks completed by each respondent. In each task, respondents choose between pairs of profiles — hypothetical scenarios with information about U.S. states and redistricting proposals. The scenarios consist of five

attributes, each with between three and five values that vary randomly. The scenario profiles are randomly generated, and attributes are ordered randomly across respondents (but not across tasks within respondent).[12] In each task, respondents are asked to choose which scenario they prefer and rank order each scenario on a seven-point scale from strongly dislike to strongly support. Figure 2.1 presents an example of a task.[13]

Each of the five attributes in the conjoint captures a feature of redistricting scenarios. Three attributes pertain to the state in which the proposal is made: a) the *existing balance of power* in the state legislature,[14] b) the level of *party polarization*, and c) the redistricting *proposal origin*. The remaining attributes relate to the redistricting proposal itself: a) the proposal's *expected effect on elections* and b) the proposal's *expected effect on the balance of power* (or *expected partisan outcomes*) in the state legislature. The theory suggests that individuals consider a tradeoff between fairness and partisanship and that this tradeoff is affected by the level of party polarization. Of these attributes, the *existing balance of power* and *expected effect on the balance of power* capture concerns related to partisanship, the *proposal origin* and *expected effect on elections* capture concerns related to (process and outcome) fairness, and *party polarization* captures the polarization between the parties on economic and social issues. Table 2.1 summarizes the attributes and their possible values.

Before continuing, it is helpful to clarify what to expect if the hypotheses are correct. Hypothesis 2.1 states that, on average, in the absence of party polarization, preferences for fairness will outweigh partisan preferences regardless of strength of partisanship. If true, we should expect the average marginal effects of attribute values relating to fairness (bipartisan districting commissions, nonpartisan districting commissions, fewer uncontested seats, and closer races on average) to be *positive and larger* in magnitude than the average marginal effects of partisanship attribute values (one party having a majority in both chambers or

[12] The design differs from complete randomization in two ways; see Appendix A for details.
[13] Prior to the experiment, the survey includes a variety of demographic and political opinion questions, allowing me to explore heterogeneous treatment effects and control for potential confounders in some analyses (the full survey instrument is in Appendix A).
[14] Only Nebraska has a unicameral legislature, so my experiment involves bicameral legislatures.

Figure 2.1: Example of a Conjoint Experiment Task

Please read the scenario descriptions carefully. Afterwards, please indicate which of the scenarios you would personally prefer. In other words, please indicate which redistricting proposal you would prefer to see enacted in its state, even if you are unsure.

	Scenario 1	Scenario 2
Existing balance of power in state legislature	Democrats have majority in both chambers	Republicans have majority in both chambers
Parties' policy preferences	Similar on economic issues, but different on social issues	Similar on economic issues, but different on social issues
Redistricting plan proposed by	Bipartisan districting commisssion (made up of people who are not currently members of the state legislature)	Partisan districting commission (made up of people who are not currently members of the state legislature)
How proposal is expected to affect elections	On average, individual races will be less close	Fewer uncontested seats
How proposal is expected to affect balance of power in state legislature	Increased seat share for Republicans	Increased seat share for Democrats

If you had to choose between the two scenarios, which of them would you prefer?

Scenario 1

Scenario 2

increased seat share for either party) when there is no party polarization (similar policy preferences on both social and economic issues).[15] Moreover, the theory implies that under no polarization, fairness considerations will outweigh partisanship considerations *even if the balance of power in the legislature will be affected* by the redistricting proposal. Thus, I interact the attributes capturing fairness with *expected partisan outcomes*.[16] Specifically, I interact a) the *proposal origin* and *expected effect on the balance of power* attributes and b)

[15] Similarly, the average marginal effects of attribute values capturing a lack of fairness (proposals by the majority party or partisan districting commissions) should be negative and larger in magnitude than the average marginal effects of the partisanship attribute values under no party polarization.
[16] There is no need to also interact the polarization variable since the reference category is no polarization.

Table 2.1: Attributes and Attribute Values in Conjoint Experiment

Attribute	Values
Existing balance of power (Existing balance of power in state legislature)	Democrats and Republicans each control one chamber Democrats have majority in both chambers Republicans have majority in both chambers
Party polarization (Parties' policy preferences)	Similar on both economic and social issues Similar on economic issues, but different on social issues Different on economic issues, but similar on social issues Different on both economic and social issues
Proposal origin (Redistricting plan proposed by)	Majority party in state legislature Bipartisan committee of state legislators Partisan districting commission (made up of people who are not currently members of the state legislature) Bipartisan districting commission (made up of people who are not currently members of the state legislature) Nonpartisan districting commission (made up of people who are not currently members of the state legislature)
Expected election outcomes (How proposal is expected to affect elections)	On average, individual races will be less close On average, individual races will be closer More uncontested seats Fewer uncontested seats
Expected partisan outcomes (How proposal is expected to affect balance of power in state legislature)	No change in seat share Increased seat share for Democrats Increased seat share for Republicans

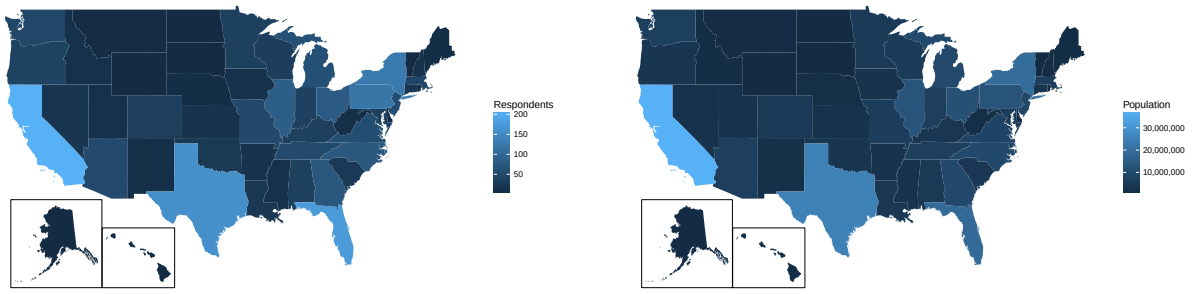
Note: The attribute text in parentheses was presented in the experiment.

the *expected effect on election outcomes* and *expected effect on the balance of power* attributes. If Hypothesis 2.1 is correct, we should expect the average marginal effect of the fairness attribute values to be relatively constant across values of the *expected effect on the balance of power* — the interaction terms should not be significant.

Hypothesis 2.2 states that as party polarization increases, strong partisans will, on average, be more likely to prioritize partisanship over fairness when evaluating redistricting proposals. Testing this requires interacting the fairness,[17] *expected effect on the balance of power*, and *party polarization* attributes. If Hypothesis 2.2 is correct, we should expect the average marginal effects of the fairness attribute values to be *smaller* in magnitude for strong partisans when polarization is high and the proposal would negatively affect the respondent's

[17] I run two analyses, one with *proposal origin* in the interaction and one with *expected effect on elections*.

Figure 2.2: Distribution of Survey Respondents and U.S. Population



Note: Distribution of survey respondents (left) and U.S. population at the 2010 census (right). Source: U.S. Census Bureau (2017).

preferred party, compared to when the proposal will not affect the balance of power.

2.2.1 Sample

The experiment involved 2,116 respondents living in the U.S. recruited from Amazon's Mechanical Turk (MTurk).[18] The sample includes at least two respondents from every state and the District of Columbia. Moreover, the distribution of respondents is roughly proportional to the distribution of the population across states (see Figure 2.2). Respondents range in age from 18 to "90 or older" with a mean of 37 and a median of 34.[19] Similar to other MTurk surveys (Berinsky, Huber, and Lenz 2012), there are slightly more women (54%) than men in the sample, most respondents identify as white or caucasian (75%), and more respondents identify with the Democratic Party (57% identify with or lean towards the Democrats) than with the Republican Party. However, these demographic distributions are similar to those in the 2008 American National Election Study (Berinsky, Huber, and Lenz 2012). See Appendix A for additional details.

[18] Of this sample, 101 respondents participated in the pilot, fielded on December 13, 2017, while 2,015 participated in the full survey fielded on December 18, 2017. Since the survey was unchanged between waves, I pool the sample for the main analysis.

[19] For these calculations, "90 or older" was recoded to 90.

2.2.2 Estimation

Conjoint designs allow for identification of several causal quantities. First, I estimate the average marginal component effects (AMCEs), which are the average changes in the probability a redistricting scenario (profile) is preferred. In other words, the AMCE is the marginal effect of a given attribute value. Additionally, I estimate the average component interaction effects (ACIEs), which allow the marginal effect of an attribute to vary depending on another attribute. Finally, I estimate the conditional AMCEs. That is, the AMCEs conditional on characteristics of respondents (measured prior to treatment to avoid post-treatment bias). Specifically, I estimate the AMCEs conditional on respondent partisanship. Hainmueller, Hopkins, and Yamamoto (2014) demonstrate that these quantities are nonparametrically identified because the attributes are conditionally independently randomized.[20] Moreover, these quantities can be estimated by classifying the sample into relevant strata and regressing an indicator of whether the profile was preferred (*Scenario Preferred*) on indicators for each attribute value (Hainmueller, Hopkins, and Yamamoto 2014).[21]

The unit of analysis is the profile (redistricting scenario). Observed responses are not independent within respondents (across profiles) so for all profile-level analyses, standard errors are clustered by respondent. Since each of the 2,116 respondents completed three tasks with two profiles per task, the design allows for up to 12,696 observations. However, the main analysis consists of 1,786 respondents and 10,716 profile-level observations. First, I exclude 328 respondents who failed at least one of two attention checks placed earlier in the survey (see Appendix A for details).[22] Second, I exclude respondents whose party identification is coded as don't know.[23] Party identification was coded on the basis of responses to standard questions (adapted from The American National Election Studies 2016; see Appendix A)

[20] This holds even with the randomization restrictions I use.
[21] Due to the randomization restrictions, the estimates are weighted averages of coefficients from regressions including interactions for the restricted attributes (Hainmueller, Hopkins, and Yamamoto 2014). These interactions are excluded from all figures, but all results have been corrected for the restrictions.
[22] All analyses exclude these observations, unless otherwise noted. All respondents completed the survey regardless of whether they passed the attention checks.
[23] Two respondents were excluded for this reason despite passing the attention checks.

that allow me to place respondents on a seven-point scale: strong Democrat, weak Democrat, independent leans Democrat, independent, independent leans Republican, weak Republican, or strong Republican. In most analyses, I combine weak partisans and independents who lean towards a party because research has shown that "leaners" behave more like weak partisans than true independents (e.g., Bartels 2000; Keith et al. 1992; Petrocik 2009).

2.3 Analysis

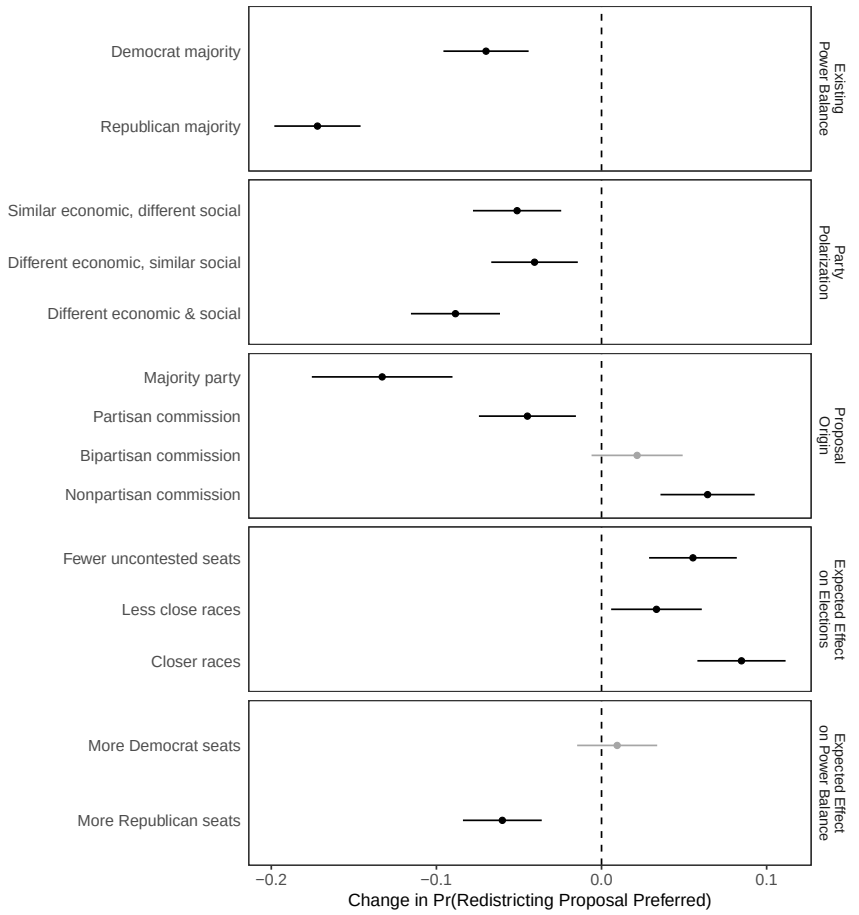
To test Hypothesis 2.1, I first estimate the average marginal component effects (AMCEs) — the causal effect of the attribute values on the change in the probability that a given redistricting scenario is preferred. Figure 2.3 presents the results. Since Hypothesis 2.1 suggests we should not expect differences across party identification, I focus on the AMCEs, but in Appendix A, I also explore the conditional AMCEs. In all figures, point estimates and 95% confidence intervals are shown and significant estimates are in black.

Figure 2.3 provides partial support for Hypothesis 2.1 that in the absence of party polarization, on average, fairness concerns will outweigh partisanship. On average, concerns about fairness affect people's decision regarding redistricting scenarios at least as much as partisanship. Considering fairness in terms of the redistricting proposal's *effect on elections*, respondents were 5.5% more likely to prefer redistricting scenarios which result in fewer uncontested seats compared to those which are expected to increase the number of uncontested seats. Additionally, respondents were 8.5% more likely to prefer redistricting scenarios that are expected to result in closer elections on average relative to those that are expected to increase the number of uncontested seats. Since no party polarization is the reference category, this suggests that in the absence of polarization, fairness in terms of election outcomes does affect the evaluation of redistricting scenarios, on average.[24]

Similarly, Figure 2.3 shows that, on average, fairness in terms of the *proposal origin* also

[24] In Appendix A, I show that these average effects also generally hold when conditioning on party identification, however, strong Republicans are noticeably different from other groups.

Figure 2.3: Effects of Attributes on the Probability Redistricting Scenario is Preferred



Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Baseline attribute values are: divided control, similar economic and social, bipartisan legislative committee, more uncontested seats, and no change.

affects respondents' redistricting choices. Respondents are 6.4% more likely to prefer proposals made by nonpartisan districting commissions relative to those made by a bipartisan committee of state legislators. Moreover, on average, respondents are less likely to support redistricting scenarios when proposals are made by the majority party in the state legislature or by partisan commissions, relative to those made by a bipartisan committee of state legislators.[25] These results provide nuance to existing studies that ask respondents which redistricting method is fairer. First, these results demonstrate that respondents care about fairness both in terms of the redistricting process and in terms of the outcome. Second, they suggest that the type of commission matters and simply providing respondents with a choice

between legislator-based proposals and those made by "independent districting commissions" obscures important variation.

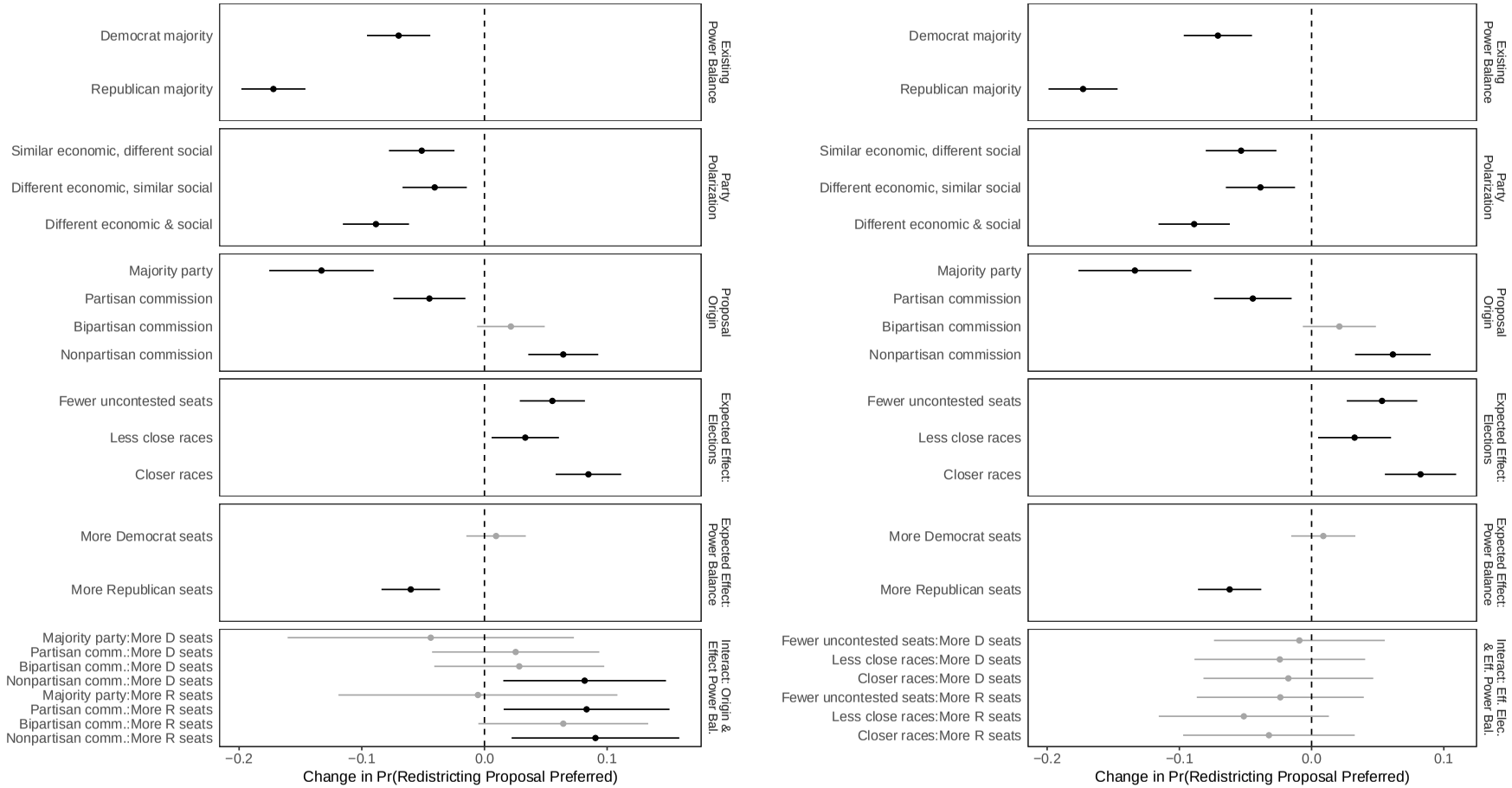
The effects of the attribute values that operationalize fairness are similar in magnitude to, if not larger than, the effects of partisan attribute values. This is a partial confirmation of Hypothesis 2.1 since fairness clearly has an effect but does not entirely outweigh partisanship in the absence of polarization, as hypothesized. On average, respondents are less likely to prefer scenarios in which either party has a majority relative to divided control and are less likely to prefer scenarios in which the Republicans would receive an increased seat share. This and the larger negative effect of Republican majorities may be due to the larger number of Democrats in the sample. Although Hypothesis 2.1 suggests that preferences for fairness should outweigh partisan preferences in the absence of party polarization, that both preferences are of similar magnitude on average provides partial support for the hypothesis.

Recall, Hypothesis 2.1 states that preferences for fairness will outweigh partisan preferences in the absence of party polarization, *even if the proposed redistricting scenario would result in a change in the legislative balance of power.* Figure 2.4 tests this additional implication by interacting the fairness and *effect on the legislative balance of power* attributes and computing the ACIEs. The left panel shows the results when the *proposal origin* is interacted with the *effect on the balance of power*, while the right panel shows the results when the *expected effect on elections* is interacted with the *expected effect on the balance of power.*[26]

If the proposed mechanism is correct, most of these interactions should be insignificant (the average marginal effect of the fairness attribute should be relatively constant across values of the *partisan outcome* attribute), which is indeed what Figure 2.4 shows. The only exceptions come when interacting *proposal origin* and the *expected effect on balance of power.* Respondents are, on average, more likely to support redistricting proposals that result in

[25] Again, Appendix A shows that these conclusions generally hold when conditioning on party identification (strong Republicans are the only group for which this is not true).
[26] I don't include a triple interaction because the baseline for the polarization attribute is no polarization.

Figure 2.4: Effects of Attributes (Including Interactions) on Probability Redistricting Scenario is Preferred



Note: Left panel shows results from model interacting *proposal origin* and *partisan outcome* while right panel shows results from model interacting *effect on elections* and *partisan outcome*. Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Baseline attribute values are: divided control, similar economic and social, bipartisan legislative committee, more uncontested seats, no change, and bipartisan legislative committee:no change (left) or more uncontested seats:no change (right).

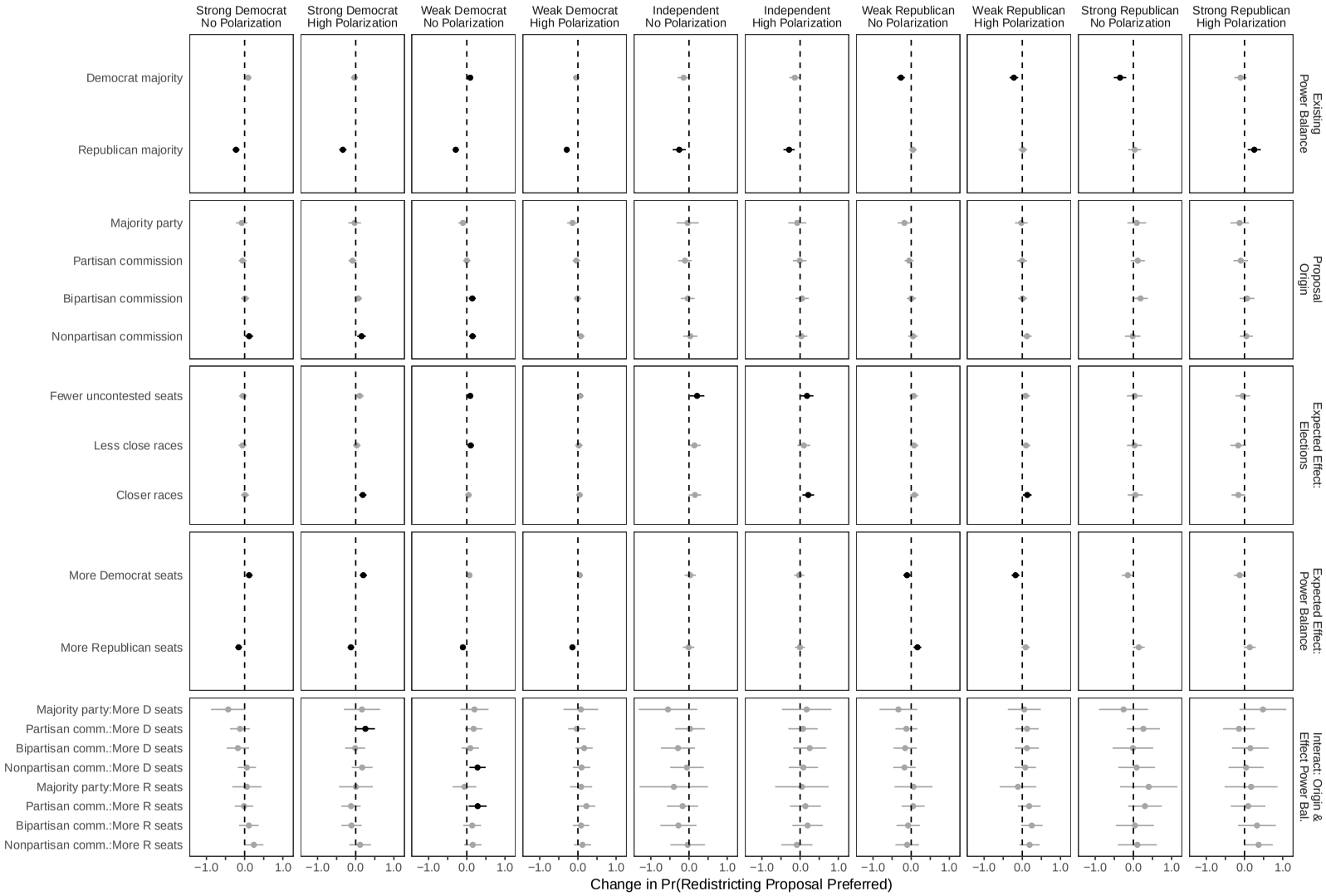
more Democratic or Republican seats if the proposal is made by a nonpartisan commission. This may reflect a view that it is acceptable for one party to benefit as long as the proposal was made in a fair (bipartisan or nonpartisan) way. The right panel in Figure 2.4 shows that none of the interaction terms are significant when I interact the *expected effect on elections* with the *expected effect on the balance of power*, as expected. Overall, these results generally support the implication from Hypothesis 2.1 that under no party polarization, preferences regarding fairness should be relatively constant even if the scenario affects the legislative balance of power.[27] In sum, there is partial support for Hypothesis 2.1 — attributes relating to fairness certainly affect the likelihood that a scenario will be preferred, but these effects do not entirely outweigh the effects of attributes relating to partisanship.

I now turn to Hypothesis 2.2, which states that as party polarization increases, strong partisans will, on average, be more likely to prioritize partisanship over fairness when evaluating redistricting proposals. Rather than testing this with a triple interaction — between the *party polarization*, fairness, and *effect on the balance of power* attributes[28] — which are notoriously difficult to interpret, I rely on subsetting. I subset the profiles (redistricting scenarios) according to the level of *party polarization*. To focus on the most relevant scenarios, I use those profiles in which there was no polarization (the parties were similar on both economic and social issues) or there was high polarization (the parties were different on both issues). Since I also subset according to respondent party identification, there are a total of ten subsets on which I run a model with a single interaction between the fairness[29] and *expected effect on balance of power* attributes.

Figure 2.5 shows the results of this analysis interacting the *proposal origin* and *expected effect on balance of power* attributes. There is some indication that the effect of the attributes that operationalize fairness changes with different levels of polarization. For example, strong Democrats, independents, and weak or leaning Republicans are all more likely to prefer sce-

[27] Appendix A presents analogous results when I estimate the ACIEs conditional on party identification. Again, the conclusions generally hold.
[28] This is effectively a quadruple interaction since I subset by respondent party identification.
[29] As above, I conduct this analysis twice — with *proposal origin* and the *expected effect on elections*.

Figure 2.5: Effects of Attributes (Including Interaction Between Proposal Origin and Partisan Outcome) on Probability Redistricting Scenario is Preferred by Party ID and Party Polarization



Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Weak partisans include independents who lean towards a party. Baseline attribute values are: divided control, bipartisan legislative committee, more uncontested seats, no change, and bipartisan legislative committee:no change.

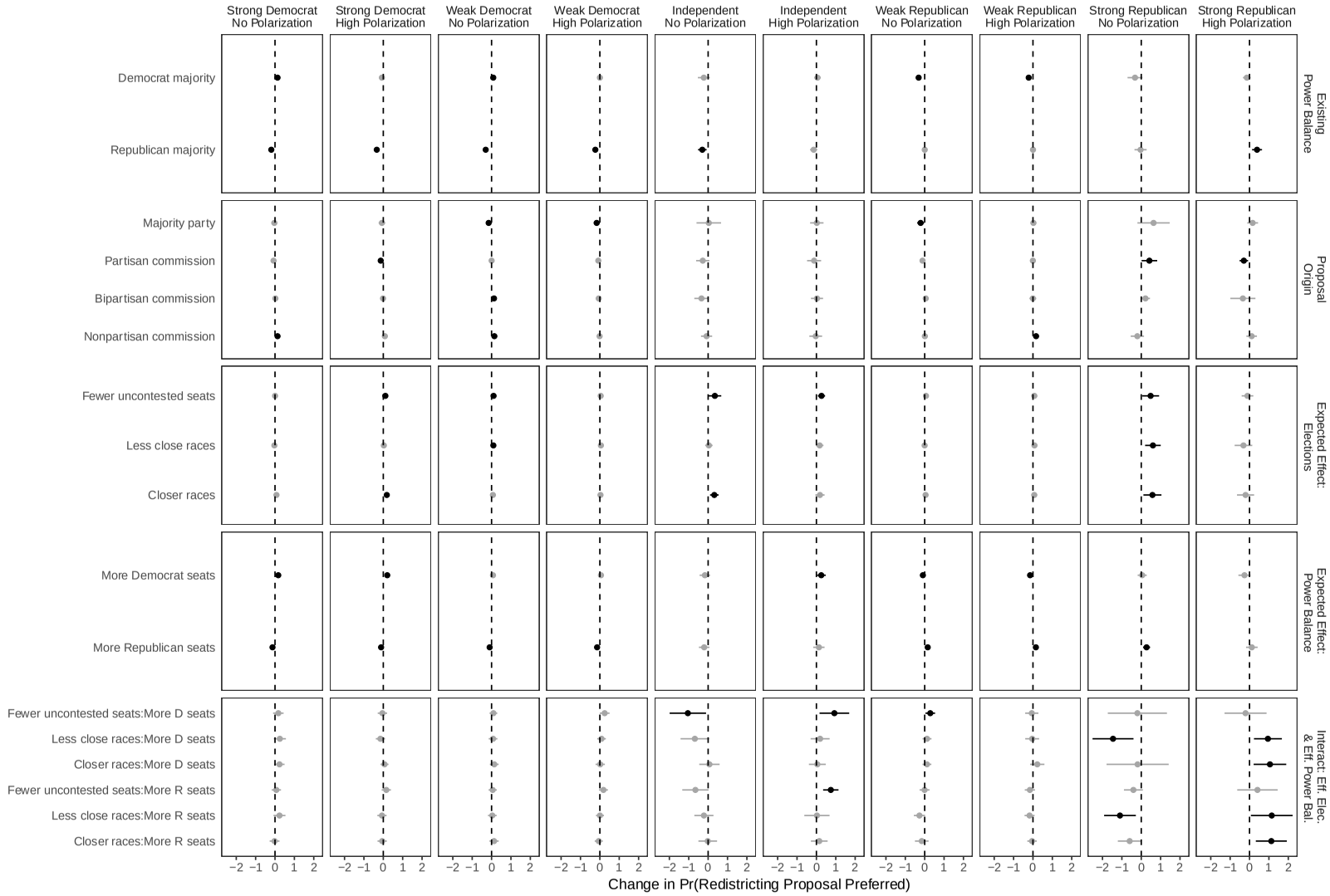
narios with close races relative to those with more uncontested seats, but only under high polarization. This is a surprising result not only because the hypothesis suggests that if anything, it should be in low polarization situations that respondents prefer fair redistricting proposals, but also because strong and weak partisans from opposite sides of the political spectrum are behaving more similarly than strong partisans. More in line with the theory, weak or leaning Democrats are more likely to prefer scenarios with fewer uncontested seats, but only when there is no party polarization. Similarly, independents prefer scenarios with fewer uncontested seats regardless of the level of polarization, as expected — independents should be most likely to prefer fair redistricting, even in the context of high party polarization, rather than redistricting that results in a particular partisan outcome.

