Do Primaries Work?

Bayesian Causal Models of Partisan Ideology and Congressional Nominations

By

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> I became a worse political scientist
> so I could be a better person.

Abstract

In contemporary electoral politics in the U.S., primary elections are widely believed to play a crucial role. Many scholars believe that primary election competition is the standout reason why classic predictions from formal models of electoral competition—that candidates take ideological positions near the median voter—fail to manifest in the real world. The general election context provides incentives for candidates to take centrist policy positions, but candidates must win their party's nomination before advancing to the general election. Because primary elections take place predominantly among voters of one political party affiliation, and because those voters tend to hold strongly partisan beliefs about political issues, candidates feel more acute incentives to take strong partisan stances on issues rather than moderate stances even amid stiff general election competition.

This story of primary elections and representation is widely believed, but is it true? Despite its prominence, the empirical evidence is unclear. The theory rests on a notion that voters make informed choices in primary elections by consulting their policy preferences and choosing the candidate with the closest policy platform. Past research has been unable to operationalize key constructs in this prediction, or it has operationalized the wrong constructs. Candidates should take more extreme positions when the primary constituency has a stronger preference for ideologically extreme policy, but studies have not directly measured the policy preferences of partisans within a candidate's district. Further, districts where partisans hold more extreme preferences should nominate candidates with more extreme campaign positions as well, but methods for estimating candidates' ideological positions have been incompletely applied to the study of primaries. Moreover, because primary elections are characterized by low levels of voter information and the partisanship of candidates is held largely constant, non-policy forces such as candidate valence and campaign spending may be more powerful than in general elections. For these reasons, the proposition that primary elections advance the ideological interest of local partisan voters is theoretically contestable.

This dissertation develops and applies new Bayesian approaches for estimating both constructs that have yet eluded the study of primary politics: the preferences of partisan voters as a group and the campaign positioning of primary candidates. With these estimates in hand, I explore the relationship between local partisan preferences and primary candidate positions. Do primary candidates position themselves relative to partisan primary voters, and is the relative extremism of partisan constituencies related to the ideological positions of the candidates they nominate?

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Introduction: Policy Ideology and Congressional Primaries

Elections are the foremost venue for citizens to influence government actors and public policy. Classic theories of voting suggest that citizens weigh the policy positions of alternative candidates and vote for the candidate whose platform most closely aligns with their own preferences (Downs 1957). Political parties simplify the voter's calculations by providing a powerful heuristic in the form of the party label, enabling voters to infer candidates' values and issue positions without expending the effort to thoroughly appraise each campaign (Campbell et al. 1960; Green, Palmquist, and Schickler 2002; Rahn 1993).

The rise of partisan polarization, however, has complicated the role of parties in U.S. politics. Although citizens, journalists, pundits, and even elected leaders frequently bemoan the bitter rhetoric and legislative gridlock that has accompanied the widening partisan divide, political scientists have noted several positive consequences to polarization. Compared to the parties of the early- and mid-1900s that political scientists believed were too similar to provide voters with meaningful choices (American Political Science Association 1950), the Democratic and Republican Parties of recent decades have taken divergent and oppositional stances across a greater number of policy issues. As a result, voters can more easily differentiate the policy platforms of the two parties in order to vote consistently with their political values. Voters in turn became more thoroughly sorted into partisan groups that represent distinct ideological viewpoints in American politics, holds beliefs across multiple issues that are more ideologically consistent, think more abstractly about the ideological underpinnings of issue stances, and participate more in politics than they did in the past (Abramowitz and Saunders 1998; Fiorina, Abrams, and Pope 2005a; Layman and Carsey 2002; Levendusky 2009).

Even as polarization has strengthened many aspects of political representation between the two parties, it may have troubling effects on representation within the two major parties. The typical voter is a partisan who intends to cast her ballot for her preferred party, whoever that candidate may be (Bartels 2000; Petrocik 2009). As party-line voting increases, voters are more thoroughly captured by their loyalties. A partisan voter's choices are locked in long before Election Day. Candidates from her preferred party have already been selected through a nomination process, and she may be more likely to abstain from voting when faced with an undesirable candidate than she is to vote for a different party (Hall and Thompson 2018). Recent research supports this notion of capture amid polarization—when voters must choose between polarized candidates, they become less responsive to candidates' actual platforms and instead are more influenced by motivated reasoning and partisan teamsmanship (Rogowski 2016). Voters relax their substantive scrutiny of candidates to cast low-cost votes for their own party, weakening the influence of *policy* as a separate consideration from partisanship.