There is also some evidence that strong partisans (specifically, strong Democrats) are willing to trade fairness for preferred partisan outcomes. Strong Democrats are more likely to prefer proposals made by partisan committees if Democrats benefit (with an increased number of seats), but only under high polarization. This suggests that strong Democrats are willing to trade fairness (in terms of nonpartisan redistricting proposals) in order to get favorable partisan outcomes, but only when there is sufficient party polarization.

Figure 2.6 shows the results of an analogous analysis in which the proposal's *expected effect on elections* is interacted with the *expected effect on the balance of power*. Again, there is some evidence that some groups of respondents are willing to trade fairness for partisan advantage, conditional on the level of party polarization. Strong Republicans are more likely to prefer redistricting scenarios with fewer uncontested seats, relative to those with more uncontested seats, but only in the absence of party polarization. Similarly, strong Republicans are more likely to prefer scenarios with close races relative to those with more uncontested seats, but only when there is either no party polarization, or there is polarization and the proposal is expected to increase the number of Republican seats.

Overall, the results are mixed with regard to Hypothesis 2.2. There is some evidence that partisans trade fairness for partisan gain when party polarization is high, but it is inconsis-

Figure 2.6: Effects of Attributes (Including Interaction Between Effect on Elections and Partisan Outcome) on Probability Redistricting Scenario is Preferred by Party ID and Party Polarization



Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Weak partisans include independents who lean towards a party. Baseline attribute values are: divided control, bipartisan legislative committee, more uncontested seats, no change, and more uncontested seats:no change.

tent. There are two possible reasons for this. First, the necessary subsetting significantly reduces sample size. The smallest sample size in this analysis, with only 228 profiles, is that for strong Republicans under high polarization. Relative to the much larger sample sizes for the earlier analyses, some of the insignificant and thus inconsistent results in this analysis could be a result of the reduced sample size due to subsetting. Second, it is possible that the polarization treatment is not strong enough. The earlier results suggest the level of party polarization does affect the likelihood respondents will choose a given redistricting proposal and there is some evidence from the manipulation checks (discussed below) that party polarization influenced decisions. Nevertheless, perhaps this treatment is simply not strong enough in an era of high party polarization at the national level.

2.3.1 Robustness

I take several approaches to probe the robustness of these results.[30] First, I rerun all analyses excluding observations from the pilot, fielded five days prior to the full experiment. Although the survey was unchanged between waves and I am not aware of any events (such as Supreme Court decisions or major news coverage) that would influence the results, perhaps the timing could affect responses. Overall, the results excluding the pilot are very similar to those presented above. Second, I rerun the analyses excluding those who failed the manipulation check (described below) at the end of the experiment.[31] Failing a manipulation check may be an indication that a respondent was not paying attention to the experiment. The results are very similar when these observations are excluded. Third, I rerun the analyses using the profile ratings by respondents on a seven-point scale rather than the forced choice.[32] Thus, I use *Scenario Rating* rather than *Scenario Preferred* as the dependent variable. While this changes the interpretation of the results, the directions of the effects are similar.

Finally, I conduct a non-causal analysis to further probe the main results. For this anal-

[30] See Appendix A for full details and results. Appendix A also includes tests of the identification assumptions for the main analyses.
[31] Specifically, I exclude the two profiles (scenarios) from each task with a failed manipulation check.
[32] See replication materials for the results.

ysis, I exploit what is, to the best of my knowledge, a unique manipulation check embedded in my experiment.[33] At the end of each task, I asked respondents an open-ended question about why they chose the scenario they did when they were forced to make a choice.[34] These responses allow me to examine the correlation between a respondent's partisanship and the likelihood that they reportedly based their choice on concerns about fairness or partisanship.

The words "gerrymandering" and "fairness" did not appear anywhere in the survey. Nevertheless, many respondents referenced these concepts in their manipulation checks. This suggests respondents understood that the conjoint attributes affected election fairness. Moreover, many respondents explicitly referenced the tradeoff between fairness and partisanship central to the theory. For example, one respondent wrote, "I absolutely do not think the first scenario is fair — but I'm a [D]emocrat, and it favors [D]emocrats so...," implying that they resolved the tradeoff by prioritizing partisanship. Other respondents resolved the tradeoff in the opposite way, saying something similar to, "any redistricting plan should be fair even if it does not favor my chosen party."

Additionally, the manipulation checks suggest that respondents understood the various ways I operationalized fairness. In addition to respondents who expressed a general concern about fairness or an aversion to gerrymandering, there were others who explicitly connected the attributes operationalizing fairness to the general concept. Some respondents expressed a preference for fairness as it relates to the *proposal origin*. For example, one respondent wrote, "I believe non-partisan redistricting is very important, even if the alternative is redistricting that benefits the party that I identify with." Other respondents connected the proposal's *effect on elections* to their preferences regarding fair elections. To illustrate, one respondent wrote, "I oppose scenario two because of the larger number of uncontested seats. I will accept a larger number of Democrats if that is the price to be paid for closer political contests."

Relatedly, respondents correctly understood the implications of the various scenario at-

[33] I thank Adam Glynn for suggesting this manipulation check.
[34] The exact prompt was: "Please briefly explain why you chose the scenario you did when you were asked to choose between the two."

tributes for partisan advantage. Respondents who prioritized partisanship may have done so for several reasons. First, they may have preferred the general partisan hue of the scenario. Second, they may have preferred the *existing balance of power*, saying something like, "I want my party to have a majority." Third, they may have based their decision on the fact that the proposed redistricting plan would increase the seat share of a particular party. For example, one respondent wrote, "I would rather have a bipartisan agreement but not if that means having more [R]epublicans" (i.e., an increased seat share for Republicans).

While these manipulation checks indicate that respondents broadly interpreted the components of the experiment as intended, I also use them to probe the experimental results. To do this, I use the three categories of fairness (general, proposer- or process-related, or outcome-related) and partisanship (general, related to the existing balance of power, or related to the partisan outcome) described above to code responses to the manipulation checks. I then run two sets of models, with the respondent as the level of analysis.[35]

First, I regress an indicator (*Fairness*) for whether a respondent referenced fairness concerns in any of their manipulation checks on a set of indicators for party identification. Second, I regress an indicator (*Partisanship*) for whether a respondent referenced partisan concerns in their manipulation checks[36] on a set of indicators for party identification. Concerns about fairness and partisanship are not mutually exclusive (there are cases in which both preferences could be simultaneously satisfied and in which the respondent referenced both). Nor are they the only possible responses to the manipulation checks (see Appendix A for details). Therefore, I rerun the analysis with each outcome variable. Additionally, I run both analyses with two different partisanship indicators. In the first set, I group strong partisans, weak partisans and leaners, and independents together since the theory suggests that it is strong partisans who should evaluate these proposals differently from other groups. However, I further explore these results by running the analyses using a full set of indicators for the seven-point partisanship scale. I report results from linear probability models without

[35] I exclude those who failed their attention checks or whose party identification is coded as don't know.
[36] Those indicating they based their choice on the level of party polarization are not included in this group.

controls for ease of interpretation, but the results with controls or using logistic regression are substantively similar.[37]

Table 2.2 shows the results of the analysis using strength of partisanship as the independent variables. Column 1 shows the results of a model regressing an indicator for whether the respondent referenced fairness or an attribute operationalizing fairness at least once in any of the three manipulation checks (*Fairness*) on strength of partisanship. While none of the coefficients are statistically significant, the coefficient for strong partisans is in the expected direction (strong partisans should be less likely than independents to base their decision on fairness). Column 2 shows an analogous regression with an indicator for whether the respondent referenced partisanship or an attribute pertaining to partisanship at least once in the manipulation checks (*Partisanship*) as the dependent variable. Here, strong partisanship is correlated with an increased likelihood of reporting that one's decision in the conjoint was based on partisanship. Strong partisans are 41% more likely than independents to reference partisanship. As expected, the coefficient for weak partisans or leaners is smaller than that for strong partisans, suggesting that this group is more likely than independents, but less likely than strong partisans to reportedly base their decision on partisanship.

Table 2.3 shows the results of the analysis with party identification as the main independent variables. Column 1 shows the results with *Fairness* as the dependent variable. Interestingly, independents who lean Democratic are significantly more likely to reference fairness in their manipulation checks than pure independents. However, strong Republicans are significantly *less* likely to mention fairness in their manipulation checks than independents. This suggests that the null results for strong partisans in column 1 of Table 2.2 are likely due to the larger number of strong Democrats in the sample. Column 2 of Table 2.3 shows the results of the model regressing *Partisanship* on party identification. All partisans are significantly more likely to mention partisanship concerns than independents. However, strong partisans at both ends of the spectrum are more likely than any other group to refer-

[37] The controls are indicators for age, state of residence, urban residence, gender, race, education, income, occupation, interest in politics, and political knowledge.

Table 2.2: Strength of Partisanship and Mentions of Fairness or Partisanship

	Dependent variable:	
	Fairness	Partisanship
	(1)	(2)
Strong Partisan	−0.014	0.406***
	(0.041)	(0.038)
Weak Partisan/Leaner	0.037	0.288***
	(0.038)	(0.036)
Constant	0.488***	0.345***
	(0.035)	(0.033)
Observations	1,786	1,786
R^2	0.002	0.060

Note: Linear probability models. Excluded category is pure independent. Standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01

ence partisanship. This is suggestive evidence that strong partisans are more likely to prefer favorable partisan outcomes over fair redistricting proposals.

This analysis is not causal so we should be wary of drawing strong conclusions from it. At the very least, however, these results suggest that respondents recognize that redistricting proposals have implications for fairness and partisanship and make decisions accordingly.

Table 2.3: Party ID and Mentions of Fairness or Partisanship

	Dependent variable:	
	Fairness	Partisanship
	(1)	(2)
Strong Democrat	0.051	0.396***
	(0.042)	(0.040)
Weak Democrat	−0.009	0.304***
	(0.044)	(0.041)
Independent Lean Democrat	0.116**	0.222***
	(0.046)	(0.043)
Independent Lean Republican	0.075	0.287***
	(0.054)	(0.051)
Weak Republican	−0.010	0.339***
	(0.047)	(0.044)
Strong Republican	−0.198***	0.431***
	(0.053)	(0.050)
Constant	0.488***	0.345***
	(0.035)	(0.033)
Observations	1,786	1,786
R^2	0.025	0.065

Note: Linear probability models. Excluded category is pure independent. Standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01

2.4 Conclusion

Free and fair elections are essential for democratic stability. Moreover, citizens are supposed to be the last line of defense protecting democracy. However, there has been relatively little work studying preferences regarding fair elections in the general public. Specifically, I study attitudes towards redistricting in the United States. This is especially important given that gerrymandering is pervasive and the Supreme Court has been hesitant to curb it.

I argue that people's preferences regarding redistricting are dependent on their policy preferences (and partisanship) as well as their preferences about fairness. However, how individuals balance these potentially competing preferences depends on the level of polarization between political parties. Specifically, in the absence of party polarization, fairness

should dominate partisan concerns when people evaluate redistricting proposals. However, as polarization increases, strong partisans should be more likely than other groups to trade fair redistricting proposals for those that result in preferred partisan outcomes.

I test this theory using a conjoint experiment with over 2,100 respondents. This design allows me to make causal inferences (not generally attempted in the existing literature on public opinion about redistricting). I find evidence that partisan concerns and fairness both influence opinions about redistricting and some evidence that the effects are conditional on the level of party polarization. That partisanship and party polarization affect citizens' commitment to democracy is in accordance with the redistricting literature that suggests gerrymandering opinions are affected by whether one perceives one's party as benefiting (e.g., Fougere, Ansolabehere, and Persily 2010; Tolbert, Smith, and Green 2009) as well as the literature that suggests polarization contributes to democratic backsliding (e.g., Bermeo 2003; Graham and Svolik 2020; Linz 1978, 1990; Sani and Sartori 1983; Svolik 2020). However, my results suggest that partisanship and party polarization may not be as pernicious as previously thought when it comes to opinions about redistricting — strong partisans do still care about fairness and party polarization does not have a uniformly negative effect.

My results also suggest strong Republicans may be different from other groups. There is some evidence (see Appendix A) that they are less interested in fairness even under no polarization, however the tests of Hypothesis 2.2 paint a more hopeful picture. One possibility is that people (across the partisan spectrum), view gerrymandering as primarily the purview of the Republican Party. Although inaccurate, such a belief may explain why strong Republicans exhibit weaker preferences for fairness than strong Democrats and why strong Democrats are more likely than expected to prefer fair redistricting. If people believe the Republicans are responsible for gerrymandering, they may view increasing fairness as advantaging the Democrats (and harming Republican interests). Due to a desire to avoid biasing the results by asking questions about gerrymandering perceptions prior to the experimental treatment and a desire to avoid post-treatment bias by asking such questions afterwards,

I cannot explore this possibility. Thus, one potentially fruitful avenue for future research would be to investigate how perceptions about gerrymandering (and particularly which party is responsible) affect opinions.

Although the theory focuses on redistricting in the United States, similar dynamics may be at play in other countries and regarding other aspects of electoral systems. For example, significant emphasis is put on fairness in campaigns around proposals for changing the electoral formula (e.g., the longstanding campaign for proportional representation in the United Kingdom, multiple referenda on proportional representation in British Columbia, or the campaigns for ranked-choice voting in U.S. states). However, my research suggests that rather than assume all voters will respond positively to campaigns built around fairness, we should more carefully consider how preferences regarding fairness interact with partisanship and party polarization in these contexts.

Chapter 3

Public Support for Electoral Reform: The Role of Electoral System Experience

What affects citizen preferences about electoral rules and under what conditions will they support changes to those rules? These questions are critical because in established democracies, citizens increasingly directly influence electoral rules through initiatives or referenda. For example, in New Zealand in 1993, voters overrode the preferences of political elites in two referenda, leading to the introduction of a mixed member proportional (MMP) system for national legislative elections (Vowles 2005). More recently, a 2016 initiative in Maine resulted in the adoption of ranked choice voting (Santucci 2018).[1] Moreover, even when electoral reform is enacted through the legislature, parties are responsive to voters and are expected to take their preferences into account (Weingast 1997).

Since political parties prefer beneficial electoral rules and support reform when they believe they can increase their seat share (e.g., Benoit 2007; Benoit and Hayden 2004; McElwain

[1] Similar votes have occurred in Canada (British Columbia in 2005, 2009, and 2018 and Ontario in 2017), Italy (1991 and 1993), and the United Kingdom (2011), among others (D'Alimonte 2005; Qvortrup 2012; Renwick 2010).

2008; Remington and Smith 1996), it is natural to suppose that citizens would similarly have preferences over electoral rules based (at least partially) on partisan preferences (Ahlquist et al. 2018; Bowler and Donovan 2007; Lamare and Vowles 1996; Plescia, Blais, and Högström 2020). But how do citizens know which electoral rules will benefit their preferred party? Generally, we expect the public to be relatively poorly informed about institutional design. One heuristic citizens can use to determine if a proposed electoral system will benefit their party is cues from parties, public officials, and other elites (Ahlquist et al. 2018; Clarke et al. 2013; Druckman, Peterson, and Slothuus 2013; Slothuus and Vreese 2010; Whiteley et al. 2012). In other words, individuals can adopt the position of a trusted party or leader. However, at times parties avoid giving cues (Renwick 2010) or political elites are divided on the issue of electoral reform (LeDuc 2011), such that there may be conflicting cues sent by a single party. In other areas of public policy, however, we know that citizen experience plays an important role in shaping opinion (e.g., Lerman and McCabe 2017). When party cues are unavailable or weak, I argue citizens will rely on their own experience with electoral systems at different levels of government to determine whether the proposed rules will benefit their preferred party and thus whether to support national-level electoral reform.

The possibility of electoral reform creates inherent uncertainty for citizens (Andrews and Jackman 2005). In many democracies, however, elections occur at multiple levels of government. Of democratic country-year observations in V-Dem, 67% have regional governments and 89% have local governments that are at least partially elected (Coppedge et al. 2016). Moreover, different rules are increasingly used for subnational versus national elections. For example, some areas in the United States use ranked choice voting for local or state-level elections, while single-member district plurality (SMDP) is used for Congressional elections. In France, while a two-round runoff system is used for National Assembly and presidential elections, some subnational elections use proportional representation (PR).[2] This experience

[2] Relatedly, some countries use multiple systems for nationwide elections. France uses a two-round runoff for National Assembly elections, but PR for European Parliament elections. Australia uses the alternative vote for House of Representatives elections and the single transferable vote for Senate elections.

with subnational electoral rules may inform citizen preferences about electoral reform at the national level.

Experience with alternative electoral systems can affect support for electoral reform because it provides information regarding how parties and policies are likely to be affected, reducing the uncertainty associated with the proposal. However, the direction of that effect is conditional on the type of experience. Partisan bias (the tendency to favor electoral systems that benefit one's preferred party), combined with information provided by experience, can either increase or decrease the likelihood that individuals support electoral reform because that experience may have been either positive or negative. Individuals who have positive experiences (i.e., their preferred party benefits from the electoral system) will be more likely to support reform, while those who have negative experiences will be less likely to do so. Additionally, I argue that for experience to affect support for reform, it must be experience with a *similar* electoral system to that which is being proposed because only such experience provides the relevant counterfactual. Furthermore, the role of institutional experience will be especially important when party cues are diluted or absent. This is when citizens must rely on their experience rather than other sources of information.

To test the theory, I leverage subnational variation in electoral rules in the United Kingdom (UK) — where electoral reform has been a recurring issue over the last 100 years, even while the use of SMDP for House of Commons elections has remained largely unchanged. Most recently, the UK held a referendum in 2011 on changing the electoral system used to elect members of Parliament (MPs) from SMDP to the alternative vote (AV) — a change rejected by voters (White 2011; Whiteley et al. 2012). However, UK voters have experience with different electoral systems. Following the referendum, a Scottish Labour MP noted that prior to the vote, there had been "an assumption that the Scottish electorate might be more open to change [be]cause it already had experience of different voting systems."[3] I build on this idea and explore the theoretical and empirical connection between experience

[3] Personal interview, May 11, 2011, Portcullis House, London, UK.

and support for electoral reform.

Specifically, I use the introduction of mixed member proportional (MMP) electoral systems in Scotland and Wales and difference-in-differences designs to estimate the causal effect of experience with different electoral systems. Since 1999, voters in Scotland and Wales have used MMP for elections to the Scottish Parliament and the National Assembly for Wales. Therefore, we can examine whether their support for electoral reform remains similar to that of their geographic neighbors in Northern England and the Midlands who do not have experience with MMP. Questions about electoral reform on the British Social Attitudes survey from 1986 to 2015 allow for difference-in-differences designs to test the effect of experience with MMP at the regional level on support for electoral reform at the national level.

The results demonstrate that experience with an electoral system similar to that under consideration affects support for electoral reform. Regional elections under a set of rules that include a proportional component lead to changes in support for national-level reforms from the current majoritarian system to a proportional system. Specifically, when a respondent's preferred political party provides weak and confusing cues (operationalized using election manifestos) and the experience did not benefit the party (i.e., the party performed worse under the alternative rules), support for reform decreases. Respondents do not support rule change when they perceive it as harming their preferred party and they especially rely on their experience with alternative rules when clear party cues are absent.

This study builds on work on electoral reform that has traditionally focused on the incentives of parties and legislators to change the rules that govern their election (e.g., Bawn 1993; Benoit 2007; Benoit and Hayden 2004; McElwain 2008; Remington and Smith 1996). Joining the literature examining public opinion about electoral reform (e.g., Banducci and Karp 1999; Norris 2011; Plescia, Blais, and Högström 2020; Renwick 2010, 2011), I probe the interaction between what parties want and what constituents prefer. As electoral rules are increasingly subject to referenda or the focus of electoral campaigns and promises (e.g., as in the case of the Canadian Liberal Party), understanding what determines public support

for electoral reform is increasingly important. The findings — that citizens prefer rules that benefit their preferred parties — have implications for how reform movements can build from the bottom up. There are also darker implications for popular incumbents who seek to manipulate electoral rules for their own gain (Ahlquist et al. 2018; Graham and Svolik 2020) and the perceived legitimacy of electoral rules — points to which I return in the conclusion.

3.1 The Effect of Electoral System Experience

Changes to the electoral system, particularly those to the electoral formula, do not only alter the translation of votes into seats. They also have the potential to affect the party system (e.g., Benoit 2007; Duverger 1963; Shugart and Taagepera 2017), the type of government that forms (single-party or coalition, minority or majority; e.g., Lijphart 1999; Shugart and Taagepera 2017), the strategic decisions of voters (e.g., Cox 1997), and by extension, policy outcomes (e.g., Bawn 1993).

Thus, electoral reforms create inherent uncertainty for voters, party leaders, and even experts (Andrews and Jackman 2005; Renwick 2010). While ordinary people may not understand the intricacies of electoral rules, they nevertheless realize that the rules have a crucial effect on their preferred parties' fortunes. Moreover, they prefer rules that result in more seats for their preferred party (e.g., Bowler and Donovan 2007; Lamare and Vowles 1996; Plescia, Blais, and Högström 2020) — what I call partisan bias. At the very least, people do not want a system that disadvantages their party. Citizens observe the electoral outcome (national results) for parties at the national level under the current rules (electoral system A). But a reform proposal forces citizens to consider a counterfactual scenario: national results under alternative rules (electoral system B). By comparing these two scenarios, citizens can determine which outcome, and therefore electoral system, they prefer. However, since national-level electoral rules rarely change, citizens are unlikely to have the opportunity to observe the counterfactual (national outcomes under system B). Given such uncertainty,

under what conditions will people support electoral reform?

Recall, democracies commonly select public officials at multiple levels of government using elections (Coppedge et al. 2016). Moreover, these elections do not always use the same electoral system as their national counterparts and this phenomenon is not limited to federal systems. Countries increasingly use different rules for subnational versus national elections or for different national elections. Australia, for example, uses different rules for elections to the House of Representatives and the Senate, while the United States, Canada, Chile, and France mix different rules across levels of government.

For citizens with experience using different electoral systems, the uncertainty that typically characterizes institutional change decreases. Having observed electoral results under a different set of rules — say, regional results under electoral system B — the unobserved counterfactual, national results under system B, becomes easier to assess. This may help supporters of parties that have previously won nationally under electoral system A overcome their status quo bias (Bowler and Donovan 2007). Importantly however, the regional results under system B may have been beneficial or detrimental to an individual's preferred political party. Thus, the direction of the effect of experience — increasing or decreasing support for reform — will depend on the nature of the experience. Citizens with experience with alternative electoral systems are more likely to support reform when the experienced system benefited their preferred party, while those with experience with rules that harmed their party will be less likely to support reform.

This argument supposes that outcomes under alternative systems (used at a different time, in a different context, or both) are perceived as informative about the current electoral environment. While the extent to which the other outcomes are actually informative will vary, given that even experts struggle to predict the effect of new electoral rules (Andrews and Jackman 2005), the public is likely to use those outcomes — particularly recent ones — as heuristics for understanding the likely effects of a proposed change and determining if one's party was successful (Plescia 2019; Stiers, Daoust, and Blais 2018).

However, experience with multiple electoral systems is not sufficient to induce changes in support for electoral reform. Rather, what affects support is experience with electoral systems *similar* to the proposed reform. This is because simply using multiple electoral systems does not necessarily provide any additional information about the proposed reform: if one has used SMDP, AV, and a two-round runoff system — all majoritarian systems — this does not help one understand PR. Experience using a variety of majoritarian systems should not affect support for a proportional system. Instead, what provides information, reduces uncertainty, and therefore affects the likelihood that an individual supports a particular electoral reform is experience with a system similar to that which is being proposed.

Hypothesis 3.1. Individuals will be more (less) supportive of electoral reform if they have a positive (negative) experience with an electoral system similar to that which is being proposed.

Here, "positive experience" means that the alternative rules resulted in favorable outcomes for the individual's preferred party (I operationalize this for the UK context below). Given partisan bias (individuals want systems that benefit their party), whether experience increases or decreases support for reform is conditional on whether that experience suggested their party would be benefited or harmed. Importantly, partisan bias is conceptually distinct from party cues. Party cues may also affect an individual's support for electoral reform, but the mechanism is quite different. Party cues affect opinions when citizens adopt the stated position of the party with which they identify or a trusted party leader. Citizens adopt these positions either as an informational shortcut in a complex environment or due to motivated reasoning (Druckman, Peterson, and Slothuus 2013; Leeper and Slothuus 2014; Slothuus and Vreese 2010). Regardless of the psychological mechanism behind citizens adopting party cues, fundamentally, party cues rely on parties taking clear positions on an issue and communicating those positions to the public. Unlike party cues, partisan bias does not rely on the parties as mediators. Individuals simply want electoral systems that benefit their party, and this desire affects whether they support reform.

However, we know that party cues affect opinions generally and support for reform specifically (Ahlquist et al. 2018; Clarke et al. 2013; Druckman, Peterson, and Slothuus 2013; Slothuus and Vreese 2010; Whiteley et al. 2012). For example, in the 1993 Italian referendum, parties took explicit stances on the proposed electoral reform in an effort to influence the vote of their supporters (Renwick 2010). Undoubtedly, citizens obtain information about electoral reform proposals from multiple sources, including parties or trusted public officials. Moreover, party cues may be easier sources of information for citizens to understand compared to experience with electoral systems that may not be exactly the same as that under consideration. However, we also know that, at times, parties either avoid the issue or give mixed signals about their preferences. In the 1991 Italian referendum, for example, some parties deliberately avoided discussing the issue in an effort to "starve the referendum of publicity" (Renwick 2010, 173). More generally, LeDuc (2011) notes that elites may be divided on the issue of electoral reform, making party cues unavailable or confusing.

Therefore, I expect that experience will be most important — have the largest effect — in cases where people are not exposed to clear, consistent party cues (which I operationalize using party election manifestos). It may be that their party has not sent any cues, or has sent inconsistent cues over time, or is simultaneously sending multiple cues. These are all cases in which we should expect experience to have a greater effect on support for reform compared to situations in which citizens receive clear, consistent, and unified party cues.

Hypothesis 3.2. The effect of electoral system experience will be larger when clear, consistent party cues are absent.

I turn now to the research design. After introducing the context in which I test these hypotheses, I operationalize the concepts of positive experience and party cues, explicitly stating the implications of the theory for this case before presenting the results.

3.2 Research Design

3.2.1 Data and Empirical Strategy

I use data from the United Kingdom, which is ideal for several reasons. First, there is subnational electoral system variation. Although single-member district plurality (SMDP) is used nationwide for elections to the House of Commons, other electoral systems have been introduced regionally for selecting members of other bodies. Seven electoral systems are used in the UK — the alternative vote (AV),[4] closed-list PR, mixed member proportional (MMP; called the additional member system, AMS, in the UK),[5] the multiple non-transferable vote (MNTV; also called the block vote),[6] SMDP, the single transferable vote (STV), and the supplementary vote (see Table 3.1). The introduction of these electoral systems, although not randomly assigned, was not directly determined by voters.[7] Thus, I am able to proxy for the electoral systems with which an individual has experience using the region in which they live and the year of the survey.

Second, during the period my data covers (1986–2015), there were three main nationwide parties in the UK — the Conservatives, Labour, and the Liberal Democrats — which gave their supporters cues regarding the electoral system to varying degrees. This allows me to explore the extent to which experience with different electoral systems is particularly useful in the absence of clear party cues. Third, the UK allows me to leverage reliable survey data on support for electoral reform over a long time period.

To measure individual-level support for electoral reform, I use the British Social Attitudes (BSA) survey. The BSA is an annual survey (repeated cross-sections) of adults in Great Britain (NatCen Social Research 2019). Since the proliferation of electoral systems did not

[4] AV is used for by-elections when a single seat needs to be filled for bodies that generally use STV.
[5] I treat MMP as a mixed system since it contains both majoritarian and proportional elements (e.g., Bormann and Golder 2013).
[6] Some sources describe all local elections (which use a mix of SMDP and MNTV) as first-past-the-post.
[7] There were referenda to establish the Scottish Parliament, National Assembly for Wales, and Greater London Authority (consisting of the London Assembly and Mayor of London), however voters were not able to choose the electoral systems (Johnston 2016).

Table 3.1: Electoral Systems in the UK (1980–Present)

Electoral System	Elected Body	Region	Years
Alternative Vote	Local by-elections	NI	1973[§]–present
	Local by-elections	Scot.	2007–present
Closed-List PR	European Parliament	Eng., Wales, Scot.	1999–present
MMP/AMS	National Assembly for Wales	Wales	1999–present
	Scottish Parliament	Scot.	1999–present
	London Assembly	Eng.[†]	2000–present
MNTV/Block Vote	Local elections	Eng.[*], Wales[*]	1973[§]–present
SMDP	House of Commons	Eng., Wales, Scot., NI	1950[‡]–present
	Local elections	Eng.,[*] Wales[*]	1973[§]–present
	Local elections	Scot.	1974[§]–2006
	European Parliament	Eng., Wales, Scot.	1979–1998
STV	Local elections	NI	1973[§]–present
	European Parliament	NI	1979–present
	Northern Ireland Assembly	NI	1998–present
	Local elections	Scot.	2007–present
Supplementary Vote	Mayor of London	Eng.[†]	2000–present
	Elected mayors	Eng.[†]	2002[|]–present
	Police & Crime Commissioners	Eng.,[†] Wales	2012–present

Note: Start year is *election year* in which system was first used, unless otherwise noted. NI is Northern Ireland. [*] Some areas use system. [†] Entire region does not elect body or only some areas have such a body. [‡] Before 1950, elections predominantly used SMDP, but some multi-member constituencies remained. [§] Elections under these systems occurred previously but were significantly reorganized that year. [|] First mayor elected that year, but introduction staggered over time. Sources: Audickas, Hawkins, and Cracknell (2016); Bush (1976); Electoral Reform Society (2007); McCartney and Rawlings (n.d.); Parry (2012); Sandford (2017); and UK Parliament (n.d.).

occur until after 1999 (except in Northern Ireland, all elections used SMDP prior to that year; Foster 2016; Mitchell 2005), the BSA survey allows me to track attitudes both before and after electoral systems were added. In most years, the survey prompted respondents,

> "Some people say that we should change the voting system to allow smaller political parties to get a fairer share of MPs. Others say that we should keep the voting system as it is, to produce more effective government. Which view comes closest to your own, that we should change the voting system, or, keep it as it

is?" (NatCen Social Research 2019)[8]

I use this question to construct an indicator for whether a respondent supports electoral reform. *Reform Support* takes a value of one if the respondent answered they believed the electoral system should be changed and zero if they preferred to keep the existing system.[9] This is the main dependent variable in the analyses.

The proposed alternative electoral system in the survey question is not explicitly stated. However, the question heavily implies PR with its focus on giving small parties a "fairer share of MPs" and enumerators were instructed to elaborate that the question referred to PR if asked (NatCen Social Research 2019). While MMP and PR are distinct, MMP is a mixed system such that 56 out of 129 seats (43.4%) in the Scottish Parliament and 20 out of 60 seats (33.3%) in the National Assembly for Wales are elected via closed-list PR (Audickas, Hawkins, and Cracknell 2016). Thus, MMP is similar to PR (the system they are being asked to consider) and should give respondents some information about how PR would work in practice.

I test the theory using difference-in-differences (DID) analyses, allowing me to estimate the causal effect of experience with MMP. I leverage the long temporal coverage of the BSA survey and the staggered introduction of electoral systems around the UK. The DID designs compare the difference in support for electoral reform at the national level before and after a new electoral system was adopted among those living inside the region in which the system was adopted to the difference in support before and after the change among those living just outside the region (i.e., those not exposed to the new system). First, I compare support for electoral reform among respondents living in Scotland to that among those living in Northern England on either side of 1999 when the Scottish Parliament was first introduced (exposing the Scottish electorate to MMP). Second, I compare support for reform among respondents living in Wales to that among those living in the Midlands on either side of 1999 when the

[8] See Appendix B for details and temporal coverage (1986–2015, but the question was not always included).
[9] Don't know and no response were coded as missing for the results presented here, but results are generally robust to coding these responses as support for the status quo.

National Assembly for Wales was introduced (exposing those in Wales to MMP).[10]

For the DID analyses, I use individual-level data, while the treatment occurs at the regional level. Since the type of experience is determined by one's party identification, I conduct the analyses using subsets of the BSA survey respondents based on party identification. Party identifiers are those who self-report they support a party, are closer to it, or are likely to vote for it at a general election (see Appendix B for details).[11] Those who do not identify with any party or who do not answer the question are dropped from the analysis. I separately consider Labour supporters, Conservative supporters, and supporters of other parties. I group other parties for several reasons. First, otherwise it is impossible to include supporters of the Scottish National Party (SNP) and Plaid Cymru since they are concentrated in Scotland and Wales, respectively. Second, the sample size would be extremely small in many cases if one were to consider each of the small parties separately. Finally, as I discuss below, the expectations of the direction of the effect are the same for the small parties. The parties represented in "other" in the analysis are the Alliance, British National Party, Green, Liberal, Liberal Democrat, Plaid Cymru, Respect (Scotland/Northern England sample only), Social Democratic Party, SNP, UK Independence Party and other (unspecified in the survey data) parties. However, the main results are unchanged if I restrict the other party sample to include only the three largest parties in each analysis (Liberal Democrats, SNP or Plaid Cymru, and Green) or if I use only Liberal Democrats.

I include year fixed effects and controls for interest in politics.[12] Political interest might affect both one's support for electoral reform and level of attention to and interpretation of one's experience with MMP. Moreover, interest (or the related concepts of awareness and sophistication) might also affect the way in which one receives and interprets party cues (Leeper and Slothuus 2014; Slothuus and Vreese 2010). Ideally, I would also include controls

[10] I am unable to leverage the introduction of closed-list PR (no regional variation), STV in Northern Ireland (survey does not cover the period), the supplementary vote or MNTV (their introduction was uneven within regions), or AV (used only for by-elections).
[11] Elsewhere, I use the terms identifiers and supporters interchangeably.
[12] The question used to create this variable was included in all survey waves except 1987. The results are generally robust to excluding controls.

for news consumption. This would address a concern that those outside the treated region receive some form of treatment through news coverage of elections within the treated area. I am unable to control for this possibility directly because the BSA survey does not ask consistent news consumption questions over time. Nevertheless, if this sort of spillover effect were occurring, it should result in bias towards finding null results.[13]

3.2.2 Empirical Expectations

With the empirical context established, I now present the theory's implications in the UK. This requires considering: 1) which respondents will be most likely to rely on experience (i.e., for whom we should expect a significant effect) and 2) the type of experience for different respondents (i.e., the direction of the effect). Both require grounding the theory in the UK party system since whether one's experience is positive or negative depends on the individual's party identification and those who do not receive strong party cues are expected to be most likely to rely on experience.

First, which respondents are most likely to rely on experience? Party cues are important determinants of support for electoral reform (Ahlquist et al. 2018; Clarke et al. 2013; Whiteley et al. 2012). Thus, the greatest effect of experience should be where these cues are weak. One of the most clearly observable ways parties give cues to their voters is through their election manifestos.[14] The Labour Party has been inconsistent and, at times, divided on the issue of national-level electoral reform. For example, in their 1997 manifesto, Labour committed to a referendum on electoral reform for the House of Commons and explicitly supported PR, but never followed through despite winning a majority (Kimber 2015). By 2005, their manifesto stated a commitment to review the electoral systems introduced for other bodies and maintained that a referendum was the appropriate method for changing

[13] I do not cluster standard errors by region due to a concern that clustering with so few clusters (two) would introduce bias (Angrist and Pischke 2009). Additionally, I do not use the survey weights because they are designed to ensure the sample is representative at the national rather than regional level.

[14] Appendix B presents systematic evidence of cues issued by Labour, the Conservatives, and the Liberal Democrats in their election manifestos.

the electoral system for the House of Commons but made no mention of PR (Kimber 2015). Thus, Labour has been inconsistent and not always explicit with their electoral system cues. Moreover, the party has sometimes been publicly divided on the issue. For example, for the 2011 alternative vote referendum, their leader, Ed Miliband, and others campaigned in favor of AV, while other prominent Labour MPs campaigned for SMDP, despite being the only party to advocate for an AV referendum in their 2010 party manifesto (BBC 2011; White 2011; Whiteley et al. 2012). This sort of division further muddies the cues they send to supporters. Since Labour supporters receive such weak and confusing cues from their party, Hypothesis 3.2 predicts these respondents will be the most likely to rely on experience.

In contrast, the Conservative Party has always been consistent (and unified) in their support for SMDP and against electoral reform for the House of Commons, but state that support in their party manifestos relatively rarely. Thus, the Conservatives issue consistent party cues (especially if we treat no mention in the manifesto as tacit support for the status quo), but historically they have been less explicit with their cues than other parties (Kimber 2015). Therefore, Conservative supporters receive medium strong cues from their party and are likely to be less reliant on experience than Labour supporters according to Hypothesis 3.2.

The small parties in the UK generally support some form of PR (often STV). For example, the Liberal Democrats (until 2015, the largest of the small parties), first contested national elections in 1992 and in every general election manifesto since, they have included a commitment to changing the electoral system for House of Commons elections to PR (Kimber 2015; Pack 2017). The Liberal Democrats have always given consistent and explicit cues about their electoral system preferences. Thus, Liberal Democrat and other small party supporters receive strong cues and should be relatively unlikely to rely on experience — Hypothesis 3.2 suggests experience with MMP may have little effect for these respondents.

Therefore, it is primarily Labour supporters (and possibly, but to a lesser extent, Conservative supporters) that lack clear partisan cues regarding electoral reform. Without clear, consistent cues from the party, it is these people for whom experience with alternative elec-

toral systems should be most important in determining their support for electoral reform — Hypothesis 3.2 suggests experience will have a significant effect for Labour supporters.

Second, what type of experience have different respondents had with MMP? I conceptualize a positive experience as one in which one's party obtains an increased proportion of seats in the current regional election relative to the most recent national election (these elections are non-concurrent). Conversely, a negative experience is when one's party obtains a lower proportion of seats in the regional election than in the last national election. This conceptualization is informed by evidence that comparisons between current and previous performance are important determinants of voter perceptions of "winning" an election (even if one's party does not form the largest or governing party in the legislature), and expectations condition the way voters interpret electoral results and institutional outcomes (Plescia 2019; Stiers, Daoust, and Blais 2018).[15]

With this conceptualization and election results from Audickas, Hawkins, and Cracknell (2016), we can establish expectations about the direction of the effect of experience for respondents who identify with each party. One further complication, however, is that national election results are reported in multiple ways. Theoretically, it is unclear if a positive experience is one in which one's party obtains an increased proportion of seats in the regional election compared to a) the proportion of seats the party obtained in the national election *within that region* or b) the proportion of seats the party obtained in the national election in the UK overall. In practice, whether one compares the regional results to the overall national results or national results only within the region, the predictions of Hypothesis 3.1 are identical for the Scottish National Party (SNP) and Plaid Cymru. For other parties however, the distinction is sometimes meaningful.

In Scotland, the Labour Party had a consistently negative experience with MMP across the full time period, while the SNP and Green parties had consistently positive experiences

[15] This conceptualization also allows for a comparison between electoral systems, which one based on only regional results (e.g., one's party obtaining more seats in the current regional election than in past regional elections) would not.

(regardless of the way national results are measured). Meanwhile, the Conservative Party had consistently positive experiences when comparing the proportion of seats in the regional election to the proportion of seats in the last national election *within Scotland*, but a negative experience if one considers national election results *in the UK as a whole*. The Liberal Democrats have a positive experience (with the exception of 2011) when comparing the proportion of seats in the regional election to the proportion of seats in the national election *in the UK as a whole*. In summary, MMP in Scotland has been a negative experience for Labour Party supporters, the prediction with respect to the Conservatives is unclear, and overall, the other parties have had a generally positive experience (with some exceptions for the Liberal Democrats depending on which national results one uses for comparison).

In Wales, the Labour Party had a consistently negative experience with MMP across the full time period — they obtain a lower proportion of seats in the National Assembly for Wales elections than they do in the previous national election (the only exception is in 2011, if one compares the regional results to the national results in the UK as a whole). In contrast, Plaid Cymru had a consistently positive experience with MMP. The Liberal Democrats generally had a positive experience with MMP in Wales. The only exceptions are if one compares the regional results to national results only within Wales in 2007 (in which case the experience was equivalent between the two systems), or if one uses national results in the UK overall as the comparison in 2011 (in which case the party performed better in the national than in the regional election by half a percentage point). Once again, the expectation for supporters of the Conservative Party is unclear, as whether the experience is positive or negative depends on whether one compares the regional results to the national election results only within Wales (positive experience) or across the UK as a whole (negative experience).

Thus, if Hypothesis 3.2 is correct, we should expect Labour supporters to be most likely to rely on experience and thus experience should have a significant effect. Moreover, Labour supporters in Scotland and Wales (who have had a negative experience with MMP) should be *less* likely to support reform compared to Labour supporters in Northern England and

Table 3.2: Empirical Expectations

Party ID	Party Cues	Experience	Expected Effect
Labour	Weak (inconsistent)	Negative	Significant, negative
Conservative	Medium (anti-reform)	Unclear	Unclear
Other	Strong (pro-reform)	Positive	Insignificant (positive)

Note: Treatment effect expectations based on extent to which respondents rely on experience and the type of experience with MMP.

the Midlands according to Hypothesis 3.1. The prediction with regard to Conservative supporters is unclear — they receive party cues that are consistent, but not always explicit so are less likely to rely on experience than Labour supporters, but more likely than other party supporters. Additionally, if there is a significant effect of experience for Conservative supporters, the expected direction is unclear. In the case of other party supporters, they should be least likely to rely on experience and thus Hypothesis 3.2 suggests experience with MMP may have no significant effect for this group. That said, we should expect supporters of other parties in Scotland and Wales (who have had a relatively positive experience with MMP) to be, if anything, more supportive of electoral reform than those in Northern England and the Midlands according to Hypothesis 3.1. Table 3.2 summarizes these expectations.

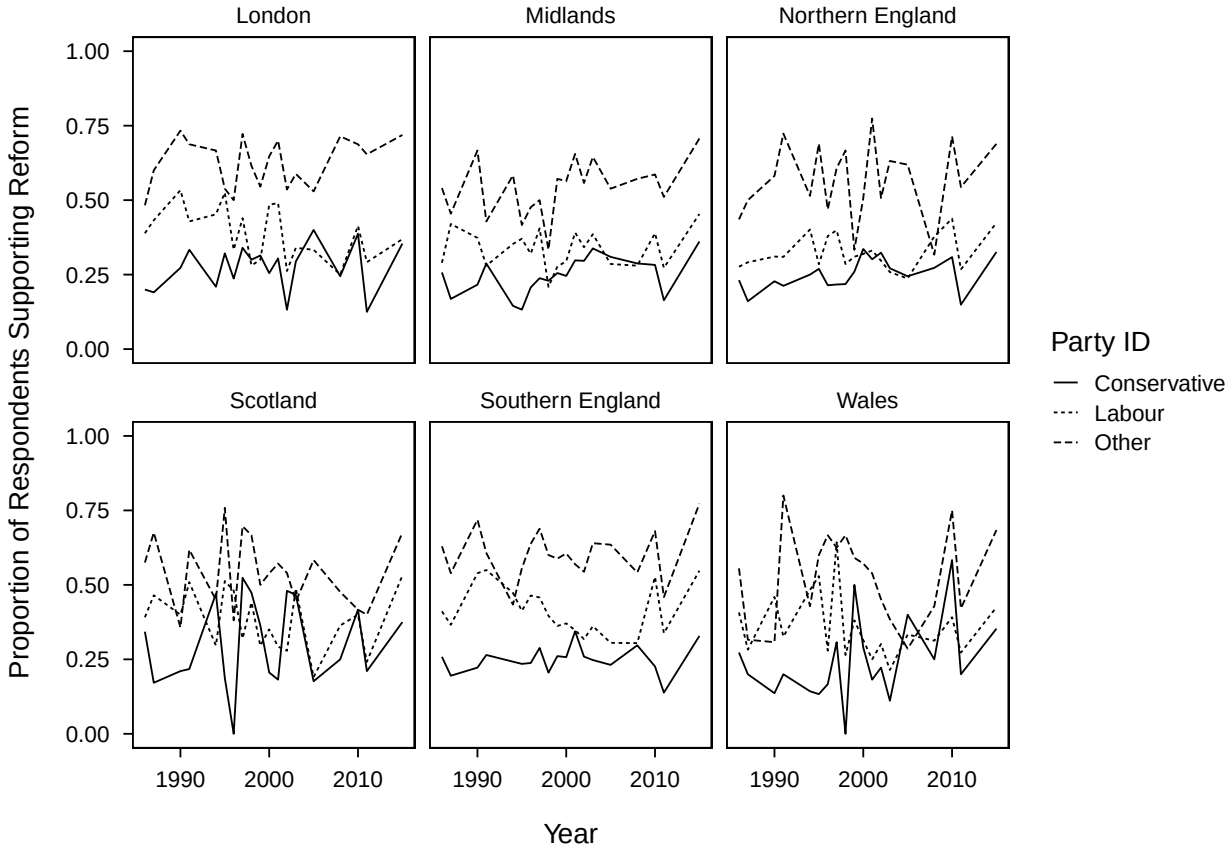
3.3 Analysis

Figure 3.1 shows the mean support for reform across party by region. The basic intuition behind the theory is that there will be differences in the average level of support for reform across parties and this is indeed what we see in the figure. As expected, supporters of small parties are, on average, more supportive of electoral reform than supporters of Labour and the Conservatives.[16]

Figure 3.2 shows the effect of experience with MMP on support for electoral reform — the treatment effect from DID models in which the dependent variable is *Reform Support*.

[16] See Appendix B for mean support for reform by party, disaggregating other party supporters.

Figure 3.1: Support for Reform by Party and Region

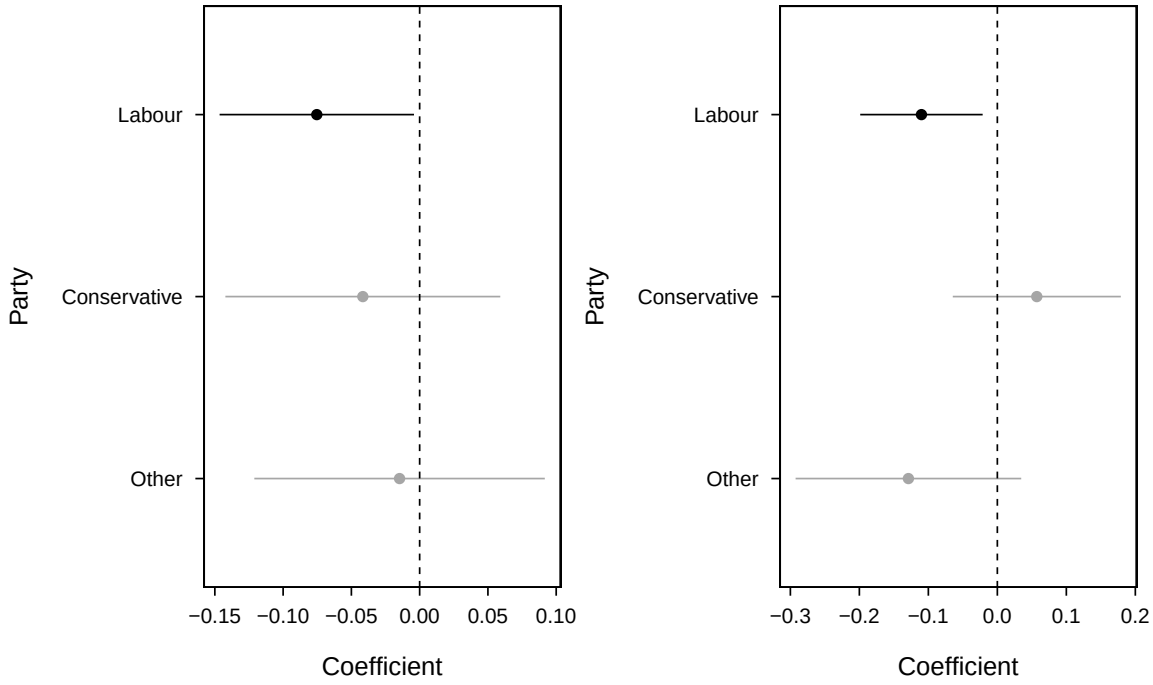


The left panel shows the results of DID analyses comparing the difference in the probability respondents will support reform before and after the 1999 introduction of MMP in Scotland within the region to the difference in the probability of support before and after 1999 in Northern England. The right panel shows the results of DID analyses comparing the difference in the probability respondents will support reform before and after the introduction of MMP in 1999 within Wales to the difference in the probability of support before and after 1999 in the Midlands. Since support for reform should be conditional on whether one's experience with a similar electoral system benefited one's preferred political party or not, Figure 3.2 shows the treatment effects obtained by running the DID analyses on subsets of respondents based on their party identification.[17]

The results in the left panel of Figure 3.2 show that experience with MMP has a significant

[17] Results tables corresponding to all figures may be found in Appendix B.

Figure 3.2: Effect of Experience with MMP on Support for Electoral Reform



Note: Left panel uses the Scotland/Northern England DID while right panel uses the Wales/Midlands DID. Results of linear probability models including year fixed effects (excluding one year due to collinearity) and control for interest in politics. 95% confidence intervals shown (significant estimates in black).

effect on support for electoral reform for Labour supporters as predicted by Hypothesis 3.2. Experience with MMP makes Labour supporters in Scotland *less* likely to support a change to PR relative to Labour supporters in Northern England as predicted by Hypothesis 3.1. As expected, Labour supporters are more likely than others to rely on experience given the weak party cues they receive and in Scotland they had a negative experience with a system similar to that which is being considered. The treatment effects are insignificant for the Conservative and other party supporters. While the expectation regarding Hypothesis 3.1 for the Conservative supporters was unclear, they do receive stronger cues than Labour supporters, and these results are consistent with the expectation from Hypothesis 3.2 that they would therefore be less reliant on experience. Similarly, other party supporters receive very strong cues and the treatment effect is insignificant, as expected by Hypothesis 3.2.

The right panel in Figure 3.2 shows that experience with MMP in Wales has a significant and negative effect on support for electoral reform among Labour supporters. Again,

this is in accordance with Hypotheses 3.1 and 3.2, since the experience with MMP in the National Assembly for Wales is negative for Labour supporters (who also receive weak party cues). The treatment effects for Conservative and other party supporters in Wales are not statistically significant, which is again consistent with the expectation from Hypothesis 3.2 that experience is most important for Labour supporters since they receive the weakest cues. Overall, the results from both regions support the theory that experience with a similar electoral system to that which is proposed influences one's support for electoral reform and that this effect is largest for those who do not receive clear party cues.

It is difficult to visually inspect the plausibility of the parallel trends assumption with individual-level data, but I probe this assumption in several ways. First, while I include the full sample results here, in Appendix B, I include results from smaller subsets based on year of the survey (i.e., one year on either side of the reform, two years on either side of the reform, etc.).[18] The surveys are repeated cross-sections rather than a panel so the parallel trends assumption critical for DID analyses may be violated due to changes in the populations surveyed (for example, caused by movement across regions). The samples in shorter time periods are less likely to be affected by such population change and it is therefore more likely the parallel trends assumption will hold (however, the sample size is also much smaller).

While the significance of the treatment effect varies with the smaller subsets, in part due to the sample size reduction, the sign of the estimates is generally consistent with the main results presented here. In the Scotland/Northern England samples, the treatment effect is significant and negative (consistent with Hypothesis 3.1) in some models for Conservative supporters, but it is never significant in the Wales/Midlands samples. Given that Conservatives receive consistent, but not always explicit party cues, it is not entirely surprising that some models would show significant treatment effects. On the other hand, across all subsets, supporters of other parties are unaffected by experience with MMP, exactly as predicted by Hypothesis 3.2 given the strong cues they receive.

[18] The survey occurred after the reforms in 1999. Additionally, the electoral reform question was asked every year between 1994 and 2003, allowing for complete coverage up to five years on either side of the reforms.

Another way the parallel trends assumption might be violated is if respondents are switching parties — if for reasons other than experience with MMP, people were sorting into parties differentially on either side of the boarder before and after 1999. Unfortunately, given the repeated cross-section nature of the data, this cannot be entirely ruled out. However, the consistent direction of the treatment effect in smaller time samples is reassuring since these samples are less likely to be affected by this type of sorting. Moreover, in Scotland, one might argue that party switching was most prevalent around the 2015 general election, given the meteoric rise of the SNP in that election. Importantly, rerunning the full sample analyses using the Scotland/Northern England samples and excluding observations from 2015 (resulting in 2011 being the last year of the survey included in the analysis) does not affect the results. This increases confidence that partisan sorting isn't driving the results.

In addition to exploring the parallel trends assumption with sample subsets, below I describe the results of several placebo tests, which further allay concerns that these effects are the result of population changes or other factors. Finally, I run diagnostics including treatment leads. Ideally, the leads should be individually and jointly insignificant since the treatment cannot have an effect prior to its introduction. In the case of the Scotland/Northern England results, most treatment leads are insignificant, but for the Labour and Conservative subsets, F-tests lead us to reject the null hypothesis that the coefficients on the treatment leads are jointly equal to zero. However, the shorter time samples presented in Appendix B are less likely to show violations of the parallel trend assumption, as expected. Additionally, for the other parties sample using the Scotland/Northern England comparison, we cannot reject the null hypothesis that the treatment leads are jointly equal to zero. For the models comparing Wales and the Midlands, all treatment leads are individually insignificant and F-tests indicate we cannot reject the null hypothesis that the leading treatment coefficients are jointly equal to zero at any conventional confidence level. Thus, while these results are not ideal for the Scotland/Northern England samples, they provide confidence that the parallel

trends assumption holds for the Wales/Midlands samples.[19]

3.4 Robustness Tests

Although the DID analyses allow me to be relatively confident that experience is driving support for electoral reform, there are two remaining issues: 1) constituency-level preferences might outweigh regional-level preferences about party performance and 2) placebo tests are necessary to increase confidence that the DID analyses are valid.

3.4.1 Constituency-level Preferences

First, while I have been considering a positive experience in terms of the fate of the individual's preferred party at the regional level, perhaps what really matters to individuals is their party's fate in their local constituency. It is possible people are willing to accept regional (or national) results that do not benefit their party if their constituency is represented by a co-partisan (e.g., Stiers, Daoust, and Blais 2018). For a subset of BSA survey years, I match respondents to their parliamentary (House of Commons) constituency.[20] I then use the Constituency-Level Election Archive (CLEA; Kollman et al. 2019) to match this subset of respondents to the constituency-level results in the most recent previous election. Using CLEA election results and respondent party identification, I code *Co-partisan MP* which takes a value of one if the respondent and the MP elected in the respondent's constituency in the most recent election are from the same party and zero otherwise.

As shown in Appendix B, the main results from the full time sample DID are unchanged when *Co-partisan MP* is included as a control. Unfortunately, it is only possible to match respondents to parliamentary constituencies and not to Scottish Parliament or National Assembly for Wales constituencies. However, due to the need to compare people outside

[19] See Appendix B for additional details.
[20] Due to data limitations, I am only able to reliably match respondents to their constituency in 1986–87, 1990–1991, 1998, and 2000–03. All other observations are dropped from analyses including *Co-partisan MP*.

these regions as well, it is necessary to use a unit that is common across regions. Moreover, I cannot compare the Scottish Parliament/National Assembly for Wales constituencies before and after MMP was introduced because the bodies were introduced concurrently with MMP. Nevertheless, the proposed change to PR that respondents are being asked to consider would occur at the parliamentary level and it is possible that they would be willing to sacrifice national partisan advantage for a co-partisan MP. That the results hold with the inclusion of the control is suggestive evidence that this local concern does not overpower the effect of experience combined with outcomes of that experience at the regional level.

Moreover, one might argue that if what really matters is the type of experience at the constituency level as opposed to the type of experience at the regional level, the DID analyses should be run on subsets, not according to party identification, but according to whether the individual has a co-partisan MP. As Appendix B demonstrates, the treatment effects are not significant when the analyses are run in this way (except the treatment effect using the Wales/Midlands sample and co-partisan MP subset, which is significant at the 90% level). While imperfect, this is further suggestive evidence that what really matters is experience with a similar electoral system combined with the outcome of that experience at the regional level, as opposed to experience and constituency-level outcomes.

3.4.2 Placebo Tests

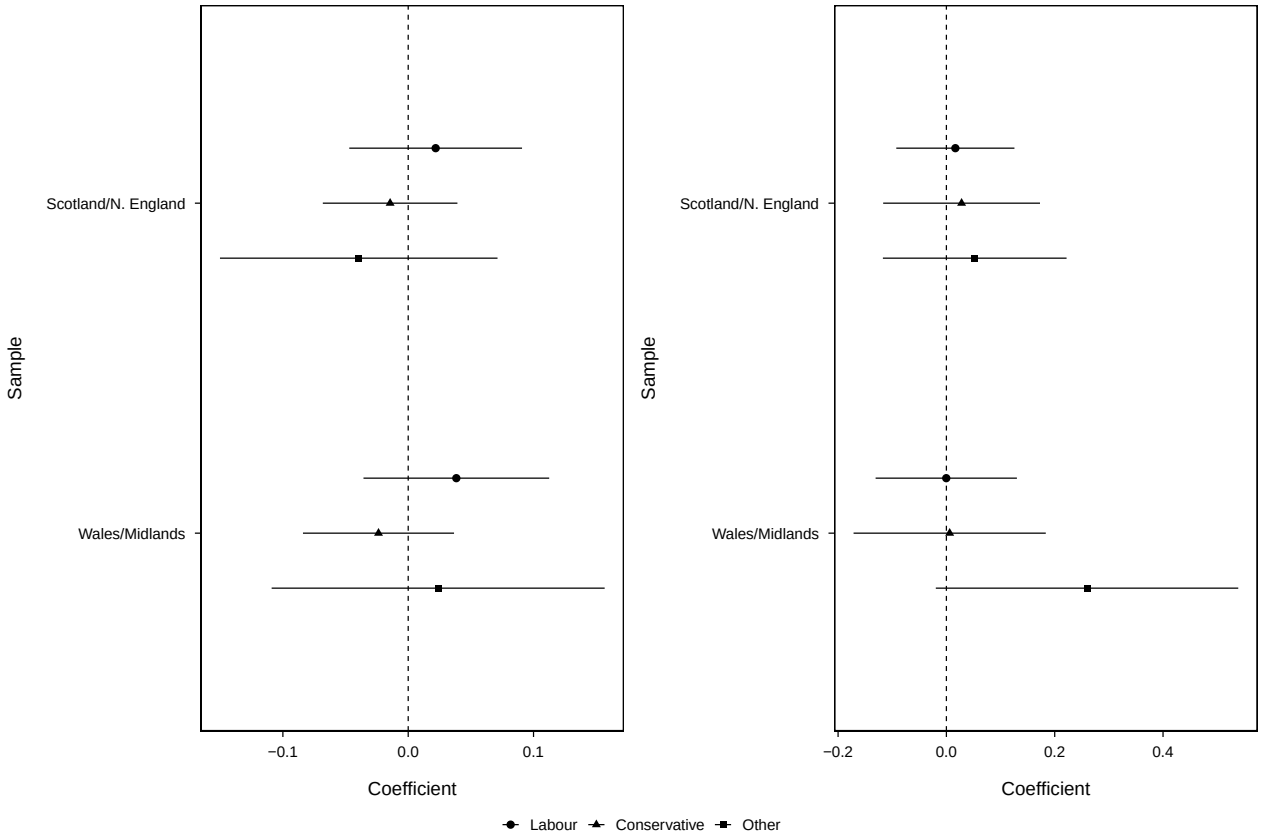
Second, I conduct placebo tests to increase confidence the DID is capturing the effect of electoral system experience as opposed to some other factor correlated with region and time (or population changes over time across regions that would violate the parallel trends assumption).[21] For the first placebo test, I use the same DID design as above (including year fixed effects and controls for interest in politics), but the dependent variable should theoretically be unaffected by the treatment — support for abolishing the monarchy. Thus, I compare the difference in the probability an individual supports abolishing the monarchy

[21] Appendix B provides additional details about these tests.

among respondents living in Scotland/Wales before and after the 1999 introduction of MMP to the difference among respondents living in Northern England/the Midlands before and after 1999. Experience with MMP should have no effect on the probability a respondent will support abolishing the monarchy. As expected, the left panel of Figure 3.3 shows the "treatment effects" are insignificant, increasing confidence that the treatment effects in the earlier DID analyses are capturing the effect of electoral system experience rather than another factor correlated with region or time.

Finally, I conduct an additional placebo test to further alleviate concerns that the main DID analyses violate the parallel trends assumption. The DID design is again identical to the main analyses, comparing support for electoral reform in Scotland/Wales to that in Northern England/the Midlands, subsetting by party identification, and including year fixed effects and controls for interest in politics. However, for this analysis, I drop all observations from the real post-treatment period (i.e., all observations after 1998). Instead, I assume that the introduction of MMP occurred between the 1994 and 1995 surveys in Scotland and Wales and thus treat all observations in Scotland and Wales from 1995 through 1998 as treated. Ideally, we should see that there is no effect of this placebo "treatment." The right panel of Figure 3.3 shows all the "treatment effects" are insignificant, as expected. Overall, this increases confidence that despite the concerns regarding the parallel trends assumption from the analysis of the treatment leads, the main DID results are valid.

Figure 3.3: Placebo Tests: Support for Abolishing the Monarchy and Support for Electoral Reform with Placebo "Treatment Period"



Note: Left panel shows the results of placebo tests of the effect of experience with MMP on support for abolishing the monarchy. Right panel shows the results of placebo tests of the effect of experience with MMP on support for electoral reform in which the real treatment periods have been dropped and a placebo "treatment" occurring prior to the 1995 survey wave was assigned in Scotland and Wales. Results of linear probability models. 95% confidence intervals shown.