This presents an important problem for our understanding of how elections contribute to the representation of voter preferences in government. Elections are intended to be a voter's choice over alternative political values to be expressed in government, but if the choice of candidates does not present the average partisan voter with realistic alternatives, how should we think about the "representation" of these voters' actual policy preferences? If general elections provide an ever-coarsening choice over policy priorities, does the U.S. electoral system incorporate voters policy preferences in other ways?

When the choice before voters in the general election does not present realistic alternatives, political scientists naturally shift their focus to the nomination of partisan candidates. V.O. Key, for example, studied Democratic Party dominance in the American South, asking if competition within the party could provide a quality of representation similar to two-party competition (Key 1949). Although scholars are right to examine within-party competition, focusing on contexts of single-party dominance is a serious limitation. Even in races between viable candidates from both major parties, within-party competition plays a crucial role simply due to the fact that partisan voters almost certainly cast a vote for their own party. Rank-and-file partisan constituents are all but captured. If they are to express their policy preferences through the act of voting, their voices may register as relatively weak because they present little electoral risk to their party in the general election. The nomination stage—the primary election in particular—remains an important venue for the representation of partisans' policy views, whether the general election is closely contested or not.

1.1 Policy Preferences and the Strategic Positioning Dilemma

This dissertation is chiefly concerned with the policy preferences of partisan voters and their role in electoral representation through Congressional primary elections. The study of American electoral politics has not ignored the representational function of primary elections (Aldrich 2011; Cohen et al. 2009; Geer 1988; Norrander 1989; Sides et al. 2018), but as I discuss below, the quantifiable impact of primary voters' policy preferences in government is a startlingly open question. Several existing studies have examined other aspects of representation through House primaries, such as the introduction of the direct primary (Ansolabehere et al. 2010), how candidates position themselves in response to the presence or threat of primary challenges (Brady, Han, and Pope 2007; Burden 2004; Hirano et al. 2010), and how primary nomination rules affect elite polarization (Hirano et al. 2010; McGhee et al. 2014; Rogowski and Langella 2015). Though these studies address interesting aspects of electoral representation and party competition, they cannot speak directly to the influence of voter's policy preferences on (1) the positioning of House primary candidates and (2) the outcomes of House primary elections.

The absence of voter preferences from the empirical study of primaries is troubling because they play a crucial role in the dominant theory that relates representation to primary politics. Although the Downsian model of candidate positioning explains the incentives for candidates to stake out moderate policy positions to cater to the ideological "median voter" (Downs 1957), candidates behave differently in the real world. Instead, candidates engage in highly partisan behavior and take divergent issue stances even on salient local issues and in closely competitive districts (Ansolabehere, Snyder, and Stewart 2001; Fowler and Hall 2016). But why? Scholars and political observers have argued that because competing in the general election requires each candidate to clinch their party's nomination contest, these candidates face a combination of convergence-promoting and divergence-promoting incentives. Primary elections tend to be dominated by partisan voters who are more attentive to politics, hold more non-centrist issue preferences, and "weight" candidates' issue positions more heavily than the average voter in the general election.[1] As a result, the risk that a candidate is defeated in the primary for being too moderate may outweigh the risk of losing the general election for being too partisan. The conflicting incentives imposed by partisan constituency and the general election constituency creates a "strategic-positioning dilemma" that leads candidates to take divergent issue stances rather than targeting a district median voter (Aldrich 1983; Brady, Han, and Pope 2007; Burden 2001; Hill 2015).

The strategic positioning dilemma (SPD) is a central theoretical feature of this project,

[1] Primary elections are not *entirely* partisan affairs. States vary in their regulations that primaries be "closed" to partisan voters only, that voters must preregister with their preferred party to vote in the primary, and even whether primaries are partisan at all (see McGhee et al. 2014 for a thorough and contemporary review of these regulations). Although many observers suspect that regulations on primary openness greatly influence the ideological extremity of the primary electorate, recent survey research finds that these regulations do little to affect the policy preferences of primary voters on average (Hill 2015).

and tests of the SPD are key empirical contributions in the following chapters. The sections that follow introduce key terms for understanding my critique of the existing research and my contribution to it in this project.

1.1.1 Key concept: policy ideology

If we had an ideal test of the SPD's implications, the policy preferences of partisan primary voters are an essential ingredient. Primary voters are one of the key constituencies that a candidate must please in the SPD view of primary elections. When partisan voters in a district are more conservative, the SPD claims that the candidate experiences a pressure to stake out a more conservative campaign position, especially in the primary. This section briefly discusses this project's terminology around voter ideology, the groups in the electorate for whom these concepts are at play, and how relate to other political science research.

When this project discusses voter "preferences" or voter "ideology," it specifically refers to a notion of *policy ideology*. An individual's policy ideology is a summary of their policy views in a left–right ideological space. Policy views are naturally complex and multidimensional, and it is possible for individuals to hold beliefs across policy areas that would strike many political scientists as being "ideologically inconsistent" (e.g. Campbell et al. 1960). Policy ideology distills this complexity into average tendencies; voters who hold a greater number of progressive preferences about policy are more ideologically progressive, and vice versa for voters with more conservative policy preferences. Voters who hold a mixture of progressive and conservative beliefs are ideologically moderate.

Policy ideology is different from policy *mood*, since mood measures voter preferences for the government to do more or less than an ever-shifting baseline, while ideology meant to be directly comparable using only issue information (Enns and Koch 2013; McGann 2014; Stimson 1991). Policy ideology is thus a similar concept to any method that measures a hidden ideological summary from one-off issue-based stimuli. This includes ideal point scores for members of Congress, Supreme Court justices, and even individual citizens (Clinton, Jackman, and Rivers 2004; Martin and Quinn 2002; Poole and Rosenthal 1997; Tausanovitch and Warshaw 2013; Treier and Hillygus 2009). Other researchers have called this concept "policy liberalism" (Caughey and Warshaw 2015), which orients the concept so that "larger" values represent "more liberalism." For this project, I prefer to orient the construct as policy *conservatism*, which orients a scale so that larger/more conservative values correspond to "rightward" movements on a number line. I try to be conscious of the difference between *consistent* issue beliefs and *extreme* issue beliefs throughout this project. Consistently conservative issue beliefs do not necessarily imply that an actor is "extremely" conservative (Fiorina, Abrams, and Pope 2005b), and an actor may appear "moderate" even if they hold a mixture of non-moderate progressive and conservative issue beliefs (Broockman 2016).

This project views policy ideology in a measurement modeling context, which we return to in Chapter 2. Policy ideology affects voters' issue beliefs, and while issue beliefs can be measured using a survey, policy ideology itself is not observable. Instead, policy ideology exists in a latent space, an survey items on specific issues reveal only limited information about voters' locations in the latent space. This is different from summarizing policy views by adding or averaging policy responses, which implicitly assumes that all items about all issues are equally informative about ideology. Modern measurement approaches relax this assumption, instead viewing survey items as sources of correlated measurement error across respondents, leading to more careful modeling approaches for estimating a latent signal from noisy survey data (Ansolabehere, Rodden, and Snyder 2008). Following this modeling tradition, I refer to an individual's location in policy-ideological space as their "ideal point," the point at which their expected utility of a policy is maximized with respect to their ideological preferences.

1.1.2 Key concept: district-party groups

I argue that another key construct at work in the SPD is the notion of *groups* in the electorate. For a given district, the general election is a contest among all voters, so we consider this constituency as a group. We sometimes refer to this group as the "general election constituency," since it contains anybody who is eligible to vote in the general election. It does not specifically refer to voters only, but contains any citizen who could potentially be a voter in the general election. This ambiguity of who among the general election constituency actually votes is important to understanding a candidate's incentives during the campaign, since the candidate is uncertain whether certain campaign tactics will galvanize some constituents while alienating others.