3.5 Conclusion

Electoral systems are increasingly being changed, either by citizens in initiatives and referenda or through the legislature. However, less is known about citizen preferences regarding electoral rules and the conditions under which citizens will support changes to those rules (although this is changing; see Banducci and Karp 1999; Norris 2011; Plescia, Blais, and Högström 2020; Renwick 2010, 2011). While I argue citizens have preferences over electoral rules that are (at least partially) based on their partisan preferences, it is not always clear how citizens determine which rules will favor their preferred political party. I explore how

experience with multiple electoral systems can provide this information (especially important in the absence of clear party cues) and thus affects support for electoral reform.

Understanding the role of experience with multiple electoral systems is important because such experience is relatively common. For example, Australia, Canada, Chile, France, Nepal, Russia, the UK, and the U.S. all use multiple electoral systems for elections at different levels of government or for different bodies. Moreover, in places such as Italy, Japan, and New Zealand, people have been exposed to multiple systems over time because of changes to the electoral system used for elections to a single body. My results suggest that experience with a system similar to that which is under consideration does influence support for reform, even if the proposed reform would occur at a different level of government than the experience. Specifically, a positive experience — one in which an individual's party performs well — may increase support for reform, while a negative experience may decrease support for reform.

Would-be reformers may be encouraged by these results because they indicate that bottom-up reform processes — in which reforms are enacted at lower levels in part as an effort to garner support for the reform at a higher level of government — have the potential to succeed. While this analysis does suggest that is a possibility, the type of experience matters greatly — both in terms of which electoral systems voters experience as well as whether that experience was positive or negative. Moreover, experience is only one source of information for citizens about electoral systems. Party cues are another. In future research, a survey experiment would allow us to better understand how the magnitude of the effect of electoral system experience compares to that of party cues.

However, these findings also have a darker implication. These results indicate that people prefer rules that benefit their preferred political party (Lamare and Vowles 1996; Plescia, Blais, and Högström 2020). This suggests that incumbents who are popular with the public may find it relatively easy to maintain or institute electoral rules that keep them in power, even if those rules are unfair or undemocratic (Ahlquist et al. 2018; Graham and Svolik 2020). In such cases, it becomes difficult to determine the extent to which incumbents are

able to remain in power only through rule manipulation or pure popularity.

More generally, this work also has implications for perceptions of the legitimacy of electoral rules among the public. Electoral rules have distributive effects in terms of which parties win and which policies are implemented. This, combined with evidence that support for different electoral systems depends on one's partisan preferences, implies that electoral reform risks delegitimating the electoral system in the eyes of the reform's "losers" and could undermine support for democracy more broadly.

While the present paper focuses on partisan motivations, citizens may have other preferences as well, for example, for fair elections or simple, easy to understand electoral rules. Additional research, likely including dedicated surveys and survey experiments focusing on electoral reform (with a wider range of questions about electoral system preferences), is needed to fully understand how citizens balance these distinct preferences as well as multiple sources of information (e.g., experience, party cues, and/or messaging from electoral commissions). Given the increasing use of referenda as a mechanism for electoral reform, exploring what shapes public opinion is critical to understanding when reform will occur and what form it will take.

Chapter 4

Legislating Themselves Out of Office: Electoral Reform and Parties as Non-Unitary Actors

Much of the electoral reform literature suggests that parties — generally conceptualized as unitary actors — will change the electoral system when they believe they can increase their legislative seat share by doing so (Benoit 2004, 2007; Benoit and Hayden 2004; Benoit and Schiemann 2001; Birch et al. 2002; Boix 1999; Colomer 2004, 2005; Kaminski 2002).[1] However, others have recognized that *intra-party* politics also affect electoral reform. For example, McElwain (2008) illustrates how the leadership of the Liberal Democratic Party (LDP) in Japan was prevented from adopting single-member district plurality (SMDP) despite evidence that doing so would increase the LDP's seat share. They were thwarted by party backbenchers (rank-and-file legislators) who feared SMDP would damage their chances of reelection. Similarly, Remington and Smith (1996) describe the adoption of the 1995 Russian electoral law as a case in which the preferences of individuals for retaining their parliamentary seats outweighed concerns about maximizing their policy influence, with the

[1] Recent work has also considered other motivations for reform besides seat maximization, including coalition management and control (Cox, Fiva, and Smith 2019; Gandhi, Heller, and Reuter 2020).

result that the 1995 system was very similar to that imposed by decree in 1993.

These cases in which major electoral reforms failed in the legislature raise a novel puzzle obscured by the traditional focus on unitary parties. If it is possible for rank-and-file legislators to stop reforms that the party leadership wishes to pass, why do reforms that disadvantage a significant portion of the party ever pass? Electoral reforms may have differing consequences across members of a single party — even if a reform is expected to benefit the party overall, it may disadvantage certain party members (McElwain 2008). Moreover, there are cases (often overlooked in the existing literature) in which parties pass reforms that are expected to *decrease* their overall seat share. For example, in 1998, the Labour Party in Britain passed a bill that reduced the number of seats in Scotland despite the fact that Scotland was a party stronghold at the time (Gay 2010). While these types of reforms may be sensible if parties are treated as unitary actors, they are harder to understand from the perspective of party backbenchers. Why would an individual politician vote in favor of an electoral reform that is likely to cost her the seat? In other words, why would a politician legislate herself out of office? In general, what affects party backbenchers' — rank-and-file legislators who do not have a party leadership position and thus might be more likely to rebel against a reform that is not uniformly beneficial — support for electoral reform?

Making these questions even more vexing, at first glance it might seem as though the lessons from the literature on party cohesion do not apply in the case of electoral reforms. This type of situation is not particularly conducive to voluntary party cohesion (legislators voting as a block voluntarily). Voluntary party cohesion that relies on shared preferences among like-minded individuals or the fact that party affiliation is beneficial because it provides voters with informational shortcuts that help legislators develop a "brand name" and helps legislators coordinate amongst themselves to achieve their goals (e.g., Aldrich 1995; Bowler, Farrell, and Katz 1999; Duverger 1963; Hicken 2009; Laver and Shepsle 1999) is unlikely to work in this situation. After all, there is little incentive to voluntarily vote with one's party on a bill that one believes, if passed, will most likely cause one to lose the seat.

Moreover, party discipline may be hard to enforce in such a situation because party leaders are hampered in their ability to use some of the "carrots and sticks" on which they can usually rely. Control over patronage, election support (including nomination/selection and funding), career advancement within the legislature, information, legislative rules and agenda, and the timing of dissolutions and elections have all been cited as ways the party leadership can ensure discipline (e.g., Benedetto and Hix 2007; Bowler, Farrell, and Katz 1999; Cheibub 2007; Cox 1987; Cox and McCubbins 1993; Duverger 1963; Eggers and Spirling 2014b; Kam 2009). In the context of a proposal for reform that is likely to harm the election prospects of individual legislators, some of these methods are either inapplicable or lose their effectiveness. After all, threats by the party leadership of withdrawing election support or future deselection from a preferred committee lose much of their force if the legislator expects to lose the election anyway.[2]

However, the party discipline literature tends to consider carrots and sticks that party leaders can wield within the legislature such as committee assignments or around elections like control over nomination (depending on the electoral system and the level of party centralization). For example, the literature that explores party discipline in the United Kingdom often focuses on the relationship between loyalty to the party (in terms of votes over one's career) and cabinet positions or other parliamentary offices (e.g., Eggers and Spirling 2014b; Kam 2009). Nevertheless, it has been noted that some classes of members of Parliament (MPs) are less likely to be controlled using ministerial appointments, either because they have previously served as ministers or because they feel they have no chance of ever doing so (Benedetto and Hix 2007). Looking beyond cabinet appointments, I argue there are also extra-parliamentary carrots that leaders can use to induce support for reform. Party leaders can offer incentives for supporting electoral reform that *do not require recipients to hold elected office*.

I argue that backbenchers will be more likely to support electoral reform, even if it

[2] Note this is true even if we assume politicians are not purely office seeking since it is generally through participation in parties and government that one can have policy influence (Cheibub 2007).

damages their individual reelection prospects, when they expect compensation from the party leadership in case of an electoral loss (and possibly a reward using the usual parliamentary channels even if they do manage to retain their seat). In other words, when MPs expect the party leadership to "take care of them" by providing patronage in the form of appointments, they will be more likely to vote in favor of a reform that they expect is likely to damage their future reelection chances. This is one way that party leaders can repurpose some of the tools they might use to discipline members to instead compensate those who expect to lose their seats and thus gain their legislative support. These rewards for those that manage to retain their seats or compensation for those that lose as a result of the reform can take a variety of forms depending on the context. I test this theory using extensive information on the careers of MPs in the United Kingdom (UK) who voted on the 1885 Redistribution of Seats Act, which drastically altered the electoral landscape in Britain. Specifically, I explore compensation by the party in the form of honors (peerages and baronetcies) and decorations (knighthoods) and rewards of offices within Parliament, including cabinet positions.

Despite a large literature on party cohesion and discipline, the electoral reform literature generally sidesteps this issue by assuming parties are unitary actors.[3] Thus, there is little scholarly work on why individual legislators would vote in favor of reforms that may damage their future prospects. In part, this is because of the focus on unitary parties, but it is also because the focus on seat maximization often leads to an implicit assumption that everyone in the party would stand to benefit equally from a reform. Or at the very least, that all members of the party will be equally motivated by the opportunity to maximize the seat share of the party overall, regardless of what effect that would have on their own electoral prospects. By highlighting issues of intra-party politics frequently overlooked in the discussion of electoral reform, this paper contributes to the electoral reform literature. Moreover, by highlighting the inducements that party leaders can offer that are outside the

[3] In a rare exception to the observation that the electoral reform and party discipline literatures are usually kept separate, Cox and Ingram III (1992) consider the effect of electoral reform (suffrage expansion) in the UK on party cohesion. In contrast, I study how party discipline tactics may enable electoral reform.

legislature, I also speak to the literature on party discipline.

Additionally, the historical case that I use to test the theory — the passage of the 1885 Redistribution of Seats Act — is important and worthy of study in and of itself. While the UK is often thought of as the prototypical case of single-member district plurality electoral rules, it was only with this Act that single-member districts became the norm (Cox 1987; Hart 1992). Prior to the reform, the majority of MPs were elected from multimember districts. By drastically reducing the number of dual-member constituencies and therefore eliminating the practice of the two main parties — the Liberals and Conservatives — each running one candidate in a district, electoral competition was significantly increased (Hawkins 1998; Mason 2015). As important as the Redistribution of Seats Act was to the creation of the electoral system with which we are now familiar in the UK, it is often overshadowed by the Representation of the People Act 1884, or the Third Reform Act, which extended the franchise. Moreover, the passage of these two bills was linked (as I describe below). Thus, by studying the choices of legislators, this paper also contributes to better understanding how the Redistribution of Seats Act came to be, which is, in itself, important.

In the next section, I begin by providing a theory of why legislators may vote themselves out of office. Next, I describe the empirical context in greater detail. While the theory I present is general, I reserve discussion of the specific hypotheses I test until after the empirical context has been presented because the specific inducements that parties can use to elicit support for a reform depend on the context. This leads to a discussion of the research design. I then present the results of the analyses and robustness tests before concluding.

4.1 Why Legislate Oneself Out of Office?

Much of the electoral reform literature treats parties as office-seeking unitary actors (e.g., Benoit 2007; Boix 1999; Colomer 2004). However, we know that parties are collections of individuals and may behave in a more or less unified way (e.g., Bowler, Farrell, and

Katz 1999; Cox 1987). Therefore, intra-party dynamics must be taken seriously to truly understand electoral reform. In addition to the motivating examples of Japan and Russia in which the reelection prospects of backbenchers prevented electoral reforms that may have increased the party's seat share or policy influence (McElwain 2008; Remington and Smith 1996), there have been cases in which parties have passed reforms despite expectations that their seat share will decrease. The UK Labour Party passed the Scotland Act 1998 which reduced the number of parliamentary seats in Scotland — a Labour stronghold at the time (Gay 2010). Moreover, even if a party's overall legislative seat share may increase with a particular reform, it does not immediately follow that all current office holders will benefit. Thus, the question of the conditions under which individual legislators will support electoral reform is critically important.

In many ways, the problem for the party leadership of inducing members to support electoral reform is the same as the problem of inducing them to support any other type of legislation, with two important caveats. First, in the case of electoral reform, some of the carrots and sticks parties have at their disposal to enforce discipline lose their effectiveness. For example, control over things like ministerial appointments, committee assignments, the legislative agenda, and the timing of dissolution (which have all be cited as ways for the leadership to maintain discipline; e.g., Benedetto and Hix 2007; Bowler, Farrell, and Katz 1999; Cheibub 2007; Cox 1987; Cox and McCubbins 1993; Duverger 1963; Eggers and Spirling 2014b; Kam 2009) is unlikely to benefit the party leadership in this context. If a legislator expects to lose her seat because of the reform, the offer of (or threat of removal from) a plum committee assignment (one of the most common inducements in parliamentary systems; Bowler, Farrell, and Katz 1999) or the threat of a snap election are unlikely to persuade her to support the reform. Second, in some cases, electoral reform can expose a much broader section of the party to lower chances of reelection than other legislation. Rather than the need to "buy off" a handful of legislators or leaders being willing and able to sacrifice some votes, electoral reform can complicate the reelection prospects of a large

minority (or even majority) of a party. This is even more problematic if reform requires a supermajority to pass (which may be more likely for institutional rules such as electoral reforms than other types of legislation). Nevertheless, I argue that the party leadership can still wield certain carrots to induce support.

A member of parliament should be more likely to vote in favor of an electoral reform that harms her reelection prospects if she expects to be compensated or otherwise "taken care of" by the party in exchange for her support. Implicit in this argument is the assumption that the party leadership prefers that the reform passes. Although this may at first appear to be a strong assumption, it is plausible for several reasons. First, in the parliamentary systems that are the focus of this paper, government-sponsored bills are much more common than private member's bills (those that are not proposed by the government).[4] This means that electoral reform proposals may well come from the government itself as opposed to the issue being forced by the opposition. Indeed, a large segment of the electoral reform literature was initially driven by the puzzle of why a party in government would change the system that brought them to power. Second, there are a number of reasons the party leadership may support an electoral reform proposal even if it damages the reelection chances of some of their members. They may believe that the party as a whole will be better off (McElwain 2008), wish to disadvantage opposition parties (Higashijima and Chang 2017; Lust-Okar and Jamal 2002), use electoral reform as a way to increase control over members (Cox, Fiva, and Smith 2019; Gandhi, Heller, and Reuter 2020), or trade the passage of electoral reform in exchange for support on some other priority. Thus, the question under investigation is why would a member of the governing party's backbench support a reform that reduces her personal chances of reelection?

The legislator may vote in favor of reform because she believes that her party will "take care of her" in the event that the reform damages her prospects or costs her the seat. In other words, if she fails to win reelection, the legislator expects the party to compensate

[4] See, for example, Rush (2001) for a discussion of how government business came to dominate the British parliamentary calendar over the course of the nineteenth century.

her for her sacrifice. This does not preclude the possibility of rewarding those who maintain their seat despite the risks posed by the reform. This may also occur, especially given that there may be uncertainty regarding who the reform will hurt and the need to induce support occurs prior to the realization of the effect of the reform on the election fate of members. Therefore, in addition to compensation for those who do lose their seats, the party leadership may use promises of rewards to induce support even for those who "survive" the effects of the reform. If these inducements are used, they can take the form of more traditional carrots considered in existing literature such as committee assignments or parliamentary office (e.g., Benedetto and Hix 2007; Eggers and Spirling 2014b; Kam 2009).

Ideally, it would be possible to track legislator expectations or promises of rewards and compensation made by the party prior to a reform. However, any such promises are likely to be hidden (or, if not actively hidden, difficult to track in a systematic way). Nevertheless, one observable implication of the argument is that those legislators who support the reform (as the party leadership prefers), run in the next election, and lose their seats are more likely than those who abstained or voted against the reform to receive compensation and patronage from the party. This compensation could take the form of government pensions, diplomatic appointments, appointments to positions within the party that do not require election, or even decorations such as knighthoods. If the party also rewards those who supported the reform and managed to keep their seat, those rewards are likely to take a different form than the compensation for those who lost. Decorations such as knighthoods may also apply for this latter group, but other types of rewards might include prime committee assignments or other positions within parliament.

It is worth noting that this compensation or reward from the party leadership need not be given out of a sense of fairness. Rather, the party leadership depends on the support of their backbenchers in order to pass preferred legislation, in this case, electoral reform. It may be that after realizing this legislation has disadvantaged members of their own party, the party leadership seeks to compensate them, motivated by fairness. However, that need

not be the case. While there is an obvious commitment problem, it is overcome because the party leadership have a relatively long time horizon and reputation concerns — they know they will need to be able to induce support for legislation in the future. Therefore, breaking promises of compensation or reward sets a dangerous precedent that makes it more difficult to control legislators in the future. In addition, losing one election does not preclude the possibility of running and winning at a later date. The party leadership may want to recruit these people again in the future. Therefore, it is in the interests of the party leadership to provide this compensation even if they are not motivated by concerns of fairness.

4.2 Empirical Context

In order to test this theory, I exploit the 1885 Redistribution of Seats Act in the United Kingdom.[5] This provides an ideal test case for the theory for several reasons. First, as I describe in the subsection below, the bill was passed by the Liberal government. Liberal MPs supported the bill despite (correctly) expecting that it would harm their reelection prospects. While the party leadership wished to pass the bill as part of a compromise that would allow them to achieve other, more important (in their view), goals, the puzzle of why backbenchers supported it remains. Second, a division (roll-call vote) was held on this bill, allowing me to determine how individual MPs voted, which is critical for the analysis. Since individual votes are not always recorded, there are other reforms that fit the theoretical story, but for which it is impossible to test the empirical implications due to an inability to determine how individual MPs voted.[6] Third, Rush (2001) and Eggers and Spirling (2014a) have collected information on the careers of MPs from this time period, allowing me to empirically track what happened to those who voted on the bill. Specifically, I am able to use this data (described in more detail below) to construct measures of compensation and rewards in the form of appointments. Before turning to the research design, it is helpful to consider the

[5] Note that at this time, the United Kingdom included all of Ireland.
[6] E.g., I am unable to leverage the passage of the Scotland Act 1998 (referenced above) because I cannot identify how individual MPs voted.

context of the Redistribution of Seats Act as well as the types of compensation the party leadership had at their disposal at the time. This will allow me to ground the hypotheses and design in the specific context.

4.2.1 The 1885 Redistribution of Seats Act

The 1885 Redistribution of Seats Act was passed with support from both the Liberal and Conservative parties during the Liberal government of William Gladstone (1880–1885). Although the Act actually increased the overall number of seats in Parliament from 652 to 670 (Uberoi and White 2015), the distribution of these seats across the country was drastically altered. Notably, many constituencies were changed from multimember districts (most frequently, dual-member) to single-member districts. Prior to the reform, more than two-thirds of the 652 members were elected from multimember districts, whereas after the reform, only 27 such constituencies remained (Hart 1992; see also, Cox 1987).[7] Moreover, although the cities saw an increase in representation, in other areas, representation was significantly reduced and around the country, electoral competition was increased (Hawkins 1998; Mason 2015). In part, competition was increased by the reform because the introduction of predominately single-member districts ended the practice of both Liberals and Conservatives each nominating a single candidate in dual-member constituencies (Mason 2015).

The Redistribution of Seats Act was the result of a compromise between Liberals and Conservatives. The Liberals wished to pass an extension of the franchise in the form of a Representation of the People Bill,[8] but it was halted by Conservatives in the House of Lords until the Liberals promised to pass a bill to redistribute seats (Butler 1963; Evans 2000; Hawkins 1998; Mason 2015). Both parties expected franchise extension to benefit the Liberals, while the redistribution of seats (particularly the change to predominately single-member districts) was expected to benefit the Conservatives (Bogdanor 1981; Butler 1963;

[7] The last of the dual-member constituencies were eliminated in 1948 (with the change taking effect at the 1950 election; Parry 2012).
[8] This became the Representation of the People Act 1884, also known as the Third Reform Act.

Carstairs 1980; Dunbabin 1988; Evans 2000; Mason 2015).[9]

Although it is clear why the Liberal leadership wished to pass the Redistribution of Seats Bill — it was necessary in order to pass the franchise extension, which they valued even more — the question remains: why would Liberal backbenchers go along with the leadership and pass the bill? They (rightly) expected to be disadvantaged by the redistribution and resulting increased competition. Additionally, the extension of the franchise had been passed prior to the redistribution of seats, so it is conceivable that Liberal backbenchers could have rebelled in the vote on redistribution in an attempt to protect their seats. Moreover, although party cohesion was higher in the 1880s than it had been in several earlier decades, it was below its 1870s level (Rush 2001), and party discipline was still not as well developed as it has been since the turn of the twentieth century — only the previous year a cabinet member defied the whip (in other words, failed to follow voting instructions; Mason 2015). Therefore, it is reasonable to ask why MPs would legislate themselves out of office, given that existing historical analyses suggest they knew the reform would damage their reelection prospects.

4.2.2 Compensation in 1880s Britain

The Liberal Party leadership wished to pass the Redistribution of Seats Bill in order to achieve their goal of franchise extension. But the redistribution of seats would damage the electoral prospects of sitting Liberal MPs. What forms of compensation were available to party leaders to maintain party discipline and ensure the passage of the bill?

There are several ways in which parties may have rewarded or compensated members during this time period. Rush (2001) documents how members in the nineteenth century often hoped to receive honors such as peerages or baronetcies or decorations such as knighthoods. Although granted by the monarch, the Prime Minister was (and is to this day) able to recommend people for such titles (Guttsman 1963; Mason 2015). These honors and

[9] The splitting of the dual-member constituencies was done in such a way as to separate the rural and urban parts of certain constituencies, thereby protecting Conservative seats in some rural and suburban areas (Carstairs 1980; Dunbabin 1988; Evans 2000; Mason 2015).

decorations would allow the party to compensate those who lose their seat in Parliament as they are not dependent on membership in the House of Commons. In fact, peerages also confer a seat in the House of Lords and thus prevent one from serving in the House of Commons in the future. Another way MPs theoretically may have been compensated with extra-parliamentary roles during this time was through diplomatic appointments such as ambassadorships. However, using data from Mackie (2017), I determined that of the more than 600 MPs who were sitting at the time the Redistribution of Seats Bill was passed, only five ever served as diplomats (and only two did so after 1885). Thus, empirically, diplomatic appointments were not a common form of compensation at this time and I do not analyze them further below.

There are also several ways in which parties could have rewarded members during the time period even if they retained their seat in the House of Commons. Sitting members could be rewarded with parliamentary offices, up to and including cabinet positions. Additionally, as parties became increasingly cohesive and organized in the second half of the nineteenth century (including developing extra-parliamentary organizations; Rush 2001), this opened up the possibility of compensation in the form of party appointments (either within Parliament or as compensation if someone lost their seat). However, due to data availability, I focus on peerages, baronetcies, knighthoods, and appointments to parliamentary offices.

In order to understand how knighthoods, baronetcies, and peerages serve as forms of compensation, it is worth briefly considering the role of honors in nineteenth century Britain. Guttsman notes that in the nineteenth century, "a knighthood, a Baronetcy and, above all, a Peerage were often bestowed as a reward for help given or in anticipation of favours [*sic*] to be received" (1963, 116). Moreover, over the course of the second half of the nineteenth century, as the party system developed, so did "the practice of rewarding men and women for services to their party," including "for faithful but inconspicuous partisanship on the backbenches" (Guttsman 1963, 120, 121). Although Parliament became increasingly diverse during the nineteenth century (Rush 2001), MPs were still relatively wealthy so the benefits

of such appointments are likely to be primarily in terms of prestige, and in the case of peerages and parliamentary offices (particularly cabinet positions) continued or expanded policy influence, rather than economics.

At the time, the vast majority of peerages were hereditary. The life peerages common today were introduced in 1958, but there were a handful of "Law Lords" during the late-nineteenth century that were granted a non-hereditary peerage on the basis of their legal expertise. Peerages, unlike knighthoods and baronetcies, entitle the holder to sit in the House of Lords and therefore disqualify them from taking a seat in the House of Commons in the future. Importantly, because peerages are hereditary, I only count *new peerages* as a form of compensation since inheriting a title is clearly not an appointment controlled by the party. Similarly, in the case of baronetcies, which are effectively hereditary knighthoods (thus above most knighthoods, but the lowest form of honor and so below peerages), I only count *new baronetcies* as a form of compensation and do not count inherited baronetcies.

4.2.3 Theoretical Expectations in this Case

What are the theoretical expectations in the context of the 1885 Redistribution of Seats Bill? Recall, the Liberals expected that this reform would damage their reelection prospects (due to the move away from dual-member seats and increased competition). For some types of reforms (or legislation more generally), it might be possible to identify ex ante specific members who will be at risk and thus in need of additional incentives to support the proposal. In this context however, I argue that this would have been nearly impossible, despite knowledge of the proposed district boundaries ahead of time. The reason is that district boundaries as well as district magnitude changed. Additionally, only the previous year, the franchise was extended. Although no election had occurred under the new franchise, it was widely expected that the extension of the franchise would benefit the Liberals. The expected countervailing effects of the two reforms (Bogdanor 1981; Butler 1963; Carstairs 1980; Dunbabin 1988; Evans 2000; Mason 2015) combined with changing district boundaries

would have made it difficult to predict the election outcome in individual districts (and of course, this was also before widespread polling of the sort we see today). Moreover, a further complication was that it was relatively common at this time for individuals to change the districts in which they ran (completely independently of the reform). This ability to switch districts would have further complicated any effort to predict who would be most at risk within the Liberal Party. In summary, the reform put everyone at risk. At the margins, some would have been more at risk than others, but overall, in terms of what could be reasonably predicted ex ante, the election risk posed by the reform was relatively uniform.

In this type of relatively uniform risk scenario, I argue that all Liberal backbenchers are roughly equally in need of inducements to vote in favor of the reform. However, if they support the bill, the *form* their reward or compensation takes is likely to differ depending on their fate in the first election after the reform (the 1885 election). Those who lose in 1885 are more likely to be compensated in forms that do not require the recipient to be a sitting legislator — through appointments to new peerages, baronetcies, and knighthoods. In contrast, those who succeed in keeping their seat in Parliament after the 1885 election are more likely to be rewarded for supporting the reform through appointments within Parliament — to offices including cabinet positions (consistent with the existing literature on party discipline in the UK; Benedetto and Hix 2007; Eggers and Spirling 2014b; Kam 2009). Since we are unable to observe promises of compensation or rewards, I focus on the following observable implications of the general theory applied to this case:

Hypothesis 4.1. Among Liberals who run in the 1885 election and lose, those who supported the Redistribution of Seats Bill are more likely to receive compensation in the form of peerages, baronetcies, and knighthoods than those who did not support the reform.

Hypothesis 4.2. Among Liberals who run in the 1885 election and win, those who supported the Redistribution of Seats Bill are more likely to receive rewards in the form of parliamentary offices (up to and including cabinet positions) than those who did not support the reform.

In the case of those who do not win in the 1885, I focus on those who *ran* since retirement may or may not have been induced by the reform itself. Of those who chose not to run in 1885, some proportion would have retired from Parliament at this time regardless, while others may have been induced to retire prematurely because of the expectation of a difficult election or election loss in 1885. While the latter group may also have been compensated by the party leadership if they supported the bill, empirically it is impossible to disentangle the group of retirees who would be eligible for compensation from those who would not. Therefore, I focus on those who ran and lost.

Finally, it is worth observing that it is technically possible for an individual to receive any of these forms of compensation (peerage, baronetcy, knighthood, or parliamentary office) regardless of their fate in the 1885 election. Knighthoods and baronetcies in particular, can be given regardless of whether the recipient is a member of Parliament. Peerages can likewise be awarded to someone whether or not they are an MP. I argue they are more likely to go to those who lose because peers are disqualified from sitting in the House of Commons in the future, but theoretically, a peerage could be awarded to a sitting MP who would then vacate their seat in the Commons. Lastly, while parliamentary offices can only be awarded to sitting members of Parliament, losing the 1885 election is not a guarantee that one can never win reelection. Indeed, there was also an election in 1886 which would have provided an opportunity to resume serving in the Commons even if one lost in 1885. However, similar to peerages, I argue that rewards in the form of parliamentary offices are more likely for those who win in 1885 even if it is possible for those who lost to secure one at some point.

4.3 Research Design

The data for this paper come from several sources. First, I use data collected by Eggers and Spirling (2014a) to determine a) the relevant sample of members of Parliament (i.e., those who were sitting at the time of the vote on the Redistribution of Seats Act[10]), the party

[10] The total number of MPs in my data after accounting for early exits due to death or resignation is 636.

affiliation of all MPs,[11] and c) the vote choice of each individual MP. While a number of divisions on the bill occurred, I focus on the Third Reading or the last vote in the House of Commons before it was sent to the House of Lords for approval for two reasons. First, this is the division in which MPs were voting on the version of the text that most closely matches the final Act. Second, the other votes on the bill that went to a division (in other words, for which I can obtain information on the way individual MPs voted) were all on amendments (either those made in the Commons or reviewing those added in the Lords) or procedure rather than on the full text of the bill itself (Eggers and Spirling 2014a; Hansard, n.d.). In the division on the Third Reading on May 11, 1885, 117 MPs (from both the Conservative and Liberal parties) voted in favor of the reform with 34[12] against (Eggers and Spirling 2014a).[13] Second, most of the data on appointments and the data necessary for many of the control variables come from Rush (2001). Rush (2001) has extensive information on all UK MPs from 1868 through the end of the twentieth century and, most importantly for my purposes, is the source of the information on peerages, baronetcies, and knighthoods.[14] Finally, data on parliamentary offices comes from Eggers and Spirling (2014a).[15]

I use this data to test Hypotheses 4.1 and 4.2. While the average rate of turnover in the House of Commons between 1874 and 1900 was 42.1%, in 1885 (the first election after the Redistribution of Seats Act took effect), turnover reached a high for that period of 60.6% (Rush 2001; see also, Evans 2000). Of the 117 MPs that voted in favor of the reform at the Third Reading division, 36 (or 30.8%) both ran in the next election and lost, while 63 (or 53.8%) won in 1885 (figures calculated by the author using data from Eggers and

[11] Specifically, I use party affiliation in 1885. Although there is a concern that this variable could be post-treatment (after the vote on the bill) due to the way Eggers and Spirling (2014a) code the variable, all results are robust to using party affiliation in 1880 (please see Appendix C for additional details).
[12] The Hansard (n.d.) indicates 116 ayes and 33 nos. I have been unable to account for this one-vote discrepancy in both categories between the Hansard and the data of Eggers and Spirling (2014a).
[13] A total of 151 votes when there were 652 MPs (636 currently sitting) at the time may seem exceedingly low. While it is somewhat low, it is not abnormal for the time period — in 1871, between 100 and 199 MPs voted in 26.5% of divisions, while in 1887, the corresponding figure is 23.8% (Rush 2001, 147).
[14] Although there is some missing information about the dates of knighthood and baronetcy appointments that I supplement with secondary sources.
[15] Appendix C provides additional details about the data and descriptive statistics.

Spirling 2014a). In particular, I focus on rewards and compensation by the Liberal Party. The reason is that the Conservatives expected to benefit from the reform, meaning that any Conservative MPs who voted in favor of it were doing so with the expectation of benefiting and thus, should be less likely to have needed an inducement to support the bill.