Another important grouping for this the partisan constituency within a district. Each congressional district contains constituents who are aligned with the Democratic Party or the Republican Party. I call these two groups of constituents *district-party groups*. All 435 congressional districts contain voters from the two major parties, totaling 870 district-party groups. For brevity, I sometimes refer to district-party groups as "party groups" or "partisan groups." A district-party group contains any voting-eligible citizen who resides in a given district and identifies with a given party. As with the general election constituency, membership in a party group is no guarantee that the constituent votes either in the primary or in the general election. The important fact is that they are nominally aligned with one party's voter base over the other. As I discuss below, decomposing a district's voters into separate party groups is the key theoretical innovation in this project. To the best of my knowledge, an empirical study of primary representation that decomposes the voter preferences into district-party groups has never been done, even though it is crucial for testing the implications of the SPD theory.

One important distinction about district-party groups is that they are made of constituents, not organizations. For this reason, it is sometimes helpful to refer to district-party groups as district party "publics," which emphasizes that the groups are composed of ordinary citizens (Caughey and Warshaw 2018). There is no formal registration requirement to be a member of a party group, only a partisan identification. This construction of district-party publics aligns most closely with Key's "party in the electorate" rather than "party as organization" (Key 1955). This distinguishes party publics from interest groups, policy groups, "intense policy demanders," or the "extended party network," which are concepts that describe organizations or maneuvers by political elites rather than rank-and-file constituents (Cohen et al. 2009; Koger, Masket, and Noel 2009; Masket 2009). Although recent research has underscored the importance of elite actors in shaping party nominations, this project focuses specifically on testing the SPD, which is a voter-centric view of primary representation. We bring in important concepts from elite-driven stories of primaries as they apply to particular claims being tested in later chapters.

1.1.3 Key concept: district-party ideology

It is important to define both "policy ideology" and "district-party publics" because they combine to form a key concept that anchors the substantive contributions of this project. This concept is *district-party ideology*: policy ideology aggregated to the level of the district-party group. Just as any individual might have a policy ideology ideal point, and any individual might affiliate with a party, district-party ideology averages the ideological variation within a district-party group into one group-level ideal point. By aggregating policy ideology within groups in this way, this project summarizes how policy ideology differs between Democrats and Republicans in the same district, and it shows how Democratic and Republican party groups vary across congressional districts. This enables us to consider how candidates are responsive to partisan sub-constituencies that together make up a shared general election constituencies (see also Clinton 2006).

1.1.4 Key concept: candidate campaign positioning

As with individual voters, we can imagine that candidates for Congress have campaign platforms, or at least promises and stated issue positions, that are located in ideological space as well. The study of United States politics most commonly places elite political actors in ideological space using their voting records, including members of Congress, Supreme Court justices, federal judges, and state legislators (Clinton, Jackman, and Rivers 2004; Epstein et al. 2007; Martin and Quinn 2002; Poole and Rosenthal 1997; Shor and McCarty 2011). Researchers have extended the modeling intuitions to estimate ideal points from unconventional sources of data, including surveys of congressional candidates, campaign finance transactions, interest group ratings, text from political advertisements, and even Twitter activity (Ansolabehere, Snyder, and Stewart 2001; Barberá 2015; Bonica 2013; Burden 2004; Burden, Caldeira, and Groseclose 2000; Henderson 2016).

This project is interested in the ideological locations of candidates for office as measured through their campaigns. The positioning of campaigns is more directly related to the strategic positioning dilemma than any other concepts that we might scale in ideological space: candidates compete against one another by positioning themselves to appeal to a partisan base of voters, and partisan constituents consult use these campaign positions to nominate the candidate of their liking. To be sure, campaign positions are influenced by other activities that researchers have used to scale candidates for office. Incumbent legislators cast votes to form a defensible record in office, for instance, which both bolsters and constrains their campaign messages (Canes-Wrone, Brady, and Cogan 2002; Mayhew 1974). Not every primary candidate has a roll-call voting record to compare, however, so this project requires an ideal point measure that places incumbents, candidates challenging incumbents, and candidates running for open seats in a comparable ideological space.

This project measures primary candidates' campaign positioning using CFscores from

Bonica's (2019b) *Database on Ideology, Money in Politics, and Elections* (DIME) database. CFscores use campaign contributions to measure the political ideologies of contributors and recipients of campaign contributions, including candidates for office, party organizations, PACs, and individual donors. The estimation method assumes that a donor makes financial contributions to political actors to maximize their utility over all potential contribution choices, which is affected by the ideological similarity between donors and potential recipients (Bonica 2013, 2014). These scores have been used in other studies of primary candidate ideology by Thomsen (2014), Thomsen (2020), Rogowski and Langella (2015), Ahler, Citrin, and Lenz (2016), and Porter and Treul (2020), and similar donation-based ideal point measures by Hall and Snyder (2015) have been used by Hall (2015) and Hall and Thompson (2018). As I discuss in future chapters, CFscore are not without controversy as indicators of elite ideology, especially when comparing members of the same party (Hill and Huber 2017; Tausanovitch and Warshaw 2017), but other research shows that donors differentiate moderate and ideological candidates within the same party (Barber, Canes-Wrone, and Thrower 2016), the ideology component of CFscores outperforms a party-only model of giving (Bonica 2014), and CFscores predict future votes by members of Congress to a similar degree of accuracy as roll-call based scores do (Bonica 2019a).

1.1.5 The strategic positioning dilemma, implications, and research questions

Now that we have defined some key terms, we can see how they relate to previous research on the strategic positioning dilemma. The theory states that candidates balance two competing constituencies during their campaign for office. Candidates face incentives to cater to the median voter in the general election, but they do not progress to the general election without first catering to partisan voters in the primary election. As a result, their campaign position is tailored to split the difference between the two constituencies, perhaps leaning more to the partisan base in safe districts and to the median voter in competitive districts. This section unpacks this intuition in detail and argues that existing research does not test the key claims.

First, how does district-party ideology affect the way candidates position themselves in a campaign? The logic of the SPD suggests that, at minimum, district-party conservatism should be positively correlated to the conservatism of a candidate's campaign position. At maximum, more conservative partisan voters exert a positive causal effect on the conservatism of a candidate's campaign position. This implies that candidates can perceive the conservatism of their partisan constituents, reflecting the relative variation in actual constituents' views if not the absolute level (Broockman and Skovron 2018).

Second, if candidates anticipate partisan voters' policy views and position themselves accordingly, this suggests that candidates believe partisan voters are capable of voting in accordance with their policy views. If this is true, we should expect that district-party groups that are more conservative should be more likely to nominate conservative nominees in primary elections.

These two predictions are the core empirical implications of the "strategic positioning dilemma" theory of representation in primaries. Crucially, testing each prediction requires a researcher to observe the policy ideologies of partisan constituents within a district, which is a separate group from the general election constituency or the location of the median voter. This project argues that district-party policy preferences are either absent from existing research or thoroughly misconstrued—an important theoretical and methodological point that I unpack in Section 1.2.3. As a result, U.S. elections research has been unable to empirically evaluate a widely held theory of representation in primaries.

Stated differently, this dissertation asks if primaries "work" the way the SPD claims they do. It is widely believed that primaries are effective means for voters to inject their sincere preferences into the selection of candidates and, in turn, the priorities of elected officials. Is this *actually* true? The two empirical research questions underlying this project are:

1. Do candidates position themselves to win the favor of primary voters?
2. Do primary voters select the candidate who best represents their issue beliefs?

1.2 Does the Strategic Positioning Dilemma Describe Primary Representation?

1.2.1 Theoretical concerns

The strategic positioning dilemma view of U.S. primaries has reasonable intuitions, but there are reasons to doubt some of its theoretical premises. First, the SPD is put forth as a theory to explain divergent candidate platforms across parties, but there are numerous theories that explain candidate divergence that do not rely on bottom-up pressures from primary voters. And second, the SPD requires voters and candidates to be highly sophisticated actors. Candidates must be capable of perceiving the relative extremity of their constituents, and voters capable of learning about candidate platforms, differentiating between candidates, and acting on sincerely-held preferences over candidate platforms.

The notion of the SPD emerges from a clash between idealized candidate positioning in formal models and the candidate positioning we observe in the real world. Classic formal models highlight a strategic logic for candidates to position themselves by "converging" to the location of the median voter: if constituents vote primarily with policy-based or ideological considerations, then candidates maximize the probability of electoral victory by positioning themselves as closely to the median constituent as possible (Black 1948; Downs 1957).[2]

[2] Some empirical studies of candidate positioning (e.g. Ansolabehere, Snyder, and Stewart 2001; Brady, Han, and Pope 2007) claim that these formal models "predict" candidate convergence at the median voter. In my opinion, this misrepresents the formal work. Downs (1957) in particular explains the logic of candidate convergence, but he also explores many circumstances that would prevent the convergent equilibrium from appearing in the real world. This is important to clarify because, although it is common to describe candidate convergence as a "Downsian result" or a "Downsian prediction," we should recognize that the convergent equilibrium is an oversimplification. Understanding the theoretical incentives that promote candidate moderation is more important than the whether we observe perfect candidate convergence empirically.