Thus, I analyze whether or not Liberal MPs who voted in favor of the reform were more likely (relative to other Liberal MPs) to receive compensation in the form of (new) peerages, baronetcies, or knighthoods or rewards in the form of cabinet positions or parliamentary offices. While I conduct some analyses with the full sample of Liberal MPs, I then focus on whether, among those who ran and lost in 1885, those who supported the reform were more likely to receive new peerages, baronetcies, or knighthoods and whether, among those who won in 1885, those who supported the reform were more likely to receive new appointments to parliamentary offices. The dependent variables are indicators for receiving one of these appointments following the passage of the Redistribution of Seats Act — *New Peerage*, *New Baronetcy or Knighthood*[16], *Cabinet Member*, and *Office Holder* (the latter includes cabinet positions, but also non-cabinet parliamentary offices). My coding of both *Cabinet Member* and *Office Holder* adjusts that of Eggers and Spirling (2014a) to exclude offices that were exclusively held by peers or that are determined by seniority rather than appointment.

I use linear probability models for ease of interpretation. The results presented here consider appointments made in 1886 (in other words, in the year following the reform).[17] I focus on appointments within a year because this allows sufficient time for the appointment to occur (for example, in the case of a knighthood, to allow for the time it takes to recommend the appointment to the monarch and for that decoration to be conferred), while also keeping in mind that if compensation or rewards for support are given, they are likely to occur relatively quickly after 1885 (the party leadership has a long time horizon, but within limits). I specifically use the one-year cutoff rather than other possible cutoffs (e.g., two or

[16] I group knighthoods and baronetcies because they are grouped in the original data source (Rush 2001), presumably because baronetcies are hereditary knighthoods.

[17] Although the results are similar if one considers appointments within five years of 1885 (coded as appointments between 1886 and 1890, inclusive).

three years) because the Liberals were out of power between mid-1886 and 1892, severely restricting their ability to make appointments. Moreover, the Liberals and Conservatives were each in power for roughly six months in 1886 meaning that each party would have had the opportunity to make appointments to both parliamentary and extra-parliamentary positions that year.[18] That said, if anything, using 1886 alone is an underestimate of the true extent of appointments because it is possible that compensation and rewards were given in 1885 after the Redistribution of Seats Bill passed. However, as I have only the year of the appointments and not the exact date, I exclude any 1885 appointments since I cannot confirm they occurred after the Third Reading.

The independent variable of interest is an indicator for whether or not the individual voted in favor of the Redistribution of Seats Act at the Third Reading — *Support*. I code *Support* as one if the individual voted in favor of the bill and zero if they either voted against or abstained. Since the analysis is limited to Liberal MPs, in practice, *Support* captures whether an MP supported the bill or abstained as no Liberal MPs voted against the bill. However, coding the variable in this way should, if anything, bias against finding results (increasing confidence in results I do find) because even those who abstained in the Third Reading division may have voted in favor of the reform in other votes on the bill. This means that there may be some MPs in my data who are coded as abstainers, but who, at other times, voted in favor of the reform and thus, who the party may need to compensate.

I also include a variety of controls. First, I control for whether an individual held a cabinet position at the time of the Third Reading. One would expect cabinet ministers to both be more likely to vote in support of a government bill and more likely to receive appointments — in particular, peerages after leaving Parliament and other cabinet positions or offices while still in the Commons. While I present the results controlling for those who were cabinet ministers at the time the vote was taken, the results are generally robust to instead using a control for whether an individual was a cabinet minister at any time prior to the vote on

[18] That the Conservatives could also make appointments in 1886 will be important for placebo tests which I discuss below.

the Redistribution of Seats Bill. Second, I use a series of dummy variables to control for the occupation of the MP when they entered Parliament and their level of education because it is possible that the party only compensated or rewarded those members who had relatively poor outside options (and it is also possible that the quality of outside options affected an MP's willingness to support a bill that might cost him his seat). Moreover, controlling for occupation also controls for whether an individual was a military officer (at least at the time he entered Parliament) which should be a particularly important predictor of honors and decorations. Additionally, I control for whether the MP had an aristocratic connection. This variable is coded as one if the MP is the son of a peer or baronet and zero otherwise. Note that this does not necessarily mean the MP would stand to inherit the title (as the MP could be a younger son). However, this type of connection may affect the likelihood the individual is given an appointment (particularly a peerage) as well as their willingness to support a bill that would damage their reelection prospects (as this can be thought of as another measure of their outside options). Finally, I control for the individual's age in 1885. Those who are older may have a longer period of service to the party and in Parliament which might affect the likelihood they would receive an appointment upon leaving office or a parliamentary office while in the Commons. Age may also affect one's willingness to support the reform (e.g., if I am planning to retire in the near future anyway, I may be more likely to support the reform regardless of any inducements offered by the party).[19]

4.4 Analysis

Table 4.1 shows the correlation between supporting the Redistribution of Seats Bill at the Third Reading (*Support*) and receiving a peerage (*New Peerage*) or a baronetcy or knighthood (*New Baronetcy or Knighthood*) in 1886. These models include only those who were members of the Liberal Party in 1885 since it is this group who would have been expected to

[19] I do not control for whether the individual won in the 1885 election (even though that clearly ought to predict appointments) because it would be post-treatment to *Support*.

Table 4.1: Support for Redistribution of Seats and Extra-Parliamentary Appointments Among Liberals

	Dependent variable:			
	New Peerage		New Baronetcy or Knighthood	
	(1)	(2)	(3)	(4)
Support	0.035**	0.040*	−0.019	−0.023
	(0.017)	(0.022)	(0.019)	(0.023)
Constant	0.006	−0.005	0.029**	−0.028
	(0.010)	(0.068)	(0.011)	(0.068)
Controls	no	yes	no	yes
Observations	270	212	270	212
R^2	0.016	0.054	0.004	0.031

Note: Coefficients and standard errors from linear probability models. Controls include cabinet member (at Third Reading), level of education, occupation when the MP entered Parliament, aristocratic connection, and age. *p<0.1; **p<0.05; ***p<0.01

be harmed by the passage of the bill and who we would therefore expect to be compensated.[20]

I expect those who vote in favor of the Redistribution of Seats Bill should be more likely to receive a peerage, baronetcy, or knighthood than those who abstain. As columns 1 and 2 of Table 4.1 shows, those who supported the bill at the Third Reading were more likely to receive a peerage in 1886. Specifically, the results indicate that Liberal MPs who support the bill at the Third Reading are approximately 4% more likely to receive a peerage in the following year than those who abstained. While this may seem like a small effect, only 43 individuals who served as members of Parliament at the time of the Third Reading were awarded a new peerage in the subsequent twenty years, across all parties. Indeed, in some ways, appointments to peerages are a particularly hard test of the theory since peers are entitled to sit in the House of Lords (and therefore could no longer be elected to the House of Commons in the future).[21]

[20] I restrict the analysis to those who are coded as Liberals by Eggers and Spirling (2014a), excluding those coded as members of Liberal-affiliated parties. The results are often stronger if a more expansive coding rule is used, but in the years following 1885, some of these affiliated groups broke away from the Liberal Party.
[21] Recall, because peerages are primarily hereditary at this time, I only count appointments to new hereditary peerages, not those who inherited their title.

As columns 3 and 4 of Table 4.1 show, I find no evidence that supporting the Redistribution of Seats Bill at the Third Reading is correlated with receiving a new baronetcy or knighthood in the following year. While contrary to expectations, this suggests that these types of honors and decorations are deployed differently (at least in this time period). While knighthoods and baronetcies confer prestige, peerages confer both (even greater) prestige and a continued role in Parliament as a member of the House of Lords. In other words, peerages also confer continued policy influence. It is noteworthy that it appears compensation takes a form that allows for a continued role in government. Overall, the results in Table 4.1 suggest that those who supported the party even when doing so was expected to weaken their chances of reelection are compensated by the party for their loyalty with peerages in the year immediately following the reform, but not with knighthoods or baronetcies.

Knighthoods, baronetcies, and particularly peerages are attractive ways to compensate MPs who did not win reelection because they do not require that the individual is sitting in the House of Commons (and in the case of peerages, precludes maintaining a seat in the Commons). However, theoretically, all Liberal MPs were in danger of losing their seats at the election and thus may have needed an incentive to support the redistribution of seats. Those who supported the bill and won the 1885 election may still have been rewarded for their loyalty. This reward could have taken the form of knighthoods or baronetcies (although the results above suggest that was not used as a reward regardless of one's fate in the 1885 election), but it might also have taken the form of offices within Parliament. Table 4.2 shows the correlation between support for the bill and subsequently receiving a cabinet position (*Cabinet Member*) or parliamentary office more generally (*Office Holder*). Again, only members of the Liberal Party in 1885 are included.

Table 4.2 provides some evidence that individuals were rewarded with parliamentary offices if they supported the redistribution of seats. While the coefficient on *Support* is not always statistically significant, the direction is consistent across all models. Unsurprisingly, by far the largest predictor of cabinet membership or parliamentary office in the year after

Table 4.2: Support for Redistribution of Seats and Parliamentary Office Among Liberals

	Dependent variable:			
	Cabinet Member		Office Holder	
	(1)	(2)	(3)	(4)
Support	0.022	0.019	0.098**	0.063
	(0.024)	(0.026)	(0.041)	(0.048)
Constant	0.029**	−0.037	0.087***	0.309**
	(0.014)	(0.078)	(0.025)	(0.143)
Controls	no	yes	no	yes
Observations	270	212	269	211
R^2	0.003	0.354	0.021	0.223

Note: Coefficients and standard errors from linear probability models. Controls include cabinet member (at Third Reading), level of education, occupation when the MP entered Parliament, aristocratic connection, and age. *p<0.1; **p<0.05; ***p<0.01

the vote is being a member of the cabinet at the time of the Third Reading (coefficients omitted due to space constraints). Additionally, these models consider appointments to these positions regardless of whether or not one was still in Parliament after 1885 which likely artificially inflates the number of zeros in the dependent variables. Further, only 24 individuals received a cabinet appointment in 1886 across all parties (only 10 Liberals) making this a particularly hard test of the theory. Despite these limitations, appointment to parliamentary offices (which includes cabinet positions) is correlated with support for the redistribution of seats in the model without controls, providing tentative evidence that this is a form of reward for loyalty to the party despite the risks this bill posed to their future.

The correlation between *Support* and subsequent appointments to new peerages, baronetcies, knighthoods, and parliamentary offices demonstrated in Tables 4.1 and 4.2 is suggestive evidence that legislators were compensated and rewarded for supporting a bill that had the potential to cause them to lose their seats. However, those results consider all Liberal Party members, while the theory suggests that the particular rewards given should depend on one's fate in the 1885 election. Those who did in fact legislate themselves out of office — those

who ran and lost in the 1885 election — should be most likely to receive compensation in the form of peerages, baronetcies, and knighthoods while those who held on to their seats in 1885 should be more likely to be rewarded within Parliament.

Therefore, Table 4.3 shows the results of regressing indicators for new appointments to peerages, baronetcies, and knighthoods on a restricted sample — Liberal MPs who ran for reelection in 1885 and lost. I exclude those who chose not to run in 1885. While some of those who chose not to run may have been ready to retire for reasons entirely unrelated to the redistribution of seats, others may have felt forced to retire early due to the reform. However, because there is no way to separate these two groups of retirees, I consider compensation only for those who ran and lost. Columns 1 and 2 of Table 4.3 show the correlation between *Support* and *New Peerage* in the subsequent year. Columns 3 and 4 show the correlation between *Support* and *New Baronetcy or Knighthood* in 1886.

Table 4.3 provides partial support for Hypothesis 4.1. The coefficient on *Support* is only significant when considering appointments to peerages, however in this sample, it falls below significance when full controls are included. However, it is possible that the large drop in sample size both when only those who ran and lost are considered and again when controls are added could account for the decrease in the precision of the estimates. Nevertheless, there is some evidence that among those who ran and lost in the 1885 election, individuals who supported the Redistribution of Seats Bill at the Third Reading are approximately 6% more likely to receive a peerage in the subsequent year than those who abstained. As with the full sample of all Liberal MPs, columns 3 and 4 suggest that knighthoods and baronetcies were not used as a form of compensation. Once again, it appears that the primary way those who legislated themselves out of office were compensated allowed them to play a continued role in governing — through a peerage and seat in the House of Lords.

Table 4.4 further explores the intuition that the inducement to support the redistribution of seats may have resulted in different types of appointments for different categories of MPs. Here, I consider appointments to cabinet positions or any parliamentary offices only among

Table 4.3: Support for Redistribution of Seats and Extra-Parliamentary Appointments Among Liberals Who Ran and Lost in 1885

	Dependent variable:			
	New Peerage		New Baronetcy or Knighthood	
	(1)	(2)	(3)	(4)
Support	0.057*	0.067	−0.021	−0.036
	(0.034)	(0.045)	(0.024)	(0.033)
Constant	−0.000	−0.076	0.021	0.025
	(0.022)	(0.150)	(0.016)	(0.111)
Controls	no	yes	no	yes
Observations	83	68	83	68
R^2	0.034	0.132	0.009	0.062

Note: Coefficients and standard errors from linear probability models. Controls include cabinet member (at Third Reading), level of education, occupation when the MP entered Parliament, aristocratic connection, and age. *p<0.1; **p<0.05; ***p<0.01

those Liberal MPs who won the 1885 election. As above, columns 1 and 2 show the results of regressing appointment as a *Cabinet Member* in 1886 on *Support*, while columns 3 and 4 show the results when *Office Holder* is used as the dependent variable.

Although the coefficient on *Support* never achieves significance, the direction of the coefficient is generally consistent across models and consistent with Hypothesis 4.2 that among Liberals who ran in 1885 and won, those who supported the reform are more likely to be rewarded with an appointment than those who did not. Again, the strongest predictor of holding a cabinet position or any parliamentary office is being a member of the cabinet at the time of the Third Reading (coefficient omitted). In addition to the reduced sample size and small number of cabinet positions, another possible explanation for the lack of significant results is that, while I consider one's fate in the 1885 election, there was another election in 1886. Thus, it is possible that some MPs included in this sample lost their seats in 1886 and therefore were not actually in Parliament to receive these appointments, thus inflating the number of zeros in the dependent variable. Unfortunately, the lack of precision in the estimates, possibly due to one or all of these factors, precludes strong conclusions on the

Table 4.4: Support for Redistribution of Seats and Parliamentary Office Among Liberals Who Won in 1885

	Dependent variable:			
	Cabinet Member		Office Holder	
	(1)	(2)	(3)	(4)
Support	0.017	−0.009	0.145	0.096
	(0.054)	(0.061)	(0.089)	(0.100)
Constant	0.070*	−0.093	0.211***	0.663**
	(0.036)	(0.196)	(0.059)	(0.323)
Controls	no	yes	no	yes
Observations	103	78	102	77
R^2	0.001	0.422	0.026	0.359

Note: Coefficients and standard errors from linear probability models. Controls include cabinet member (at Third Reading), level of education, occupation when the MP entered Parliament, aristocratic connection, and age. *p<0.1; **p<0.05; ***p<0.01

basis of these results.

4.5 Robustness Tests

The evidence presented thus far suggests that Liberal MPs who supported the Redistribution of Seats Bill at the Third Reading were more likely to receive peerages and parliamentary offices in the subsequent year, however the results are weaker when 1885 election winners are separated from those who ran and lost in 1885. I argue that appointments serve as an inducement from the party to vote in favor of a bill that reduced the chances of reelection for all Liberal MPs (and compensation if one did lose their seat). However, there is no way to observe offers of rewards or compensation. Given that, how can we be sure the appointments observed above are really a form of compensation or reward for support on *this bill*?

To explore this question, I use a series of placebo tests to estimate the correlation between support for the Redistribution of Seats Bill and appointments among those who should not have needed an inducement to support the bill — Conservative MPs. Indications that

Support and subsequent appointments are correlated in these analyses would suggest that the main results were picking up something other than evidence of a reward for supporting this particular bill.

Recall that although the bill was proposed by the Liberal Government, the redistribution of seats was actually a way to satisfy the Conservative Party in order to induce Conservatives in the House of Lords to agree to the extension of the franchise the previous year. Moreover, the Redistribution of Seats Bill was expected to benefit the Conservatives and therefore their MPs should be less likely to need appointments as a way to induce support in the vote. Since I am unable to track behind-the-scenes promises of compensation for Liberal MPs, if support on the bill is *not* correlated with appointments for Conservatives, that increases confidence that these appointments are serving as a reward or compensation for the Liberal Party.

Table 4.5 shows the results of models analogous to the baseline models in Table 4.1. Once again, *New Peerage* and *New Baronetcy or Knighthood* appointments in 1886 are regressed on an indicator for *Support* on the Third Reading of the bill and the results of linear probability models are shown. Interestingly, unlike the Liberals who expected this bill to disadvantage them, there were some Conservatives who voted against the bill. Thus, in this case, *Support* equals one if the individual voted in favor of the bill and zero if they either voted no or abstained. Further, because the Conservatives were the opposition at the time and I do not have data on membership of the opposition frontbench (i.e., the shadow cabinet), in these models, I use an indicator for whether the individual was in cabinet at any time *before* the Third Reading (rather than the control for cabinet member at the time of the vote that I use for the analyses with the Liberal Party).

The results in Table 4.5 show there is no correlation between *Support* for the reform and appointment to a *New Peerage* or *New Baronetcy or Knighthood* among Conservative MPs. In fact, the coefficients on *Support*, while not significant, are consistently negative. As expected, among Conservative MPs who should not have needed any compensation, there is no evidence that their vote on this bill is correlated with subsequent grants of peerages,

Table 4.5: Support for Redistribution of Seats and Extra-Parliamentary Appointments Among Conservatives

	Dependent variable:			
	New Peerage		New Baronetcy or Knighthood	
	(1)	(2)	(3)	(4)
Support	−0.013	−0.015	−0.030	−0.025
	(0.046)	(0.048)	(0.069)	(0.076)
Constant	0.013*	0.022	0.030***	0.121
	(0.007)	(0.063)	(0.011)	(0.099)
Controls	no	yes	no	yes
Observations	243	216	243	216
R^2	0.0003	0.238	0.001	0.054

Note: Coefficients and standard errors from linear probability models. Controls include cabinet member (any time before Third Reading), level of education, occupation when the MP entered Parliament, aristocratic connection, and age. *p<0.1; **p<0.05; ***p<0.01

baronetcies, or knighthoods. This increases confidence that new peerage appointments are significantly related to support on this bill for Liberal MPs because these MPs were compensated for their willingness to risk legislating themselves out of office.[22]

Table 4.6 presents an analysis analogous to that in Table 4.2. Here, I explore whether support for reform is correlated with appointments to the cabinet or parliamentary office more broadly in the subsequent year. In addition to the sample, the only differences between this analysis and that in Table 4.2 are the slightly altered coding of *Support* and the alternative measure of whether one was a cabinet member at any time prior to the vote used as a control, as discussed above. With the exception of one model in which the coefficient on *Support* is significant, the results in Table 4.6 generally support the intuition that Conservatives did not need an inducement for supporting the Redistribution of Seats Bill since it was expected to benefit them and therefore they were not rewarded in the subsequent year. Moreover, if one considers only Conservatives who *won* in 1885 (i.e., the sample analogous to

[22] Unfortunately, it is not possible to consider appointments to new peerages, baronetcies, or knighthoods among Conservatives who ran and lost in 1885 (analogously to the analysis in Table 4.3) because there is no variation in *Support* for that group.

Table 4.6: Support for Redistribution of Seats and Parliamentary Office Among Conservatives

	Dependent variable:			
	Cabinet Member		Office Holder	
	(1)	(2)	(3)	(4)
Support	0.112	0.171**	0.023	0.025
	(0.096)	(0.081)	(0.146)	(0.141)
Constant	0.055***	0.109	0.143***	0.634***
	(0.015)	(0.106)	(0.023)	(0.184)
Controls	no	yes	no	yes
Observations	243	216	243	216
R^2	0.006	0.483	0.0001	0.303

Note: Coefficients and standard errors from linear probability models. Controls include cabinet member (any time before Third Reading), level of education, occupation when the MP entered Parliament, aristocratic connection, and age. *p<0.1; **p<0.05; ***p<0.01

that in Table 4.4), *Support* is never correlated with cabinet appointments or appointments to parliamentary offices.[23] This further increases confidence that Conservatives were not rewarded for supporting the Redistribution of Seats Bill in the same way the Liberals were.

4.6 Conclusion

Due to a focus on seat maximization incentives and frameworks that treat parties as unitary actors, the electoral reform literature has traditionally (with some notable exceptions, e.g., Cox, Fiva, and Smith 2019; Gandhi, Heller, and Reuter 2020; McElwain 2008; Remington and Smith 1996) overlooked intra-party dynamics that affect the ability of parties to pass reform through the legislature. Thus, as a whole, the literature rarely addresses cases in which electoral reforms pass that do not increase the seat share of the party overall and/or harm the electoral prospects of some legislators or in which electoral reforms that would increase the party's seat share fail to pass due to revolts by backbenchers. In this paper, I relax the unitary-party assumption and consider what affects party backbenchers' support

[23] See replication materials for the results.

for electoral reform. In particular, why would legislators support reform that they believe may cost them their seat, effectively legislating themselves out of office?

While the party discipline literature recognizes that party leaders have a variety of "carrots and sticks" they can use to induce legislators to fall in line (e.g., Benedetto and Hix 2007; Bowler, Farrell, and Katz 1999; Cheibub 2007; Cox 1987; Cox and McCubbins 1993; Eggers and Spirling 2014b; Kam 2009), some of these inducements and threats are ineffective in the context of an electoral reform that legislators believe will cause them to lose their seat. I argue that similar carrots that do not require the recipient to hold elected office can still be used to induce support for electoral reform. In particular, I argue that backbenchers will be more willing to support reform that damages their electoral chances if they believe they will be provided with patronage in the form of extra-parliamentary appointments or support in the event they lose their seats and possibly rewards in parliament if they are able to retain their seats despite the risks posed by the reform.

Using the case of the 1885 Redistribution of Seats Act in the UK, I have shown that Liberal MPs who supported the bill are more likely receive peerages in the year immediately following the reform and there is some evidence those who supported the reform are more likely to be rewarded with parliamentary offices after the reform as well. While the lack of strong evidence in favor of the hypothesis that among Liberals who won in 1885, those who supported the bill at the Third Reading are more likely to receive parliamentary offices is somewhat unexpected, it does suggest an interesting alternative interpretation of the results. In the theory, I focused on the fact that, due to the relatively uniform risk faced by all Liberal MPs, they would all be equally likely to require additional inducements to support the reform. This led to the expectation that both those who won and lost in the next election would be rewarded for their support, but that the form of the reward would differ. In contrast, the results are consistent with "carrots" offered by the party leadership to compensate MPs, but only in the event they lose the election. In other words, while the inducement may be offered prior to the election, the realization of it may be contingent on

the outcome of the election and reserved for those who were in fact harmed by the reform.

While I leverage data from the UK to test the theory, I believe it is more widely applicable. I ground the hypotheses in the case because the specific form compensation and rewards take may depend on the case, but the general mechanism and theory that individual legislators may need to be induced to support reform and that these inducements may include compensation outside the legislature apply across contexts. Other possible forms of compensation or rewards besides those studied here might include additional financial transfers to aid imperiled candidates in the election following reform, diplomatic appointments, appointments to positions within the party (either that do not require one to simultaneously hold an elected position as compensation for a loss or that do require elected office in the case of rewards for those who manage to retain their seats despite the risks posed by the reform), or even appointments to executive branch positions in presidential systems if the wider party is sufficiently strong to offer such an inducement. Future studies should explore these additional methods of compensation and reward in other contexts beyond the UK.

Finally, returning to the motivating examples that intra-party politics matter for electoral reform — the failure of reforms in Japan and Russia — the present analysis also raises a new question. Why are party leaders in some cases unable or unwilling to use inducements to ensure backbenchers support reforms favored by the leadership? One possible explanation is that other systems have fewer options for compensating members who lose their seats and are more reliant on inducements within the legislature. Future work exploring this question can improve our understanding of both party discipline and electoral reform.

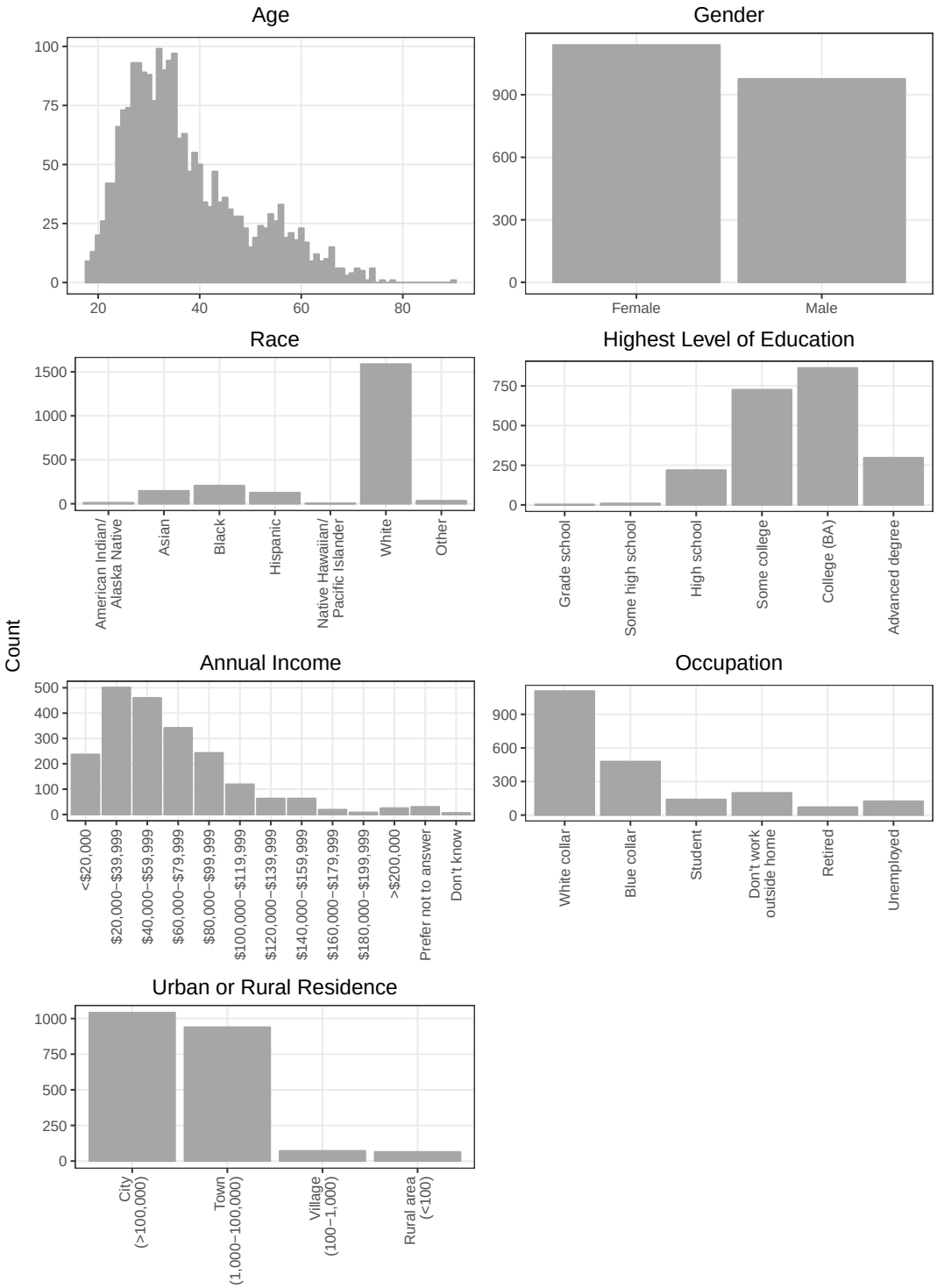
Appendix A

Appendix to "The Battle over Gerrymandered Districts: How Americans Balance Fairness and Partisanship"

A.1 Descriptive Statistics

In this section, I provide descriptive statistics summarizing the distribution of respondent characteristics and preferences expressed in the survey. Figure A.1 shows the distribution of demographic characteristics in the sample. As expected with an online survey, the sample is relatively young, with a mean age of 37 and a median age of 34. Additionally, there are more women (54%) than men and the sample is predominantly caucasian (75%). However, as noted in the text, this is similar to other MTurk surveys (Berinsky, Huber, and Lenz 2012). Furthermore, most respondents have completed at least some college (89%) and most respondents characterize their work as white collar (52%). Unsurprisingly, given the corresponding population distribution, the distribution of annual income in the sample is

Figure A.1: Distribution of Demographic Variables in Sample



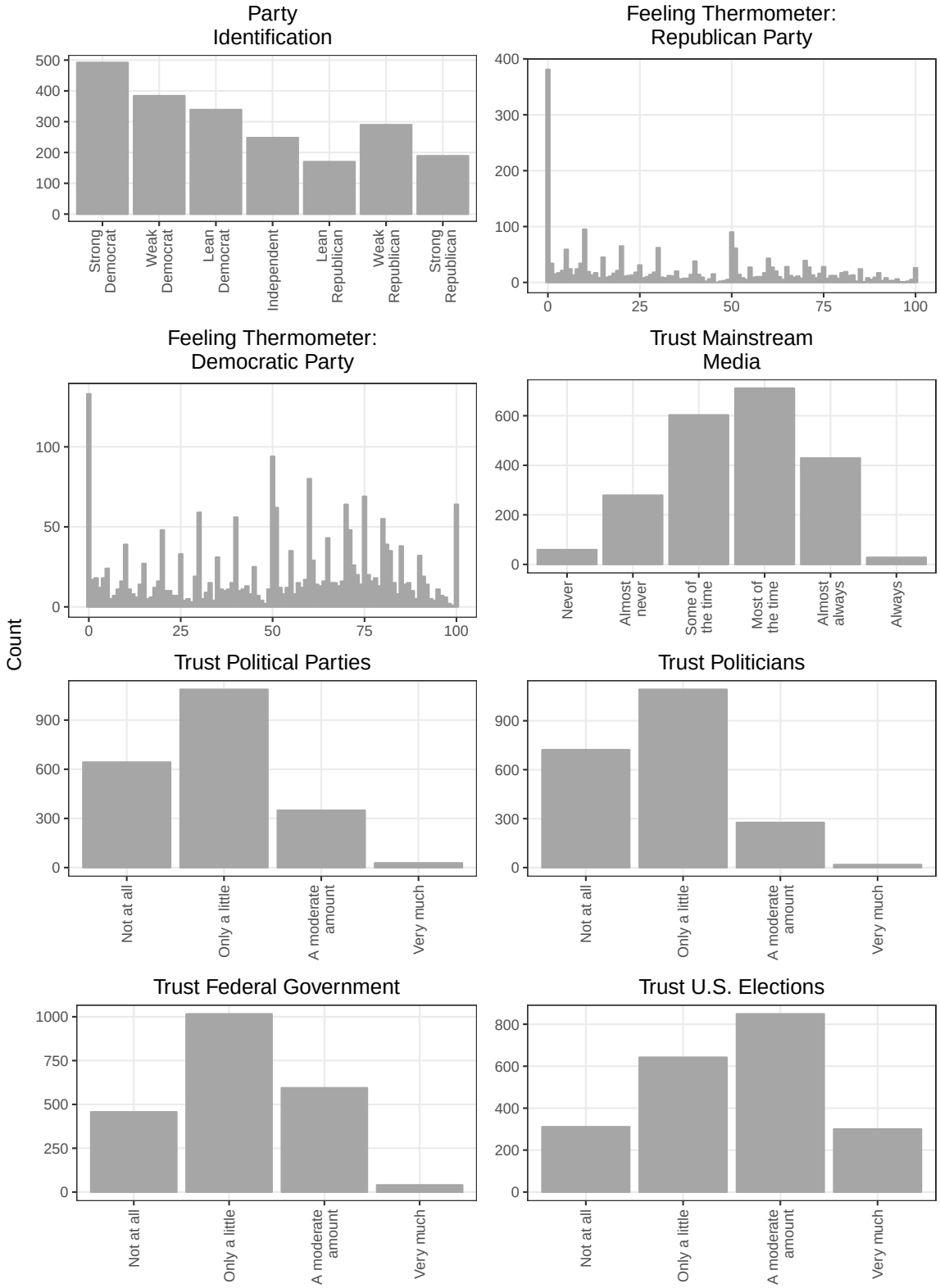
Note: Plots (gender, occupation, and urban/rural residence) omit missing values. One respondent indicated their age was "90 or older"; that is recoded as 90 for this figure.

right skewed. Finally, the vast majority of the sample is drawn from relatively urban areas, with 94% of respondents describing their primary place of residence as a city or town (a population of 1,000 or more).

Figure A.2 shows the distribution of political opinions in the sample. As noted in the text, the sample consists of more Democrats (57% identify with or lean towards the Democrats) than independents (12%) or Republicans (31% identify with or lean towards the Republicans).[1] However, this distribution is in accordance with those found in other MTurk surveys (Berinsky, Huber, and Lenz 2012). Given that the sample leans Democratic, it is unsurprising that more respondents rate the Republican Party low (negatively) on the feeling thermometers than the Democratic Party. Additionally, Figure A.2 shows that most respondents have low levels of trust in a variety of democratic institutions. A majority of respondents indicated that they had no trust or only a little trust in political parties (82%), politicians (86%), and the federal government (70%). Interestingly, given the current political climate, some of the highest levels of trust are those displayed for the mainstream media and U.S. elections — 55% of respondents indicated that they trusted the mainstream media to report the news fairly at least 50% of the time (this corresponds to answering "most of the time," "almost always," or "always"), while 57% indicated that they trusted elections "very much" or "a moderate amount."

[1] Four respondents answered don't know to the party identification questions.

Figure A.2: Distribution of Political Opinion Variables in Sample

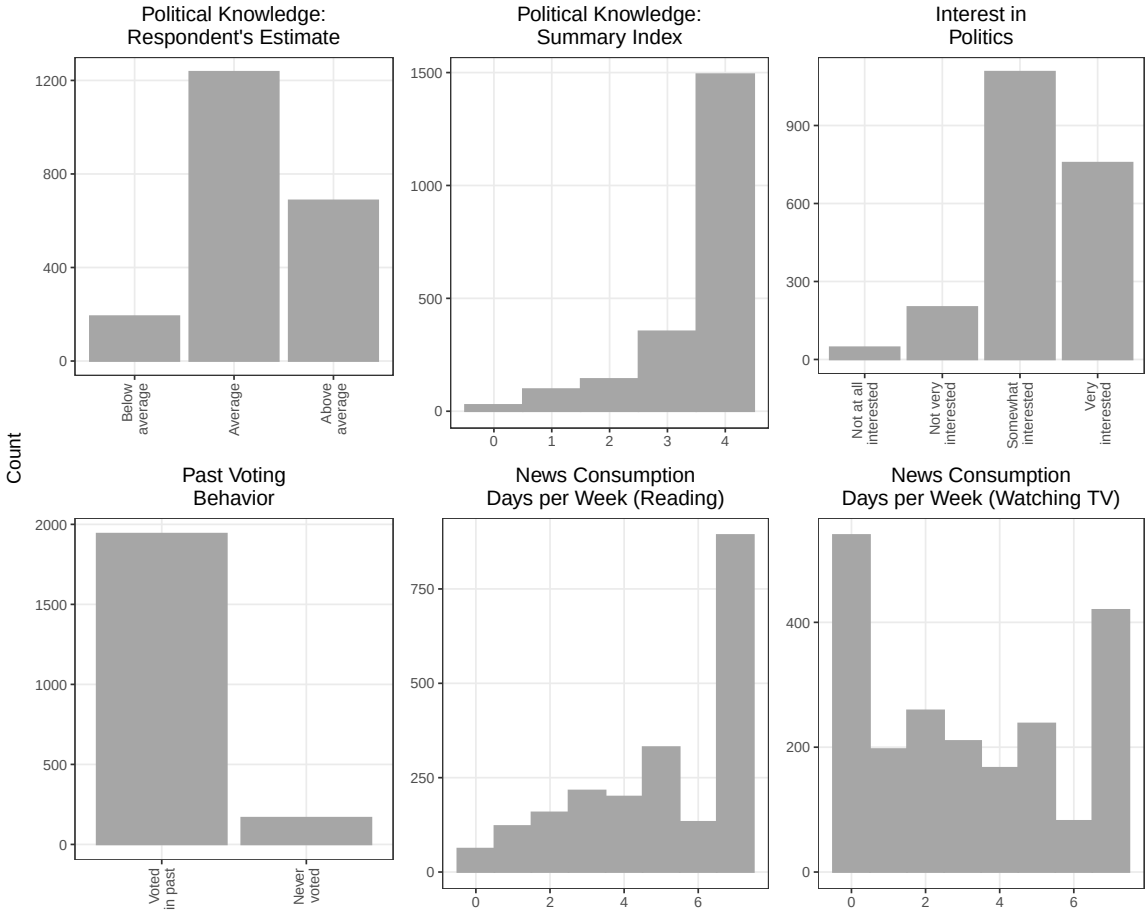


Note: All plots omit don't know responses. Feeling thermometers run from 0 (one does not particularly like the party) to 100 (one feels favorably towards the party); 50 corresponds to neutral.

Figure A.3 shows the distribution of several other characteristics of respondents in the sample. There are two measures of political knowledge included in the survey. The first is an estimate by the respondent of their own level of knowledge of politics (58% estimated their own knowledge as "average"). This question was included because there is a concern with self-administered, online surveys that respondents will simply look up the answers to factual questions. Nevertheless, I also included four factual questions about U.S. politics. These were used to create an additive index of political knowledge running from zero to four; 71% of respondents obtained a perfect score on this index. Given that respondents opted into completing the survey (due to MTurk's design), it is perhaps unsurprising that 88% of respondents indicated they are somewhat or very interested in politics and that the vast majority (92%) of respondents reported having voted at some point in the past. Finally, in accordance with their interest levels, many of the respondents report consuming news (either reading it in print or online or watching it on television) multiple times per week.

Finally, the sample used for the main analyses in the text excludes respondents who failed at least one of two attention checks. Inattentive respondents are a particular concern with self-administered survey experiments, but recent research indicates that "screener" questions or Instructional Manipulation Checks (IMCs) allow researchers to identify attentive respondents (Berinsky, Margolis, and Sances 2014; Goodman, Cryder, and Cheema 2013). The first attention check was a fill-in-the-blank state of residence question at the beginning of the survey. This ensured that respondents were at least paying enough attention to type the name of their state as opposed to some other word or phrase (only 20 people — 1% of the sample — failed this attention check). The second, more conventional, attention check was placed in the middle of the survey (i.e., closer to the experiment) and adapted from Berinsky, Margolis, and Sances (2014). Respondents were presented with the following text: "People often get information about breaking news stories from online news sources. I am interested in learning about which news websites people trust. I am also interested in learning whether or not people pay attention to the survey questions. To demonstrate

Figure A.3: Distribution of Additional Variables in Sample



Note: Plots omit missing values (interest and news consumption) and don't know responses (interest and past voting behavior).

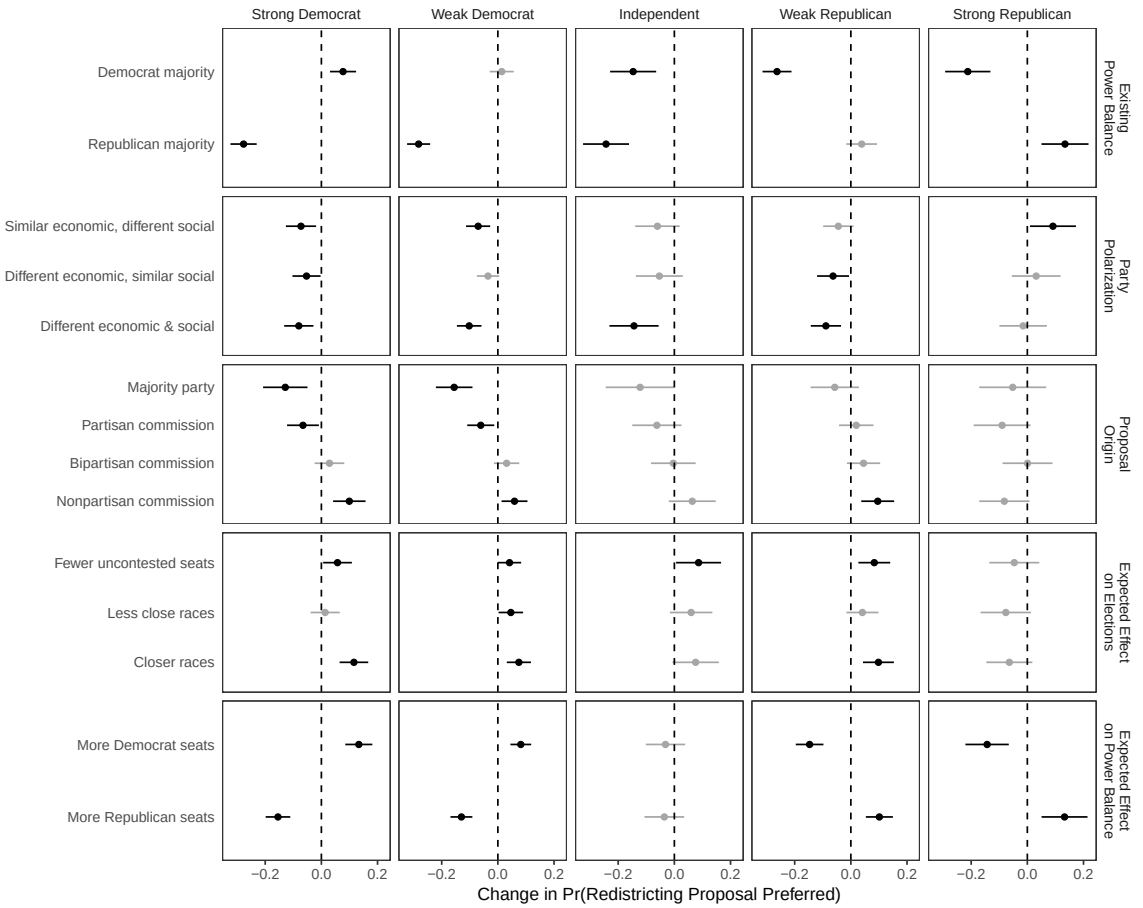
you've read this, please ignore the below question and select both CNN and Fox News as your answers." After a small vertical space, respondents saw the question, "When you are interested in getting information about breaking news, which news website do you visit first? (Please select only one)." Respondents were able to choose from twelve named news websites, "Some other website," or "I don't go online for news." Of the 2,116 respondents, 319 (15%) failed this attention check (11 respondents failed both).

A.2 Supplementary Analyses and Robustness

In this section, I present additional details regarding the analyses described in the text. Figure A.4 shows the results of one of the tests of Hypothesis 2.1 in which I estimate the average marginal component effects conditional on respondent partisanship (collapsed to a five-point scale combining weak and leaning partisans). The results provide partial support for Hypothesis 2.1. For most groups, fairness affects their decision regarding redistricting scenarios at least as much as partisanship does. All groups (except strong Republicans) are between 4 and 8% more likely to prefer redistricting scenarios which result in fewer uncontested seats compared to those which are expected to increase the number of uncontested seats. Additionally, strong Democrats, weak or leaning Democrats, and weak or leaning Republicans are all more likely to prefer redistricting scenarios that are expected to result in closer elections on average relative to those that are expected to increase the number of uncontested seats (the corresponding estimate for independents is in the expected direction, but not significant). This suggests that in the absence of polarization, fairness in terms of election outcomes does affect the evaluation of redistricting scenarios for most groups.

Figure A.4 also shows that, on average, fairness in terms of the proposal origin also affects respondents' redistricting choices. Strong Democrats, weak or leaning Democrats, and weak or leaning Republicans are all between 6 and 10% more likely to prefer proposals made by nonpartisan districting commissions relative to those made by a bipartisan committee of state legislators. Strong Democrats and weak or leaning Democrats are also less likely to support redistricting scenarios when the proposals were made by the majority party in the state legislature or by partisan commissions. Although none of the estimates for independents are significant at the 95% confidence level, all coefficients are in the expected direction. The effects of the attribute values that operationalize fairness are similar in magnitude to the effects of partisan attribute values (for all groups except strong Republicans). Since fairness has a significant effect, but does not outweigh partisanship across all party identifications, this is partial support for Hypothesis 2.1.

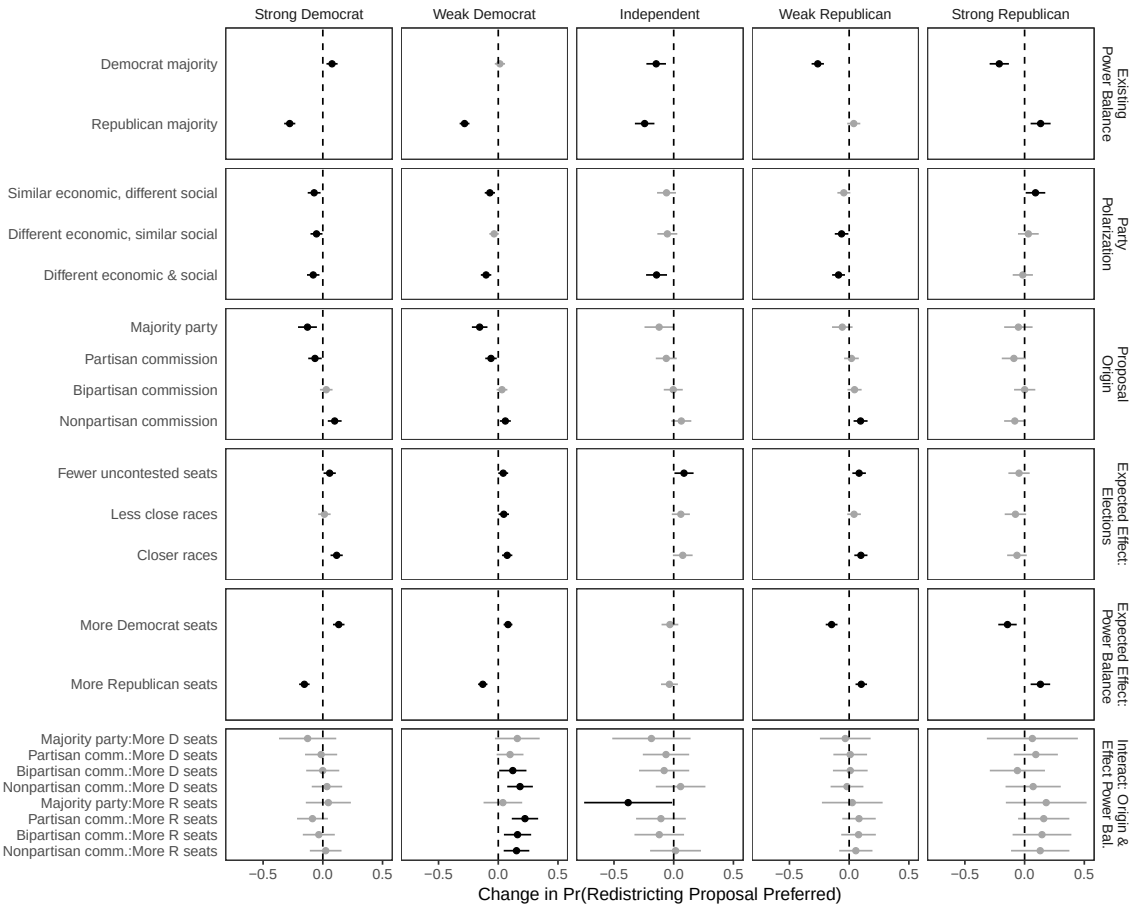
Figure A.4: Effects of Attributes on the Probability Redistricting Scenario is Preferred by Party ID



Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Weak partisans include independents who lean towards the party. Baseline attribute values are: divided control, similar economic and social, bipartisan legislative committee, more uncontested seats, and no change.

As described in the text, I conduct further tests of Hypothesis 2.1 by interacting the fairness attributes and the *expected effect of the proposals on the legislative balance of power*. Figure A.5 shows the results from the models interacting the *proposal origin* and the *effect of the proposal on the balance of power*. As expected, most of the interaction terms are insignificant. The only exceptions are for independents and weak or leaning Democrats. Independents, on average, are less likely to support a redistricting proposal made by the majority party in the legislature if that proposal will result in an increased number of seats for Republicans. Weak or leaning Democrats are more likely to support redistricting proposals

Figure A.5: Effects of Attributes (Including Interaction Between Proposal Origin and Partisan Outcome) on Probability Redistricting Scenario is Preferred by Party ID

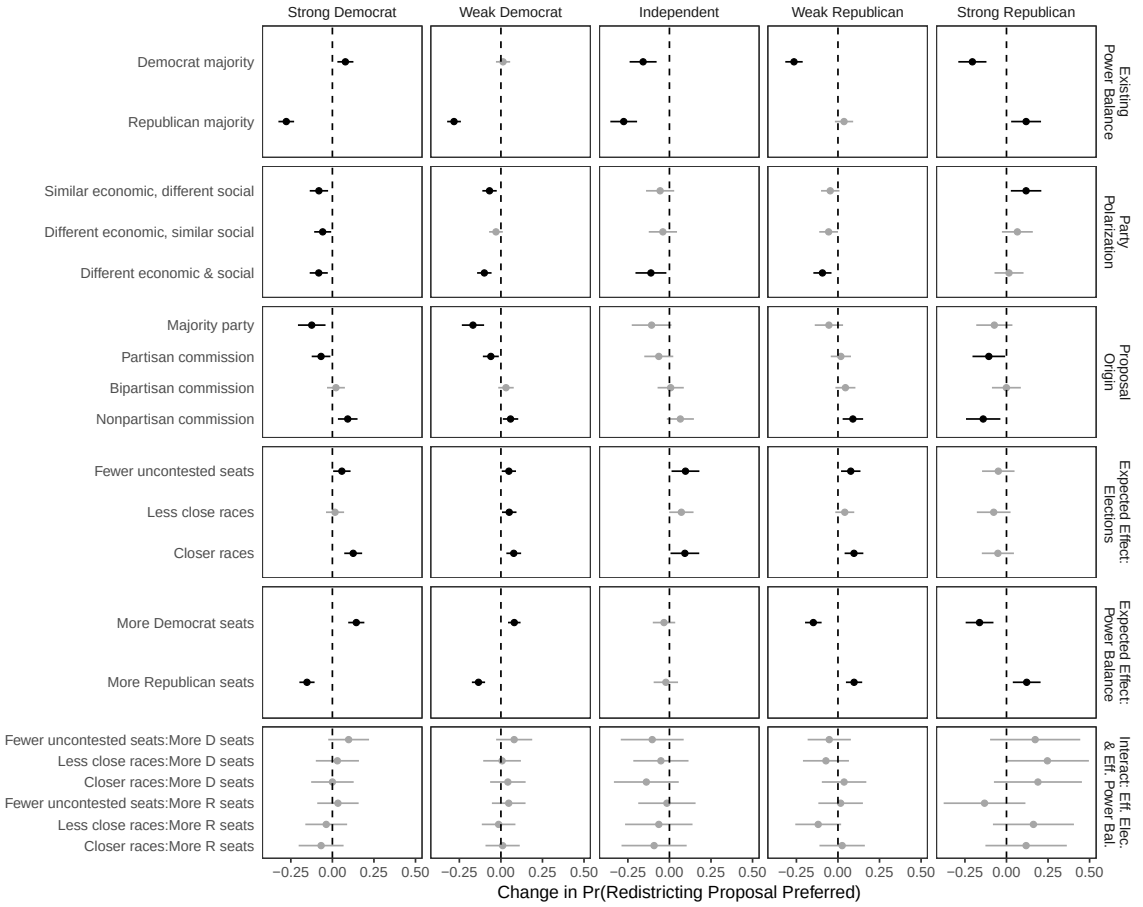


Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Weak partisans include independents who lean towards a party. Baseline attribute values are: divided control, similar economic and social, bipartisan legislative committee, more uncontested seats, no change, and bipartisan legislative committee:no change.

that result in an increased number of seats for either Democrats or Republicans if those proposals are made by bipartisan or nonpartisan committees. Perhaps most surprisingly, weak or leaning Democrats are also more likely to support redistricting proposals that increase the number of seats for Republicans if they are made by a partisan committee.

Figure A.6 shows the results from models that interact the *effect of the proposal on elections* and the *partisan outcome* attributes. All interaction terms are insignificant, which supports Hypothesis 2.1. Thus, overall, these the results that explore the AMCEs conditional on party identification support the conclusions drawn from the unconditional AMCEs

Figure A.6: Effects of Attributes (Including Interaction Between Effect on Elections and Partisan Outcome) on Probability Redistricting Scenario is Preferred by Party ID

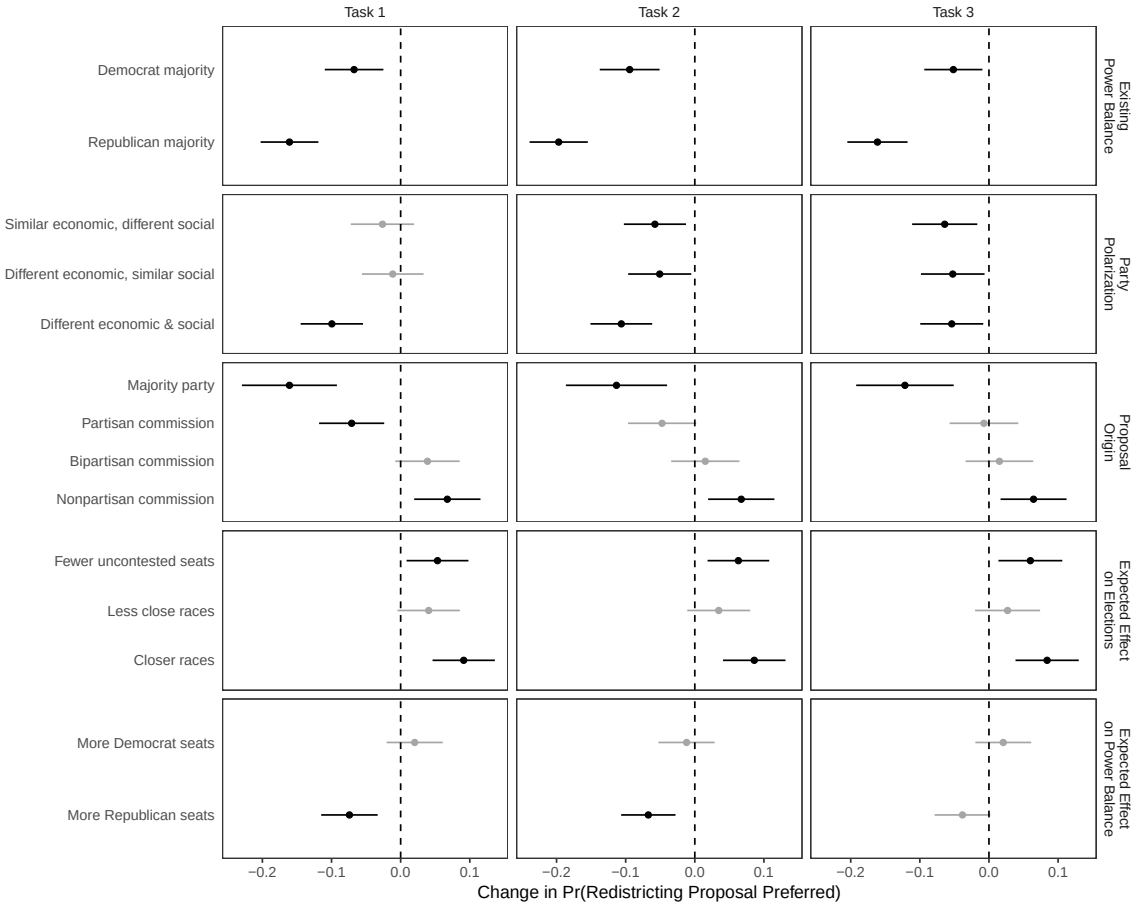


Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Weak partisans include independents who lean towards a party. Baseline attribute values are: divided control, similar economic and social, bipartisan legislative committee, more uncontested seats, no change, and more uncontested seats:no change.

presented in the text.

Next, I turn to tests of the identification assumptions. For the quantities of interest to be nonparametrically identified, three assumptions must hold. First, there must be no carryover effects (Hainmueller, Hopkins, and Yamamoto 2014). It must be that respondents would choose the same scenario in a given task, regardless of the scenarios they had seen in previous tasks. I test the plausibility of this assumption in two ways. First, I verify that the AMCEs are similar across each of the three tasks in the experiment. Figure A.7 shows the average marginal component effects (AMCEs) conditional on the task (recall that

Figure A.7: Effects of Attributes on Probability Scenario is Preferred by Task



Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Baseline attribute values are: divided control, similar economic and social, bipartisan legislative committee, more uncontested seats, and no change.

respondents completed three tasks in the experiment that consisted of choices between pairs of redistricting scenarios). Figure A.7 confirms that the results are similar across tasks in the experiment. Second, I evaluate if the results hold using only the data from the first task. This test is not ideal because precision is reduced given the drastic reduction in sample size. Indeed, the sample size becomes so small as to make running some of the models testing Hypothesis 2.2 problematic. Nevertheless, the results are generally similar between the main results and this robustness test (where possible), suggesting this is a reasonable assumption.[2]

Second, another assumption that must hold for the relevant estimates to be nonpara-

[2] See replication materials for results.

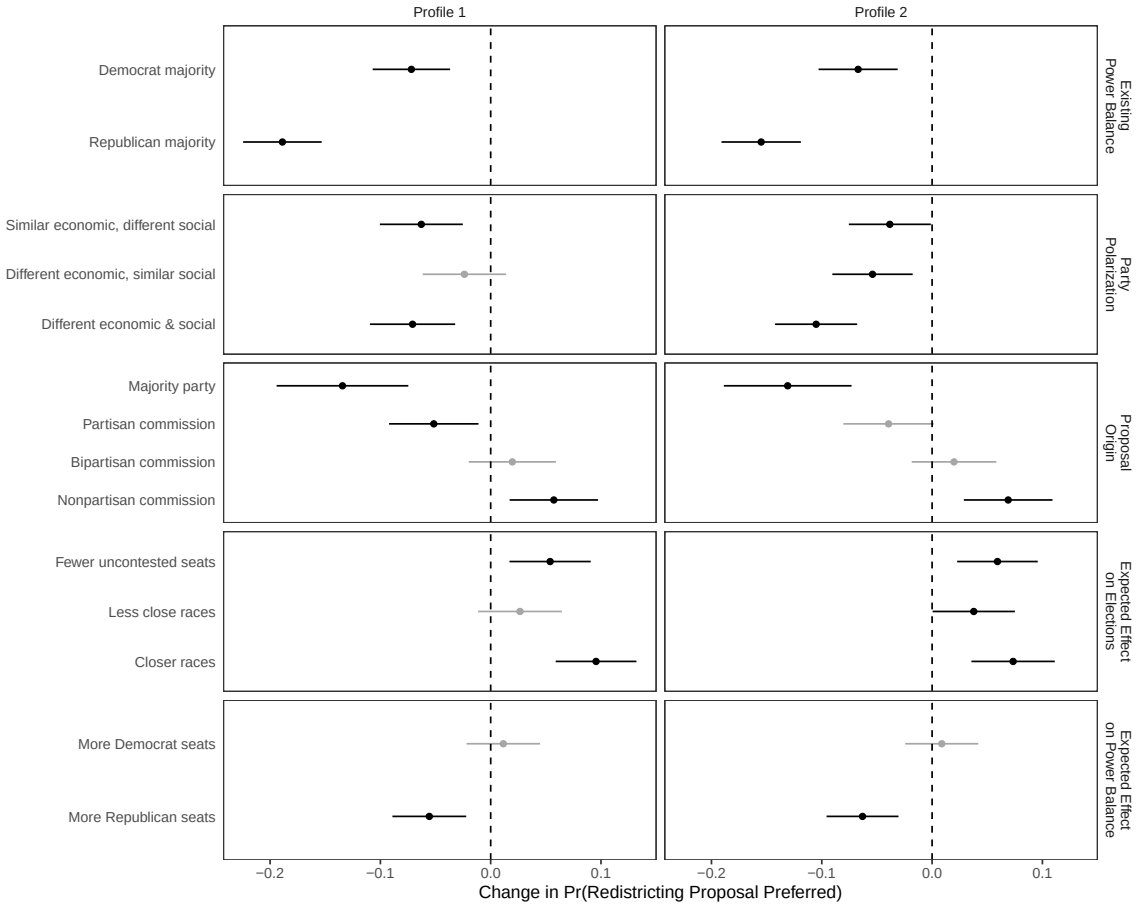
metrically identified is that there must not be profile order effects (Hainmueller, Hopkins, and Yamamoto 2014). It must be that respondents would select the same profile regardless of whether it was presented first or second in a given task. I test the plausibility of this assumption by estimating the AMCEs conditional on the profile. As shown in Figure A.8, the results are similar across profiles, suggesting that this assumption is likely to hold.

Third, the conjoint's randomization ensures profiles are independent from potential outcomes (Hainmueller, Hopkins, and Yamamoto 2014). However, this assumption is guaranteed to hold only at the population level. Therefore, I run a series of multivariate balance tests in which respondent characteristics — age, gender, race, highest level of education, income, occupation, and urban residence — were regressed on a full set of attribute indicators. Ideally, the attributes should be jointly insignificant. Indeed, that is the case for the regressions involving age, gender, income, occupation, and urban residence; in those cases, the attributes are well balanced. For race and education, the attributes are not jointly insignificant. However, this is due to the very small sample sizes for some of the race and education categories. For example, only four respondents identify as Native Hawaiian or Pacific Islander and only two completed only grade school. When race is recoded to be a dichotomous indicator for whether a respondent identified as white and education is recoded so that the lowest level of education was high school or less, F-tests indicate the attributes are jointly insignificant. Thus, I conclude that balance is not a significant problem in the experiment.[3]

Finally, in the text, I describe a non-causal analysis based on a unique manipulation check embedded in my experiment. The manipulation check consisted of an open-ended question at the end of each task asking respondents why they chose the redistricting scenario they did when they were forced to make a choice. I then coded these responses for use in the analysis. Table A.1 shows the complete list of possible codes as well as the coding rules. Importantly, these categories are not mutually exclusive so that any given manipulation check could receive multiple codes. Even fairness and partisanship can both be reasonable

[3] See replication materials for results.