Empirical work finds evidence in partial support of both convergent and divergent candidate incentives. Candidates who run in electorally competitive districts are more moderate than co-partisans who are running in districts that run in electorally "safe" districts (Ansolabehere, Snyder, and Stewart 2001; Burden 2004), and even candidates who run in safe districts are marginally rewarded for taking more moderate issue positions than a typical party member would (Canes-Wrone, Brady, and Cogan 2002). Extremist candidates, meanwhile, earn fewer votes and are less likely to win in Congressional elections, and this tendency is stronger in competitive districts than in safe districts (Hall 2015). Despite these incentives to take moderate campaign positions, candidates nonetheless take divergent rather than convergent stances by and large. Republican and Democratic members of Congress vote very differently from one another, and this partisan divergence increased in recent years (McCarty and Poole 2006; Poole and Rosenthal 1997). The difference in legislative voting behavior across parties isn't simply because Republicans and Democrats represent different districts, since Republicans and Democrats who represent similar districts (or the same state, in the case of U.S. Senators) nonetheless vote differently from one another (Brunell 2006; Brunell, Grofman, and Merrill 2016; McCarty, Poole, and Rosenthal 2009). Even among Congressional races in the exact same district, there is a sizable gap between Republican and Democratic candidate positions (Rogowski and Langella 2015). And although qualitative evidence from decades past suggests that candidates take careful positions on issues of local concern (Fenno 1978), more recent systematic tests find mixed evidence of localized, particularistic position-taking. (Canes-Wrone, Minozzi, and Reveley 2011; Fowler and Hall 2016). In total, even though there is some evidence that candidates benefit by positioning themselves as marginally more moderate or more in line with local public opinion, the dominant finding is that candidates take divergent positions that are more closely aligned with a national party platform than with a set of local issue priorities.

The Downsian logic is a strong "centripetal" force that promotes moderation among candidates, but what "centrifugal" forces explain the non-moderate stances (Cox 1990)? Political scientists have explored several theories whose underlying mechanisms are distinct from the SPD notion of competing constituencies. Parties are interested in cultivating long-term reputations for pursuing certain policy priorities (Downs 1957; Stokes 1963). It benefits both major parties for these reputations to be distinct from one another, since parties have office-seeking motivations to mutually divide districts into geographic bases that tend to support one party platform consistently over time (Snyder 1994). Party leaders maintain these party reputations by constructing brand-consistent legislative agendas and pressuring legislators to support reputation-boosting legislation (Butler and Powell 2014; Cox and McCubbins 2005; Lebo, McGlynn, and Koger 2007). In turn, non-median party platforms are more appealing to constituents with ideologically consistent issue beliefs. Candidates benefit by rewarding these constituents in particular because they are more likely to be influence election outcomes in favor of the candidate (Hirano and Ting 2015). These voters are more likely to turn out in general elections than moderate voters are, so it is more efficient for candidates to cater to these constituents. Partisan constituents are also more likely to engage in pro-party activism, such as staffing campaigns, contributing financially to campaigns, and attending party conventions (Aldrich 1983; Barber 2016; La Raja and Schaffner 2015; Layman et al. 2010; McClosky, Hoffmann, and O'Hara 1960).

These incentives for candidates to diverge from median positions are possible without considering primary elections whatsoever. Even if we introduce primary elections into the theoretical story, many plausible explanations for divergence do not rely on outward pressures from ideological primary voters either. Many scholars of political parties maintain that parties retained their gatekeeping roles over party nominations even as the direct primary ostensibly removed their formal powers over candidate selection. Although primary campaigns take place, these scholars argue that an informal network of party actors wields enormous influence behind the scenes, controlling which candidates obtain access to the party's resources, donor lists, and partisan campaign labor (Cohen et al. 2009; Masket 2009). Through these mechanisms, candidates can live or die by the nomination process long before primary *voters* ever enter the picture.