Figure A.8: Effects of Attributes on Probability Scenario is Preferred by Profile



Note: Standard errors clustered by respondent. 95% confidence intervals shown (significant estimates in black). Baseline attribute values are: divided control, similar economic and social, bipartisan legislative committee, more uncontested seats, and no change.

reasons for a given choice since the scenario might be such that both preferences work in the same direction. The only exception is that a manipulation check could not be coded as both failed (or failed alternative) and fall into another category.[4]

I then used these coded manipulation checks to conduct the analysis. Specifically, the analysis used two dependent variables. The first was an indicator (*Fairness*) for whether or not a respondent referenced fairness or an attribute relating to fairness in any of their three manipulation checks. This indicator took a value of one if any of the respondent's manipulation checks were coded as fairness general, fairness proposer, or fairness outcome,

[4] Failed and failed alternative are separated because it is unclear if writing a single word is more akin to a nonsense response or to shorthand for a reasonable answer.

Table A.1: Manipulation Check Coding Rules

Code	Rule
Fairness general	R based choice on aversion to gerrymandering, or R explicitly referenced fairness (in general sense)
Fairness proposer	R based choice on preference for bipartisan proposers, or R based choice on preference for nonpartisan proposers, or R based choice on dislike of partisan proposers
Fairness outcome	R based choice on preference for fewer uncontested seats, or R based choice on preference for more competition
Balance	R based choice on preference for divided power in legislature, or R referenced the balance of the scenario, or R expressed preference for no single party controlling all decisions
Partisanship general	R based choice on the scenario favoring a party, and R did not specify what about the scenario favored the party
Partisanship current	R based choice on preference for party with current legislative majority, or R based choice on dislike for party with current legislative majority
Partisanship outcome	R based choice on expected partisan outcome of proposal, and R specified they preferred increased (decreased) seat share for party X
Partisanship parties	R based choice on level of party polarization
Confused	R indicated they were confused or didn't understand
Other	R gave other or vague reason for choice, or R wrote "no change in seat share" (unless they clarify), or R wrote "same as before" for 3rd check and previous responses differed
Failed	R failed manipulation check by writing a string of gibberish, or a number, or a single letter/symbol, or "NA"/"no comment"
Failed alternative	R failed manipulation check by writing single word, and word was not a party name, "balance," "nonpartisan," "bipartisan," or "unsure"/"confused"

Note: R is respondent. Categories are not mutually exclusive, except failed and failed alternative. The word "same" was coded as failed alternative because it is unclear if their reason is the same as a previous reason or if the profiles happened to be exactly the same.

and zero otherwise. The second dependent variable was an indicator (*Partisanship*) for whether or not a respondent referenced partisan concerns or an attribute that related to partisanship in any of their three manipulation checks. This indictor was one if any of the respondent's manipulation checks were coded as partisanship general, partisanship current, or partisanship outcome, and zero otherwise. Note that the partisanship indicator was coded as zero if the respondent referenced the level of polarization between the parties but did not reference any other partisan concerns. The results of this analysis are presented in the text.

A.3 Survey Instrument

Before presenting the survey instrument, it is worth noting that my design slightly differed from a basic conjoint design. Basic conjoint designs completely randomize the attributes, but I deviated from complete randomization in several ways. First, the order of the attributes was not completely randomized. To minimize confusion, attributes related to the proposal were always grouped together, as were attributes related to the state.[5] Second, there were two restrictions on combinations of attribute values to exclude illogical scenarios. First, the majority party in the state legislature can only make a proposal if either the Democrats or the Republicans currently have a majority in both chambers. Second, if the majority party makes a proposal, the *effect on the balance of power* can only take the values no change in seat share or the party that has the current majority benefits.[6]

1. What is your age?

    a. *There was a drop-down menu from which the respondent selected.*

2. What state do you currently live in?

    a. *There was a fill-in-the-blank box for respondents to answer this question.*

3. How would you describe the place where you live? (If you live in more than one place, please select the option that best describes where you spend the most time.)

    a. A city (about 100,000 people or more)

    b. A town (about 1,000–100,000 people)

    c. A village (about 100–1,000 people)

[5] In practice, I used the Conjoint Survey Design Tool (Strezhnev et al. 2014), so attributes appeared in the order of a) *existing balance of power*, *party polarization*, *proposal origin*, *expected election outcomes*, *expected partisan outcomes*; or b) *expected election outcomes*, *expected partisan outcomes*, *existing balance of power*, *party polarization*, *proposal origin*.

[6] Given the level of information available to legislators when drawing electoral districts, we would not expect them to draw districts benefitting the opposing party.

   d. A rural area (fewer than 100 people)

4. What is your gender?

   a. Male

   b. Female

5. Please choose the primary race that you consider yourself to be.[7]

   a. American Indian or Alaska Native

   b. Asian

   c. Black or African-American

   d. Hispanic

   e. Native Hawaiian or Pacific Islander

   f. White or Caucasian

   g. Other

6. What is the highest level of school you have completed or the highest degree you have received?[8]

   a. Grade school — 8 grades or less

   b. Some high school — 9–12 grades (no diploma)

   c. High school — 12 grades with a diploma or equivalent

   d. Some college — community/junior college degree (AA degree), college without a degree

   e. College — BA level degree

   f. Advanced degree

[7] Adapted from the American National Election Studies (2016) and the General Social Survey (2016).
[8] Question and adapted answers from the American National Election Studies (2016).

7. What is your total annual household income before taxes or deductions?[9]

    a. Less than $20,000 per year

    b. $20,000–$39,999

    c. $40,000–$59,999

    d. $60,000–$79,999

    e. $80,000–$99,999

    f. $100,000–$119,999

    g. $120,000–$139,999

    h. $140,000–$159,999

    i. $160,000–$179,999

    j. $180,000–$199,999

    k. $200,000 or above

    l. Don't know

    m. Prefer not to answer

8. Which of the following best describes your occupation?

    a. White collar

    b. Blue collar

    c. Student

    d. Don't work outside the home (e.g., stay at home parent)

    e. Unemployed

    f. Retired

[9] Question adapted from the British Election Study (2011) with answer options adapted from the General Social Survey (2016).

9. Some people pay a lot of attention, while others don't pay much attention to politics. How would you rate your own interest in politics?[10]

    a. Very interested

    b. Somewhat interested

    c. Not very interested

    d. Not at all interested

    e. Don't know

10. Have you voted in at least one national, state, or local election in the last 10 years?

    a. Yes, I've voted

    b. No, I haven't voted

    c. Don't know

11. (*If individual answers no or don't know to question 10*:) Have you ever voted in a national, state, or local election?

    a. Yes, I've voted

    b. No, I've never voted

    c. Don't know

12. During a typical week, how many days do you read news (either in a newspaper or online)?[11]

    a. None

    b. One day

    c. Two days

[10] Adapted from the American National Election Studies (2016).
[11] This question and the next adapted from the American National Election Studies (2016).

d. Three days

e. Four days

f. Five days

g. Six days

h. Every day

13. During a typical week, how many days do you watch national news on TV?

a. None

b. One day

c. Two days

d. Three days

e. Four days

f. Five days

g. Six days

h. Every day

14. (*Screener question:*) People often get information about breaking news stories from online news sources. I am interested in learning about which news websites people trust. I am also interested in learning whether or not people pay attention to the survey questions. To demonstrate you've read this, please ignore the below question and select both CNN and Fox News as your answers.

When you are interested in getting information about breaking news, which news website do you visit first? (Please select only one)[12]

[12] Adapted from Berinsky, Margolis, and Sances (2014).

a. Huffington Post

b. New York Times

c. Washington Post

d. CNN

e. ABC News

f. CBS New

g. NBC News

h. MSNBC

i. Fox News

j. USA Today

k. The Associated Press (AP)

l. Reuters

m. Some other website

n. I don't go online for news

15. How much of the time do you think you can trust the mainstream media to report the news fairly?[13]

a. Always (100% of the time)

b. Almost always (75–99% of the time)

c. Most of the time (50–74% of the time)

d. Some of the time (25–49% of the time)

e. Almost never (1–24% of the time)

f. Never (0% of the time)

[13] Question and adapted answers from the American National Election Studies (2016).

g. Don't know

16. How would you rate your own knowledge of politics?

    a. Above average

    b. Average

    c. Below average

17. Do you know which party currently has a majority (more than half the seats) in the U.S. House of Representatives? Which one?[14]

    a. Republicans

    b. Democrats

    c. Don't know

18. Do you know which party currently has a majority (more than half the seats) in the U.S. Senate? Which one?

    a. Republicans

    b. Democrats

    c. Neither, both have the same number of seats

    d. Don't know

19. Do you know which of the following people is the current Vice President? Who?

    a. Donald Trump

    b. Paul Ryan

    c. Mike Pence

    d. Mitch McConnell

[14] This question and the next adapted from the American National Election Studies (2016).

e. Don't know

20. Do you know which of the following people is the current Speaker of the House of Representatives? Who?

    a. Nancy Pelosi

    b. Mike Pence

    c. Mitch McConnell

    d. Paul Ryan

    e. Don't know

21. In general, how much would you say you trust political parties?[15]

    a. Very much

    b. A moderate amount

    c. Only a little

    d. Not at all

    e. Don't know

22. In general, how much would you say you trust politicians?

    a. Very much

    b. A moderate amount

    c. Only a little

    d. Not at all

    e. Don't know

23. In general, how much do you trust the federal government?

[15] This question and the next adapted from the British Election Study (2011).

a. Very much

b. A moderate amount

c. Only a little

d. Not at all

e. Don't know

24. In general, how much do you trust elections in this country?

a. Very much

b. A moderate amount

c. Only a little

d. Not at all

e. Don't know

25. Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or something else?[16]

a. Republican

b. Democrat

c. Independent

d. Other

e. No preference

f. Don't know

26. (*If individual responds either Republican or Democrat in question 25*:) Do you think of yourself as a strong *Republican/Democrat* or not a very strong *Republican/Democrat*?

a. Strong *Republican/Democrat*

[16] This question and the following two are adapted from the American National Election Studies (2016).

b. Not very strong *Republican/Democrat*

27. (*If individual responds with independent, other, no preference, or don't know in question 25*:) Do you think of yourself as closer to Republican or Democratic Party?

    a. Closer to the Republican Party

    b. Closer to the Democratic Party

    c. No preference

    d. Don't know

28. Please rate how you feel about the Republican Party on this feeling thermometer. A rating between 50 and 100 degrees means that you feel favorably or warm towards them, while a rating between 0 and 50 degrees means that you don't feel favorably towards them or do not particularly like them. If you feel neutral towards them, you can choose 50 degrees.[17]

    a. *There was a slider allowing respondents to choose whole numbers from 0 to 100.*

    b. Don't know

29. Please rate how you feel about the Democratic Party on this feeling thermometer. A rating between 50 and 100 degrees means that you feel favorably or warm towards them, while a rating between 0 and 50 degrees means that you don't feel favorably towards them or do not particularly like them. If you feel neutral towards them, you can choose 50 degrees.

    a. *There was a slider allowing respondents to choose whole numbers from 0 to 100.*

    b. Don't know

[17] This question and the following were adapted from the American National Election Studies (2016).

30. *See the paper for details of the conjoint design. Here, I provide an example of a single task (each respondent completed three tasks, each on separate screens).*[18]

    (*Introduction (on its own screen in the survey):*) "For the next part of the survey, you will be asked to compare pairs of hypothetical scenarios. Each scenario consists of some information about a U.S. state and a proposal to redraw the districts for state legislative elections in that state. Please read the descriptions of the proposals and states carefully. For each pair, please indicate which scenario you would personally prefer, even if you are unsure. In other words, please indicate which proposal you would prefer to see enacted in its state."

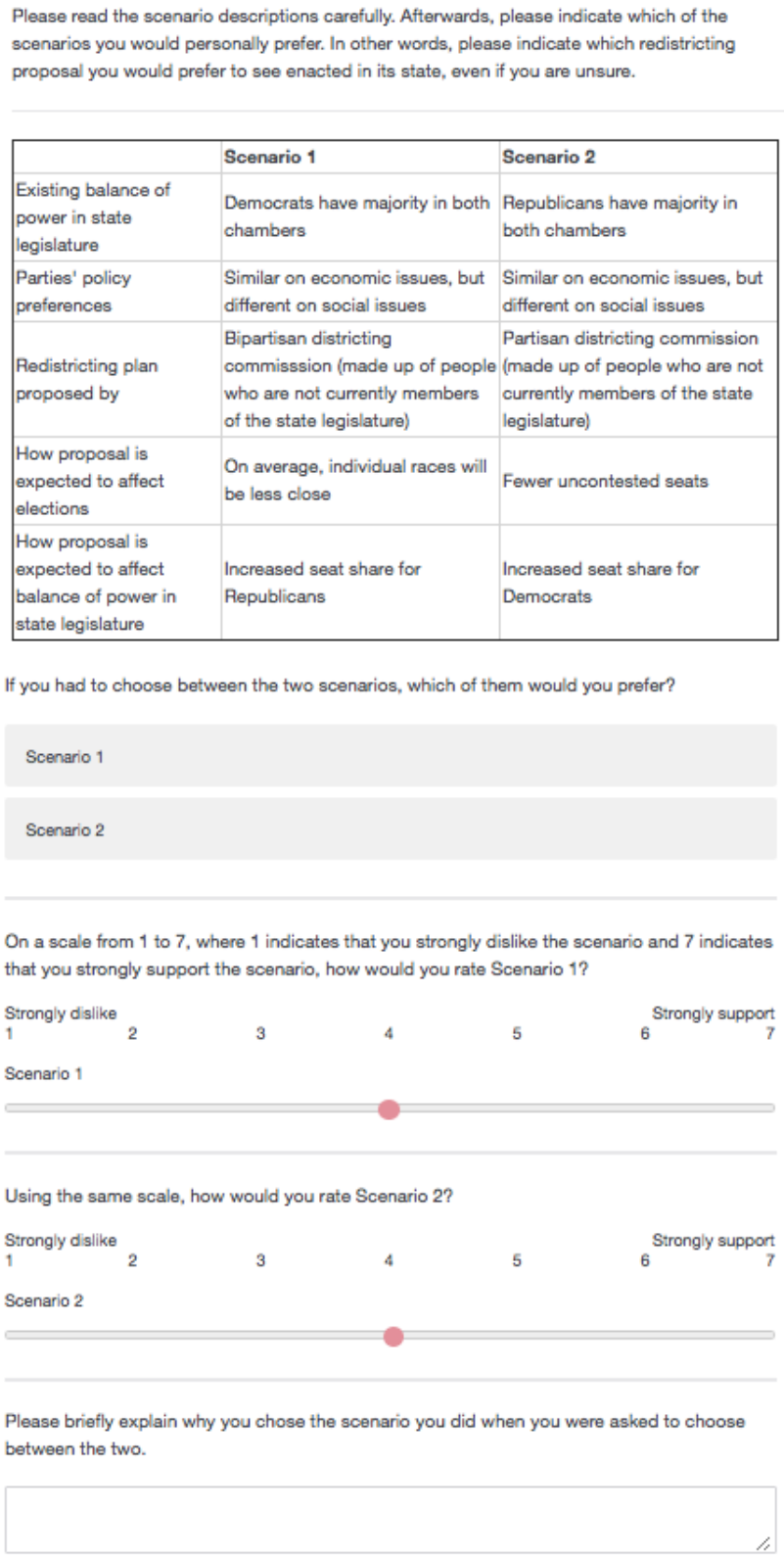
    See example task shown in Figure A.9.

31. Please let me know if you have any feedback about the survey itself. In particular, was there anything you found confusing or unclear?[19]

[18] This design is based on those in Hainmueller, Hopkins, and Yamamoto (2014) and Hainmueller and Hopkins (2015).

[19] This question was included in the full survey as well as the pilot so that I could use the combined sample (i.e., so that there were no differences between the pilot and full survey).

Figure A.9: Example of a Task Completed by Respondents in the Conjoint Experiment



Please read the scenario descriptions carefully. Afterwards, please indicate which of the scenarios you would personally prefer. In other words, please indicate which redistricting proposal you would prefer to see enacted in its state, even if you are unsure.

	Scenario 1	**Scenario 2**
Existing balance of power in state legislature	Democrats have majority in both chambers	Republicans have majority in both chambers
Parties' policy preferences	Similar on economic issues, but different on social issues	Similar on economic issues, but different on social issues
Redistricting plan proposed by	Bipartisan districting commisssion (made up of people who are not currently members of the state legislature)	Partisan districting commission (made up of people who are not currently members of the state legislature)
How proposal is expected to affect elections	On average, individual races will be less close	Fewer uncontested seats
How proposal is expected to affect balance of power in state legislature	Increased seat share for Republicans	Increased seat share for Democrats

If you had to choose between the two scenarios, which of them would you prefer?

Scenario 1

Scenario 2

On a scale from 1 to 7, where 1 indicates that you strongly dislike the scenario and 7 indicates that you strongly support the scenario, how would you rate Scenario 1?

Strongly dislike 1 2 3 4 5 6 Strongly support 7

Scenario 1

Using the same scale, how would you rate Scenario 2?

Strongly dislike 1 2 3 4 5 6 Strongly support 7

Scenario 2

Please briefly explain why you chose the scenario you did when you were asked to choose between the two.

Appendix B

Appendix to "Public Support for Electoral Reform: The Role of Electoral System Experience"

B.1 Survey Data

For this project, I make use of the British Social Attitudes (BSA) survey, which is a national survey (repeated cross-sections) of adults in England, Wales, and Scotland. Specifically, I utilize the survey results from every year in which the survey was fielded (it was not fielded in 1988 or 1992) that includes a question about electoral reform (the question was excluded in 1983–85, 1989, 1993, 2004, 2006–07, 2009, 2012–14, and 2016; NatCen Social Research 2019).[1] However, when I include the political interest control, I am forced to drop data from 1987 since the relevant question was not asked that year. Thus, the data used in the paper spans 1986–2015, but not all years are included.

There are three variables that are particularly critical for the analysis. The first is

[1] If desired, the BSA survey could be supplemented with the Northern Ireland Social Attitudes (NISA) survey. The NISA is a survey of adults in Northern Ireland fielded between 1989 and 1996 (excluding 1992) and an identical electoral reform question to that on the BSA survey was included in 1994 and 1996 (Northern Ireland Social Attitudes 1996).

the region in which the respondent lives. Unfortunately, the BSA survey has changed the regions they use over time (changed the variables used in addition to occasionally changing the coding used within a single variable). I use these variables to code each respondent as living in London, the Midlands, Northern England, Scotland, Southern England (excluding London), or Wales.

The second critical variable is party identification. The questions used to construct the respondents' party identification (called "partyid1" in the raw data) are identical in every year included in my analysis. First, respondents are asked, "generally speaking, do you think of yourself as a supporter of any one political party?" If yes, they are asked, "which one?" and their response to this open-ended question is their party identification (they are a "supporter" in the language of the BSA survey). If they answer no or don't know to the supporter question, they are asked, "do you think of yourself as a little closer to one political party than to the others?" If yes, they are asked, "which one?" and their response is their party identification (they are a "sympathizer" in the BSA survey language). If respondents answer no to the supporter question and no or don't know to the sympathizer question, they are asked, "if there were a general election tomorrow, which political party do you think you would be most likely to support?" These people are "residual identifiers" in the language of the survey and their response to this open-ended question is used as their party identification. I treat all party identifiers (whether supporters, sympathizers, or residual identifiers) as identifiers or supporters (terms which I use interchangeably in the main text).

However, there is one final complication with the party identification. The BSA survey may or may not alter the skip logic slightly in 1996 and later compared to surveys before 1996. Specifically, it is not clear from the documentation prior to 1996 how don't know responses in the supporter or sympathizer questions are treated. My assumption is that don't knows in these questions are always treated in the way described here (which is explicitly the procedure in 1996 and later), but it is not clear from the documentation.

The final critical variable is the electoral reform variable (called "votesyst" in the raw

data). Respondents were asked,

> "Some people say that we should change the voting system to allow smaller political parties to get a fairer share of MPs. Others say that we should keep the voting system as it is, to produce more effective government. Which view comes closest to your own, that we should change the voting system, or, keep it as it is?" (NatCen Social Research 2019; Northern Ireland Social Attitudes 1996).

While this question does not specify an alternative electoral system, enumerators were instructed to elaborate that the question referred to proportional representation if they were asked. This question text was used in the NISA survey and at the start of the BSA survey, but there were some minor changes to the wording over the course of the BSA survey. The exact question wording for each year (when it differs from the original) is shown below:

1. 1997: changes original to "Some people say we[...]to produce effective government[...]"
2. 1998, 1999: changes original to "[...]Others say we should[...]"
3. 2000: changes to "Some people say we should change the voting system for general elections to the (UK) House of Commons to allow smaller political parties to get a fairer share of MPs. Others say we should keep the voting system for the House of Commons as it is, to produce effective government. Which view comes closer to your own, that we should change the voting system for the UK House of Commons, or, keep it as it is?" (If asked, refers to proportional representation)
4. 2001: changes the 2000 text to "[...]Others say that we should[...]Which view comes closer to your own, that we should change the voting system for the House of Commons[...]"
5. 2002: changes the 2000 text to "[...]Which view comes closer to your own, that we should change the voting system for the (UK) House of Commons[...]"
6. 2003, 2005, 2008, 2010, 2011, 2015: change the 2000 text to "[...]to the UK House of Commons[...]Others say that we should[...]Which view comes closer to your own, that we should change the voting system for the House of Commons[...]"

I use this question to construct the main dependent variable for the analyses. *Reform Support* is one if the respondent believes the electoral system should be changed and zero if they answered they preferred to keep the existing electoral system (it is missing if the respondent answered that they don't know or refused to answer, however the main results in the paper are unchanged if those responses are instead coded as support for the status quo). Figure B.1 shows the proportion of respondents in each region who support reform over time. The figure also shows the proportion of respondents in each region who offered an opinion about electoral reform over time. *Reform Opinion* is one if the respondent believes the electoral system should be changed or kept the same and zero if they answered don't know (it is missing if they offered no response, but this is the case for only 81 out of 27,272 respondents). As shown in the figure, very few respondents decline to offer an opinion about electoral reform, which suggests that this is a salient issue about which voters form opinions.[2]

Figure B.2 shows the mean support for reform by party identification. In this figure, I disaggregate supporters of "other" parties from the main analysis. The Liberal Democrat, Scottish National Party (SNP), and Green parties are by far the largest parties in "other" in the main analysis in terms of number of supporters among the BSA survey respondents. In Figure B.2, "other" includes supporters of the Alliance, the British National Party, the Liberal Party, Respect, the Social Democratic Party, and other (not specified in the BSA survey data).

[2] Figure B.1 excludes Northern Ireland because the electoral reform question was only included in the 1994 and 1996 NISA survey. However, the proportions of respondents in Northern Ireland who supported or offered an opinion about electoral reform in those years are similar to the proportions in other regions.

Figure B.1: Opinions About and Support for Reform by Region

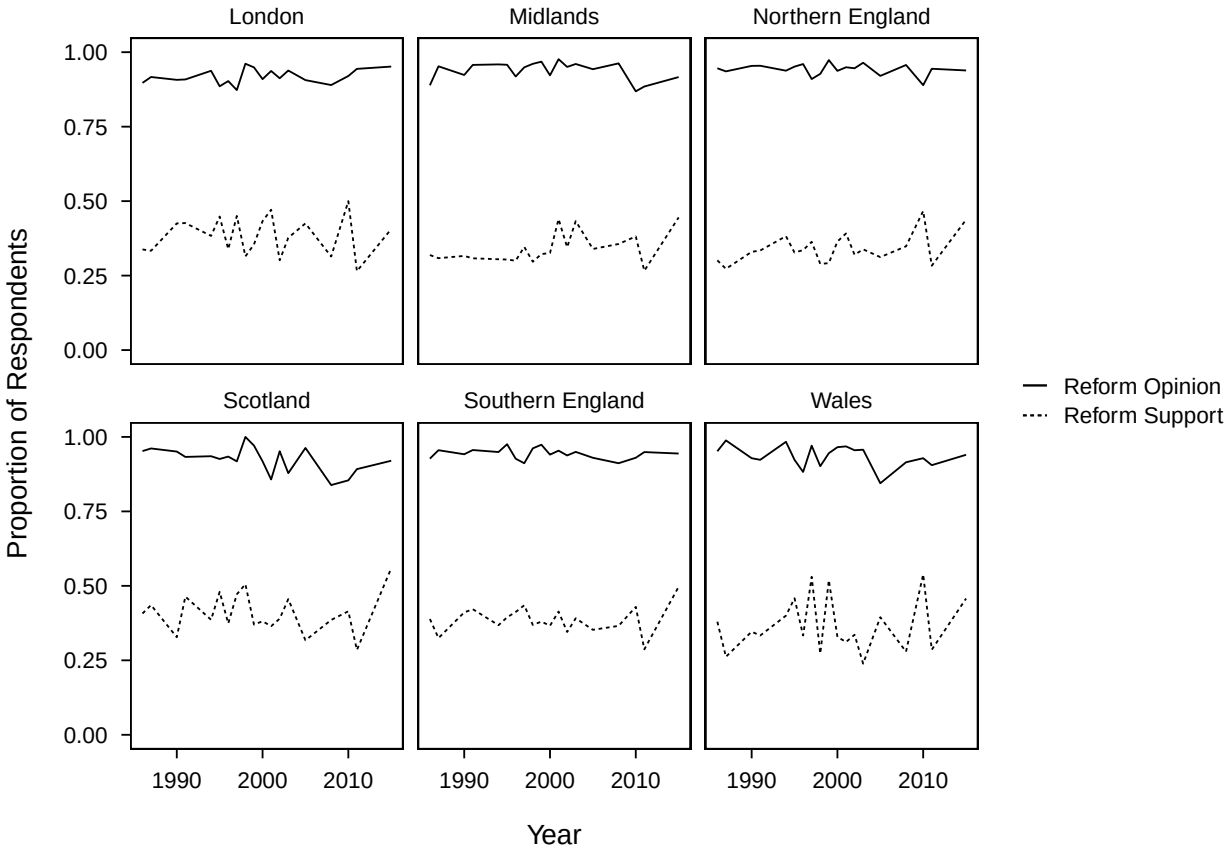
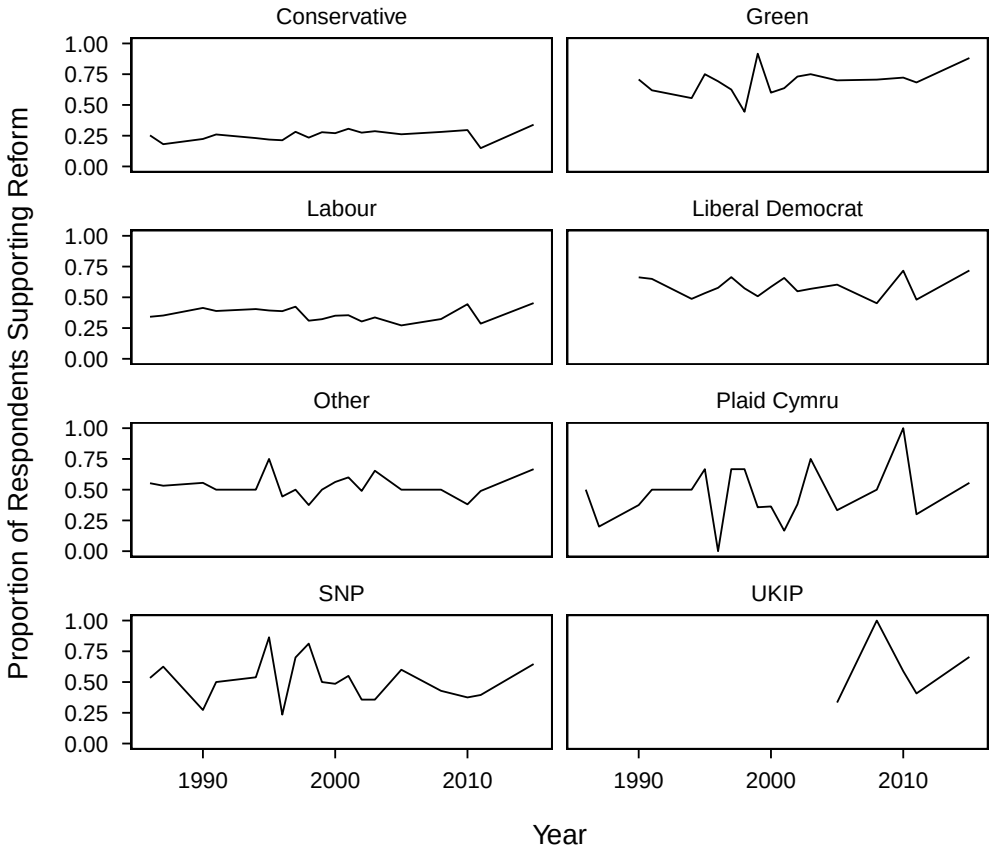


Figure B.2: Support for Reform by Party



B.2 Party Cues

Political parties have many methods by which they communicate with voters. However, one of the most clearly observable methods of communication is the election manifestos produced by parties that explain what they will do if elected. Therefore, I examine the General Election manifestos of the three main UK parties — Labour, the Conservatives, and the Liberal Democrats — between 1983 and 2015 (in other words, the manifestos that cover the entire time period of the survey data) to track the cues these parties were giving regarding the electoral system.

Table B.1 summarizes the commitments regarding the electoral system(s) made in the parties' election manifestos. The Liberal Democrats are relatively consistent because the single transferable vote (STV) is itself a proportional system. Thus, the 1997 Liberal Democrat manifesto is not actually a departure from their other manifestos, simply less specific about the type of proportional system they prefer. Moreover, the Liberal Democrats have always been explicit in their cues to supporters about the electoral system.

One can think of no explicit mention of the electoral system in a party's election manifesto as tacit support for the status quo. Conceptualizing no mention of the electoral system in this way, the Conservatives are consistent across the entire time period in their support for single-member district plurality (SMDP). However, although consistent, the Conservatives are not always explicit about the cues they are giving to supporters — they are less likely than the other two parties to explicitly reference the electoral system in their manifesto.

On the other hand, the Labour Party has been both inconsistent and, at times, not explicit, about the cues they give supporters regarding the electoral system. Labour has been by far the least consistent of all three parties — at the level of the House of Commons, they have alternated between tacit support for SMDP, supporting the creation of a working group or independent commission on the electoral system, support for proportional representation (PR), and support for an alternative vote (AV) referendum. In addition to being inconsistent, they have at times been publicly divided. While the Conservatives and Liberal Democrats

Table B.1: Electoral System Manifesto Commitments

Election	Labour	Conservative	Liberal Democrat
1983	No explicit mention	No explicit mention	NA*
1987	No explicit mention	No explicit mention	NA*
1992	HoC: Electoral system working group; Scot.: MMP	No explicit mention	HoC: STV; Local: STV; EP: STV
1997	HoC: Commission to recommend PR, referendum; Scot.: MMP; Wales: MMP; EP: PR	HoC: Keep SMDP	HoC: PR; Scot.: PR; Wales: PR; NI: PR; EP: PR
2001	HoC: Review report of Independent Commission	HoC: Keep SMDP	HoC: Commission recommendation of AV referendum, ultimately STV; Local: STV; EP: STV
2005	No explicit preference†	No explicit mention	HoC: STV; Local: STV; Scot.: STV; Wales: STV
2010	HoC: AV referendum	HoC: Keep SMDP	HoC: STV
2015	No explicit mention	HoC: Keep SMDP	HoC: STV; Local: STV

Note: HoC is the House of Commons. Scot. refers to the Scottish Parliament. Wales refers to the Welsh National Assembly. EP is the European Parliament. NI refers to the Northern Ireland Assembly. The table uses abbreviations to refer to the following electoral systems: the alternative vote (AV), mixed member proportional (MMP), proportional representation (PR), single-member district plurality (SMDP), and the single transferable vote (STV). * The Liberal Democrats were not formed until after the 1987 election. † Manifesto makes no specific comment on preferred electoral system but supports reviewing the systems in use and states that the HoC electoral system should only be changed via a referendum. Manifestos from Kimber (2015) and Pack (2017).

campaigned in the 2011 alternative vote referendum in a unified way that was also broadly consistent with their manifesto commitments (the Conservatives campaigned against the reform and the Liberal Democrats campaigned for AV while noting that they would prefer to switch to STV eventually), the Labour Party was split. Despite being the only party that supported AV in their 2010 election manifesto, the Labour leader, Ed Miliband, and others campaigned for AV while other prominent Labour members campaigned for SMDP (BBC 2011; White 2011; Whiteley et al. 2012). Thus, Labour has been less than explicit in their electoral system cues as well as being inconsistent over time and at times, divided.

B.3 Additional Results and Tables

In this section, I present several additional results discussed in the paper as well as result tables. The majority of these tables correspond to figures in the paper. However, the results for the models that make use of *Co-partisan MP* are also presented in this section and those results do not have a corresponding figure in the paper.

To begin, I present result tables that correspond to Figure 3.2 in the paper. For each of these tables, the first column presents the results of the models using the full time sample. In other words, the first column shows the results presented in Figure 3.2. The other columns show the results using smaller subsets based on year of the survey — results which are referenced in the paper, but not shown in a figure.

Table B.2 presents the coefficients and standard errors that correspond to the left panel of Figure 3.2, specifically, the results for supporters of the Labour Party. The table shows the effect of the introduction of MMP on the probability of support for electoral reform using the Scotland/Northern England sample. *Region* is an indicator for whether the respondent lives in the treated region (it is one if the respondent lives in Scotland and zero if they live in Northern England), while *Period* is an indicator that is one in the years after MMP has been introduced in the region (1999 and later) and zero prior to the introduction of MMP. The causal quantity of interest or the treatment effect is given by the interaction of the *Region* and *Period* variables. Table B.2 includes the results of models run on the full sample as well as subsets based on the year of the survey (e.g., one year on either side of the reform, two years on either side of the reform, etc.). Additionally, all results are from models with year fixed effects (except for the one-year sample) and controls for interest in politics.

Table B.3 presents the coefficients and standard errors that correspond to the left panel of Figure 3.2, but in this case, it shows the results for supporters of the Conservative Party. Again, the table shows the effect of the introduction of MMP on the probability of support for electoral reform using the Scotland/Northern England sample. The treatment effect is again given by the interaction of the *Region* and *Period* variables. As before, the results of

Table B.2: Effect of Experience with MMP on Support for Electoral Reform Among Labour Supporters, Scotland/Northern England Sample

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	0.088***	0.168*	0.026	0.059	0.100**	0.055
	(0.028)	(0.087)	(0.057)	(0.046)	(0.040)	(0.035)
Period	0.132***	0.034	−0.053	−0.051	−0.006	−0.040
	(0.043)	(0.056)	(0.042)	(0.051)	(0.042)	(0.051)
Region ∗ Period	−0.075**	−0.182	−0.018	−0.065	−0.110**	−0.035
	(0.036)	(0.117)	(0.075)	(0.062)	(0.052)	(0.047)
Constant	0.227***	0.299***	0.351***	0.344***	0.281***	0.334***
	(0.039)	(0.082)	(0.059)	(0.055)	(0.048)	(0.048)
Sample	Full	1 year	2 years	3 years	4 years	5 years
Region	Scotland	Scotland	Scotland	Scotland	Scotland	Scotland
Observations	3,885	339	940	1,314	1,895	2,257
R^2	0.037	0.087	0.036	0.032	0.034	0.032

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. Year fixed effects are excluded for the one-year sample. *p<0.1; **p<0.05; ***p<0.01

models run on the full sample as well as subsets based on the year of the survey are included and the models include year fixed effects (except for the one-year sample) and controls for interest in politics.

Similarly, Table B.4 presents the coefficients and standard errors corresponding to the left panel of Figure 3.2, however, it shows the results for supporters of parties other than Labour and the Conservatives, using the Scotland/Northern England sample. Once again, the treatment effect is given by the interaction of the *Region* and *Period* variables. The results of models run on the full sample as well as subsets based on the year of the survey are shown and year fixed effects (except for the one-year sample) and controls for interest in politics are included.

Table B.3: Effect of Experience with MMP on Support for Electoral Reform Among Conservative Supporters, Scotland/Northern England Sample

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	0.072*	0.258**	0.279***	0.135**	0.086	0.111**
	(0.037)	(0.124)	(0.082)	(0.068)	(0.061)	(0.055)
Period	0.087*	0.034	0.108*	0.147**	0.120*	0.033
	(0.052)	(0.090)	(0.062)	(0.073)	(0.071)	(0.075)
Region ∗ Period	−0.042	−0.151	−0.331***	−0.195**	−0.087	−0.084
	(0.051)	(0.169)	(0.108)	(0.092)	(0.081)	(0.074)
Constant	0.218***	0.095	0.223**	0.188*	0.267***	0.284***
	(0.060)	(0.165)	(0.098)	(0.096)	(0.088)	(0.084)
Sample	Full	1 year	2 years	3 years	4 years	5 years
Region	Scotland	Scotland	Scotland	Scotland	Scotland	Scotland
Observations	1,881	146	409	569	763	918
R^2	0.019	0.059	0.035	0.019	0.017	0.017

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. Year fixed effects are excluded for the one-year sample. *p<0.1; **p<0.05; ***p<0.01

Table B.4: Effect of Experience with MMP on Support for Electoral Reform Among Other Party Supporters, Scotland/Northern England Sample

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	−0.021	−0.0001	0.038	−0.012	0.019	0.004
	(0.043)	(0.167)	(0.101)	(0.082)	(0.068)	(0.061)
Period	0.162**	−0.298*	−0.152	0.230**	−0.165*	0.088
	(0.067)	(0.155)	(0.100)	(0.101)	(0.088)	(0.094)
Region ∗ Period	−0.015	0.174	0.035	0.011	−0.009	−0.021
	(0.054)	(0.212)	(0.124)	(0.102)	(0.085)	(0.077)
Constant	0.438***	0.604**	0.579***	0.372***	0.592***	0.426***
	(0.067)	(0.261)	(0.129)	(0.116)	(0.105)	(0.094)
Sample	Full	1 year	2 years	3 years	4 years	5 years
Region	Scotland	Scotland	Scotland	Scotland	Scotland	Scotland
Observations	1,426	93	290	409	590	710
R^2	0.067	0.124	0.059	0.057	0.056	0.054

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. Year fixed effects are excluded for the one-year sample. *p<0.1; **p<0.05; ***p<0.01

Table B.5 shows the results corresponding to the right panel of Figure 3.2 — the effect of MMP on the probability of support for electoral reform among Labour Party supporters using the Wales/Midlands sample. Here, the treated region is Wales, but otherwise the results are analogous to those presented above. Again, the causal quantity of interest is the coefficient on the interaction term and the models include year fixed effects and controls for interest in politics.

Table B.6 shows the results corresponding to the right panel of Figure 3.2 for supporters of the Conservative Party. It shows the effect of MMP on the probability of support for electoral reform among Conservative supporters using the Wales/Midlands sample. Once again, the treatment effect is the coefficient on the interaction term and the models include year fixed effects and controls for interest in politics.

Table B.7 shows the results corresponding to the right panel of Figure 3.2 for supporters of parties other than Labour or the Conservatives. The table shows the effect of MMP on the probability of support for electoral reform using the Wales/Midlands sample. Again, the treatment effect is the coefficient on the interaction term and the models include year fixed effects and controls for interest in politics.

In the paper, I discuss diagnostics from models including treatment leads and lags. There, I focus on the diagnostics for the full sample models, but here I provide additional details regarding the diagnostics for the smaller time samples as well. The diagnostics present somewhat mixed results. For the models comparing Scotland and Northern England, most treatment leads are insignificant, but we must sometimes reject the null hypothesis that the coefficients on the treatment leads are jointly equal to zero. For the models run on the Labour Party subset, as expected, the longer time samples are more likely to show evidence of violations of the parallel trends assumption. In the four-year, five-year, and full samples, F-tests lead us to reject the null hypothesis that the leading treatment coefficients are jointly equal to zero. However, we cannot reject the null hypothesis for the three-year sample and in the two-year sample, the treatment lead is significant only at the 90% confidence level.

Table B.5: Effect of Experience with MMP on Support for Electoral Reform Among Labour Supporters, Wales/Midlands Sample

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	0.088***	0.041	0.124*	0.063	0.085*	0.091**
	(0.033)	(0.094)	(0.071)	(0.055)	(0.048)	(0.043)
Period	0.128**	0.065	−0.137**	0.057	−0.050	−0.009
	(0.061)	(0.077)	(0.064)	(0.068)	(0.057)	(0.066)
Region ∗ Period	−0.110**	0.057	−0.084	−0.070	−0.105	−0.133**
	(0.045)	(0.146)	(0.095)	(0.077)	(0.064)	(0.059)
Constant	0.232***	0.199*	0.318***	0.173**	0.295***	0.274***
	(0.056)	(0.115)	(0.086)	(0.071)	(0.068)	(0.066)
Sample	Full	1 year	2 years	3 years	4 years	5 years
Region	Wales	Wales	Wales	Wales	Wales	Wales
Observations	2,045	187	465	680	1,013	1,225
R^2	0.039	0.039	0.059	0.047	0.045	0.044

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. Year fixed effects are excluded for the one-year sample. *p<0.1; **p<0.05; ***p<0.01

Similarly, for the Conservative Party subsets, F-tests suggest we reject the null hypothesis that the treatment leads are jointly equal to zero for the three-year, four-year, five-year, and full samples. However, in the two-year sample, the treatment lead is not significant at any traditional confidence level. Reassuringly, for the models with the other parties sample, all treatment leads are insignificant except for the 1990 lead in the full sample (significant at the 90% confidence level). Moreover, for all models with the other parties sample, we cannot reject the null hypothesis that the treatment leads are jointly equal to zero.

More promisingly, for the models comparing Wales and the Midlands, all treatment leads are individually insignificant except in the Labour Party subset run with the three-year sample, the 1997 lead is significant at the 90% confidence level. However, in that model and all others, F-tests indicate that we cannot reject the null hypothesis that the leading treatment coefficients are jointly equal to zero at any conventional confidence level. In other

Table B.6: Effect of Experience with MMP on Support for Electoral Reform Among Conservative Supporters, Wales/Midlands Sample

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	−0.043	−0.199	−0.049	−0.045	−0.029	−0.023
	(0.048)	(0.154)	(0.100)	(0.088)	(0.071)	(0.065)
Period	0.101*	0.038	−0.015	0.075	0.154**	0.171**
	(0.054)	(0.089)	(0.061)	(0.080)	(0.062)	(0.070)
Region ∗ Period	0.057	0.398	0.111	0.070	0.025	−0.009
	(0.062)	(0.270)	(0.126)	(0.111)	(0.091)	(0.084)
Constant	0.192***	−0.038	0.202	0.060	−0.015	−0.020
	(0.071)	(0.426)	(0.143)	(0.116)	(0.095)	(0.093)
Sample	Full	1 year	2 years	3 years	4 years	5 years
Region	Wales	Wales	Wales	Wales	Wales	Wales
Observations	1,643	108	357	479	688	837
R^2	0.026	0.098	0.011	0.010	0.019	0.025

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. Year fixed effects are excluded for the one-year sample. *p<0.1; **p<0.05; ***p<0.01

words, these results present some concerns about violations of the parallel trends assumption in some of the models using the Scotland/Northern England data (although, as expected, the assumption seems to hold when using shorter time samples), but provide confidence that the assumption holds with the Wales/Midlands samples.[3]

[3] See the replication code for full results.

Table B.7: Effect of Experience with MMP on Support for Electoral Reform Among Other Party Supporters, Wales/Midlands Sample

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	0.058	0.340	0.153	0.161	0.155	0.072
	(0.069)	(0.311)	(0.149)	(0.124)	(0.104)	(0.088)
Period	0.161*	0.285	0.054	0.115	0.071	0.089
	(0.093)	(0.179)	(0.130)	(0.126)	(0.108)	(0.115)
Region ∗ Period	−0.129	−0.415	−0.186	−0.213	−0.193	−0.144
	(0.083)	(0.358)	(0.182)	(0.152)	(0.126)	(0.108)
Constant	0.374***	0.400*	0.282*	0.401***	0.351***	0.369***
	(0.093)	(0.210)	(0.163)	(0.142)	(0.130)	(0.127)
Sample	Full	1 year	2 years	3 years	4 years	5 years
Region	Wales	Wales	Wales	Wales	Wales	Wales
Observations	785	57	151	220	337	419
R^2	0.073	0.186	0.126	0.106	0.083	0.083

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. Year fixed effects are excluded for the one-year sample. *p<0.1; **p<0.05; ***p<0.01

Table B.8 shows the results of the difference-in-differences (DID) analyses using the full time sample when *Co-partisan MP* is included as a control. This table does not have a corresponding figure in the paper. As discussed in the paper, due to data limitations, whether an individual has a co-partisan MP can only be coded for a subset of respondents, which is why the sample sizes are significantly smaller than the sample sizes for the full samples in the main models. The models show the treatment effect (given by the interaction term) of experience with MMP and include year fixed effects and controls for interest in politics and having a co-partisan MP.

Table B.9 shows the results of the DID analyses when subsetting on the basis of whether or not the individual has a co-partisan MP instead of based on party identification. Again, this table does not have a corresponding figure in the paper. The models show the treatment

Table B.8: Effect of Experience with MMP on Support for Electoral Reform with Co-partisan MP Control

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	0.145***	0.067	−0.049	0.074	−0.082	0.101
	(0.040)	(0.048)	(0.059)	(0.047)	(0.065)	(0.104)
Period	0.048	0.064	0.040	0.034	0.067	−0.002
	(0.049)	(0.067)	(0.084)	(0.068)	(0.067)	(0.103)
Region ∗ Period	−0.117**	−0.055	0.007	−0.128**	0.040	−0.126
	(0.051)	(0.073)	(0.078)	(0.062)	(0.085)	(0.122)
Constant	0.278***	0.231***	0.438***	0.231***	0.152*	0.144
	(0.048)	(0.077)	(0.081)	(0.069)	(0.090)	(0.118)
Region	Scotland	Scotland	Scotland	Wales	Wales	Wales
Party Subset	Labour	Conservative	Others	Labour	Conservative	Others
Observations	1,930	925	680	1,040	779	356
R^2	0.037	0.016	0.054	0.029	0.020	0.159

Note: Coefficients and standard errors from linear probability models. All models include year fixed effects (excluding one year due to collinearity), controls for interest in politics, and a control for having a co-partisan MP. *p<0.1; **p<0.05; ***p<0.01

effect of experience with MMP (given by the interaction term) and include year fixed effects and controls for interest in politics. If a control for respondent party identification is included, the results are largely the same, with the exception that the treatment effects in the Scotland samples are significant at the 90% confidence level (results not shown, but available in the replication code).

Table B.9: Effect of Experience with MMP on Support for Electoral Reform Subset by Co-partisan MP

	Dependent variable:			
	Reform Support			
	(1)	(2)	(3)	(4)
Region	0.126***	0.073**	0.065	0.010
	(0.040)	(0.033)	(0.043)	(0.051)
Period	0.077	0.034	0.124**	0.005
	(0.048)	(0.046)	(0.060)	(0.056)
Region ∗ Period	−0.075	−0.050	−0.114*	−0.053
	(0.051)	(0.046)	(0.059)	(0.064)
Constant	0.258***	0.254***	0.174***	0.308***
	(0.045)	(0.039)	(0.065)	(0.051)
Region	Scotland	Scotland	Wales	Wales
Subset	Co-partisan MP	No Co-partisan MP	Co-partisan MP	No Co-partisan MP
Observations	1,916	2,207	1,174	1,384
R^2	0.027	0.018	0.023	0.032

Note: Coefficients and standard errors from linear probability models. All models include year fixed effects (excluding one year due to collinearity) and controls for interest in politics. *p<0.1; **p<0.05; ***p<0.01

For the first placebo test discussed in the paper, I use the same DID design as in the main analyses, but the dependent variable is now support for abolishing the monarchy — something that should be unaffected by the treatment. Therefore, I compare the difference in the probability an individual supports abolishing the monarchy among respondents living in Scotland/Wales before and after 1999 when MMP was introduced to the difference among respondents living in Northern England/the Midlands before and after 1999.

For this test, I use a question on the BSA survey that asked respondents,

> "How important or unimportant do you think it is for Britain to continue to have a monarchy... very important, quite important, not very important, not at all important, or, do you think the monarchy should be abolished?" (NatCen Social Research 2019)

While it is not in the same format as the electoral reform question (as would have been ideal), it is a question about a similar constitutional issue where respondents are given the choice to

indicate support for the status quo or support for reform.[4] Although this question was asked on some surveys in which the electoral reform question was *not* asked, I limit the analysis to those surveys where the electoral reform question was also asked to keep the sample as consistent as possible across analyses.[5] Using this question, I construct the dependent variable for this analysis, *Abolish Monarchy*, which takes a value of one if the respondent indicated they support abolishing the monarchy and zero if the respondent answered by rating the importance of the monarchy.[6] To be consistent with the main analysis, I run the models on subsets according to party identification and include controls for political interest as well as year fixed effects (although the results are unchanged if the controls for interest in politics are excluded).

Experience with MMP should have no effect on support for the monarchy so we should expect the "treatment effect" to be insignificant. Table B.10 presents the full results corresponding to left panel of Figure 3.3 in the paper (with the "treatment effect" given by the interaction of the *Region* and *Period* variables). Models 1 through 3 show the results using the Scotland/Northern England comparison while columns 4 through 6 use the Wales/Midlands comparison. As expected, the "treatment effect" is always insignificant. The results of this test increase confidence that the treatment effects in the main DID analyses are capturing the effect of electoral system experience as opposed to some other factor correlated with region or time.

Finally, Table B.11 shows the results of the final placebo test described in the paper (corresponding to the right panel of Figure 3.3). For this test, observations from the real treatment period (1999 and later) have been excluded. A placebo "treatment" occurring prior to the 1995 survey wave in both Scotland and Wales has been assigned. The "treatment

[4] The BSA survey has very few questions on constitutional issues. Ideally, I would also run a placebo test using support for reforming the House of Lords, but the relevant question was changed significantly over time, precluding its use.

[5] However, there are years in which the monarchy question was not asked even though the electoral reform question was asked so the sample is not identical.

[6] As with *Reform Support*, don't know and no answer responses were both coded as missing for this analysis, but the results are robust to coding those responses as support for keeping the monarchy.

Table B.10: Placebo Test: Effect of Experience with MMP on Support for Abolishing the Monarchy

	Dependent variable:					
	Abolish Monarchy					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	0.047*	−0.005	0.105**	0.024	0.008	0.074
	(0.028)	(0.022)	(0.049)	(0.029)	(0.025)	(0.058)
Period	−0.124***	−0.013	0.045	0.013	−0.014	−0.096
	(0.040)	(0.026)	(0.064)	(0.044)	(0.024)	(0.070)
Region ∗ Period	0.022	−0.014	−0.039	0.038	−0.024	0.024
	(0.035)	(0.027)	(0.056)	(0.038)	(0.031)	(0.068)
Constant	0.206***	0.101***	0.029	0.149***	0.003	0.069
	(0.034)	(0.031)	(0.066)	(0.041)	(0.033)	(0.068)
Region	Scotland	Scotland	Scotland	Wales	Wales	Wales
Party Subset	Labour	Conservative	Others	Labour	Conservative	Others
Observations	2,291	1,008	802	1,232	927	467
R^2	0.027	0.030	0.040	0.026	0.012	0.075

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. *p<0.1; **p<0.05; ***p<0.01

effect" is again given by the interaction of the *Region* and *Period* variables and, as expected, is largely insignificant. Columns 1 through 3 show the results using the Scotland/Northern England subset while Columns 4 through 6 display the results using the data from Wales/the Midlands. Again, the models are run on subsets based on the party identification of the respondent (the other parties subset excludes those who do not support a party) and each model includes year fixed effects and controls for interest in politics (but the results are unchanged if the interest in politics controls are excluded).

Table B.11: Placebo Test: Support for Electoral Reform with Placebo "Treatment" Period

	Dependent variable:					
	Reform Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Region	0.084**	0.063	−0.042	0.086*	−0.044	−0.046
	(0.039)	(0.046)	(0.056)	(0.046)	(0.060)	(0.094)
Period	0.003	0.016	0.098	−0.117*	−0.057	−0.242*
	(0.049)	(0.064)	(0.102)	(0.068)	(0.067)	(0.130)
Region ∗ Period	0.017	0.028	0.052	−0.0003	0.006	0.260*
	(0.056)	(0.074)	(0.087)	(0.067)	(0.091)	(0.142)
Constant	0.158***	0.282***	0.387***	0.267***	0.176*	0.278**
	(0.047)	(0.076)	(0.087)	(0.068)	(0.097)	(0.117)
Region	Scotland	Scotland	Scotland	Wales	Wales	Wales
Party Subset	Labour	Conservative	Others	Labour	Conservative	Others
Observations	1,682	831	517	906	680	261
R^2	0.047	0.014	0.108	0.057	0.036	0.109

Note: Coefficients and standard errors from linear probability models that include both year fixed effects (excluding one year due to collinearity) and controls for interest in politics. Observations from real treatment period have been dropped and placebo "treatment" occurs prior to the 1995 survey wave. *p<0.1; **p<0.05; ***p<0.01

Appendix C

Appendix to "Legislating Themselves Out of Office: Electoral Reform and Parties as Non-Unitary Actors"

C.1 Data and Descriptive Statistics

Here, I present additional details about the data used in the paper. As discussed in the paper, the data primarily comes from Eggers and Spirling (2014a) and Rush (2001), although I supplemented this with secondary sources where necessary.[1] To construct the dataset used for the analysis, I first obtained the votes on the Third Reading of the Redistribution of Seats Bill from the divisions data (which also included the party affiliation of each MP at the beginning and end of the parliament) by Eggers and Spirling (2014a) and combined this with their data on MPs (which includes name and dates of birth and death). I dropped those who were listed as having died prior to the vote. I then merged in information (from Eggers and Spirling 2014a) on dates of service in parliament so that I could drop those who were not serving as an MP at the time of the of the Third Reading (e.g., due to early resignation). I

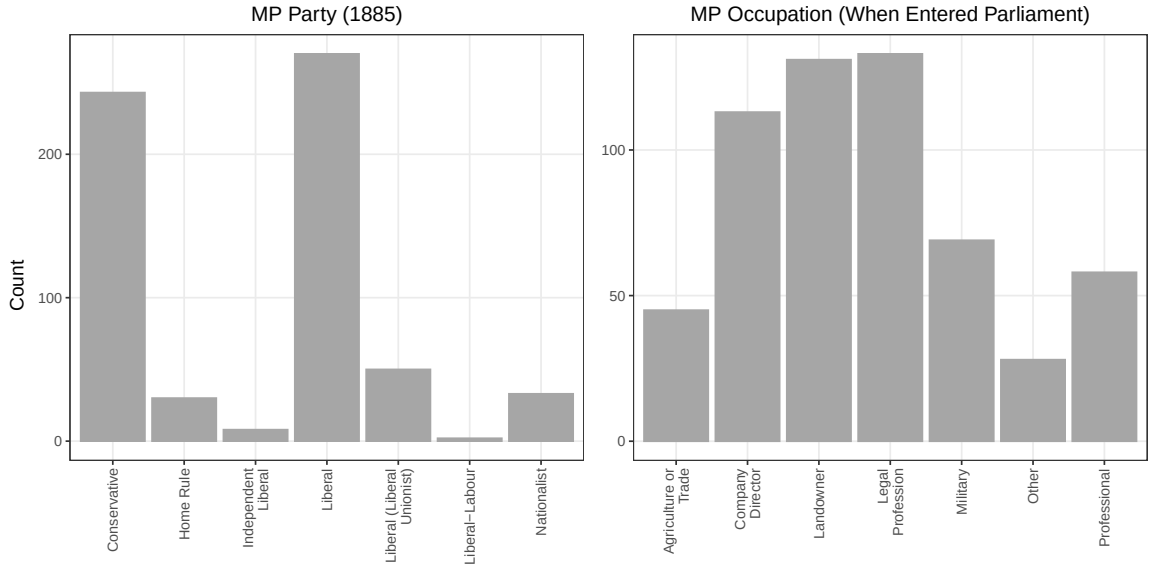
[1] I am grateful to Andy Eggers and Arthur Spirling for making their data available online and to Michael Rush for providing his data (and Emma Peplow at the History of Parliament Project for connecting us).

then combined this with the data from Eggers and Spirling (2014a) on parliamentary offices (including cabinet membership) and information on whether the MP ran in the 1885 election and their election fate if they ran (which I extracted from their elections data).

In order to prepare the Rush (2001) data, I combined his data on MPs (which includes name, dates of birth and death, information on aristocratic connections, whether the individual was ever knighted or awarded a baronetcy and the year of the award, occupation, and educational background, among other details). I combined this data with data on peerages from Rush (2001), which I coded to identify new peerages that had been awarded in the years following 1885 (the results in the paper use the coding of a new peerage in 1886). I matched the MPs identified as sitting at the time of the Third Reading using the Eggers and Spirling (2014a) data with the MP background, knighthood, baronetcy, and peerage data from Rush (2001) by hand, using names, birthdates, and sometimes dates of death to ensure I was matching the correct individuals. Finally, the dataset also includes information on appointments to diplomatic positions which I coded by hand, using data from Mackie (2017), but due to the limited number of diplomatic appointments held by MPs in the data, I was unable to use this in the analysis. The final dataset includes 636 MPs (lower than the total number of MPs at the time due to vacancies at the time of the Third Reading).

Figure C.1 shows the distribution of MP party membership and occupation in the data. The left panel shows the number of MPs associated with each party in 1885 according to the Eggers and Spirling (2014a) data. This variable is used to identify Liberal and Conservative Party members. Unfortunately for my purposes, this variable is coded based on the party affiliation of MPs at the time of the 1885 election (i.e., slightly after the Third Reading vote on the Redistribution of Seats Bill). While this coding is technically post-treatment, I use the 1885 party coding because it is likely that the party/faction affiliation would have been known (by both the MP and the party leadership) well in advance of the election (the vote occurred roughly six months before the election) and therefore this variable is likely to capture the true party affiliation of MPs just prior to the vote on the Third Reading.

Figure C.1: Distribution of Party and Occupation Variables



Note: The occupation plot omits (59) missing values.

Nevertheless, all the results are robust to using the unambiguously pre-treatment coding of party affiliation at the time of the 1880 election (also from Eggers and Spirling 2014a).[2] As shown in the left panel of Figure C.1, by far the two largest parties are the Liberals (270 MPs) and the Conservatives (243 MPs).[3]

The right panel of Figure C.1 presents the distribution of occupations at the time the MPs initially entered parliament. This variable is used as a control in the analyses. While the original occupations were coded by Rush (2001), I am responsible for the categorization of the occupations into the categories shown in the figure. There are 59 MPs for whom the occupation information is missing. As shown, although there was more variation in the background of MPs by this time compared to earlier in the century, there were still relatively few occupations represented in parliament.

While I use Figure C.1 to present information about the categorical variables used in the

[2] In fact, the results of the regressions of *Office Holding* on *Support* with no controls using both the full sample of Liberals and only Liberals who won in 1885 actually improve if the 1880 coding of party is used.
[3] For the purposes of the figure, I have combined several of the smaller party classifications — two variations of Home Rulers as well as four variations of Independent Liberals that were originally separated by Eggers and Spirling (2014a).

analyses, Table C.1 shows descriptive statistics for the other variables used in the paper. The top panel summarizes the dependent variables used in the analyses, all of which take a value of one if the MP received a new appointment to one of those positions in 1886 (the year following the reform). The data for *New Peerage* and *New Baronetcy or Knighthood*, come from Rush (2001), but I used secondary sources to code the appointment years that were missing for some baronetcies and knighthoods. Similarly, while I use office holding data from Eggers and Spirling (2014a) to identify which individuals received an appointment to a parliamentary office or cabinet position in 1886, I made some minor corrections (using secondary sources) to their original data. In addition, I deviate from the original data so that I do not count either positions granted according to seniority (Father of the House) or positions which one must be a member of the House of Lords to hold so that I am more confident that these variables capture discretionary appointments to positions in the House of Commons.

The middle panel in Table C.1 summarizes the independent variable of interest in all the analyses, *Support*. Again, while this variable originally came from Eggers and Spirling (2014a), I made a few minor corrections (to ensure that those who were listed elsewhere as not sitting at the time of the Third Reading, were not counted as having voted). As noted in the paper, this variable is coded as one if the MP voted in support of the Redistribution of Seats Bill at the Third Reading and a zero if they voted against or abstained.

Finally, the bottom panel in Table C.1 presents the summary statistics for the control variables used in the analyses (with the exception of occupation, shown above). For the analyses using the Liberal Party subset, I use *Cabinet Member (At 3rd Reading)*, while I use *Cabinet (Before 3rd Reading)* for the analyses using the Conservative Party since Eggers and Spirling (2014a) do not have information on the shadow cabinet. The data for *University Graduate or Higher* originally comes from Rush (2001), but I collapsed his coding to this dichotomous variable. *Aristocratic Connection* is coded as one if the MP is the son (not necessarily the eldest) of a peer or baronet and zero otherwise (Rush 2001). Finally, I

Table C.1: Descriptive Statistics

Variable	Observations	Mean	Std. Deviation	Minimum	Maximum
New Peerage	636	0.013	0.112	0	1
New Baronetcy or Knighthood	636	0.020	0.142	0	1
Cabinet Member	636	0.038	0.191	0	1
Office Holder	631	0.113	0.316	0	1
Support	636	0.184	0.388	0	1
Cabinet Member (At 3rd Reading)	636	0.013	0.112	0	1
Cabinet (Before 3rd Reading)	636	0.031	0.175	0	1
University Graduate or Higher	536	0.610	0.488	0	1
Aristocratic Connection	636	0.286	0.452	0	1
Age	630	53.39	12.232	19	85

Note: Summary statistics for the dependent variables (top panel; all consider new appointments in 1886), independent variable of interest (middle panel), and control variables (bottom panel) used in the analyses.

calculated the *Age* of the MP using the date of birth recorded in Eggers and Spirling (2014a). Since the month and day of birth is often estimated in the original data, I elected not to attempt to code the age of the MPs at the time of the Third Reading. Instead, to maximize coverage, *Age* is coded as the MP's age at the end of 1885 by subtracting their birth year from 1885.

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