One reason to doubt the SPD on theoretical grounds is that it has high demands of voter sophistication in primary elections. It is well understood that learning about the characteristics and issue positions of political candidates is costly for voters, particularly in non-presidential elections. Party labels on the ballot are valuable heuristics for voters to differentiate the issue positions of Republican and Democratic candidates likely hold (Hill 2015). Primary elections, however, occur most of the time between candidates in the same party,[3] which denies voters' the informational shortcut of a candidate's party affiliation (Norrander 1989). Primary elections often occur during months when voters are paying less attention to politics, and the press cover primary campaigns less closely than general election campaigns. Primary voters have a reputation for being more attentive and sophisticated consumers of political information, but in these lower-information environments, they may cast their ballots for non-policy reasons by prioritizing "Washington outsiders" or identity-based candidate features such as gender or race (Porter and Treul 2020; Thomsen 2020). They may also vote for the familiar candidate instead of the ideologically proximate one, in which case asymmetric campaign expenditures or news coverage may advantage one candidate over the other. For example, Bonica (2020) attributes lawyers' numerical prominence in Congress to their ability to raise early money from their wealthy social networks. Furthermore, despite the disproportionate news coverage received by primary candidates who challenge incumbents on ideological grounds, the absolute number of explicitly ideological primary challenges in a given election cycle is low (Boatright 2013), so primary voters are unlikely to experience a deluge of policy-focused campaign messages even if they are attentive and sophisticated to

[3] There are a few exceptions to this institutional configuration of intra-party nominations. Some states hold blanket primaries, top-two primaries, or "jungle" primaries, where candidates from all parties compete on one ballot to be included in a runoff general election.

receive and process those messages. In short, the claim that voters' policy preferences affect their choices in primary campaigns sounds straightforward, but the information environment of primary campaigns makes it difficult for constituents to vote foremost with their policy ideologies.

The SPD also requires candidates to perceive the policy ideologies of their partisan constituencies accurately in order to position their candidacies in relation to the partisan base and the median voter. Broockman and Skovron (2018) lend contradictory evidence to this notion by measuring the degree to which politicians "misperceive" their constituency's policy views. The authors find that elected politicians believe that their constituents are much more conservative on many issues than they actually are, which could affect how accurately candidates position themselves in relation to constituent views.

1.2.2 Empirical ambiguity

Empirical support for the strategic positioning dilemma is as unclear as the theoretical underpinning. When researchers conduct empirical tests of the SPD or the narrower premises of primary representation and competition on which it rests, the results are ambiguous and often contradictory of the SPD story. This section reviews existing research in this area to review the outstanding questions and preview the substantive innovations in this project.

Much of the interest in primary elections and representation comes from a focus on candidate divergence and partisan polarization. Why do candidates who stand for general election take divergent stances from one another, and do the competitive dynamics of primary elections increase this divergence? Prominent studies of candidate positioning in general elections initially found conflicting evidence about the influence of stiff primary competition on candidate extremity. Using survey data from congressional candidates during the 2000 campaign, Burden (2004) finds that general election candidates take more extreme policy positions in their campaigns if they also faced stronger primary competition. This makes sense especially if primary candidates care more about the candidate's ideological positioning than general election voters do, the latter of whom are also receptive to non-policy appeals. Ansolabehere, Snyder, and Stewart (2001) find the reverse pattern using 1996 survey data. The gap between major party candidates was actually smaller when one of the candidates faced stiffer primary competition. This counter-intuitive finding makes sense if the presence of a primary challenger is itself a consequence of candidate positioning. If an incumbent maintains a partisan reputation, this may fend off credible primary challengers who have less room to wage an ideological campaign against the incumbent. As a result, the *threat* of a primary challenge exerts a centrifugal force on candidate positioning, even if a primary challenger never actually appears (Hacker, Pierson, and others 2005). Hirano et al. (2010) study this threat-based hypothesis by measuring potential primary threat as the average presence of primary competitors in down-ballot races. In district with high levels of latent primary threat, we might expect the incumbent to take more extreme stances in Congress. Although the idea that incumbents vote as party faithfuls to preempt opportunistic challengers is intuitive and supported by other research (e.g. Mann 1978), this measure was not meaningfully related to the extremity of an incumbent's voting record in Congress (Hirano et al. 2010). In short, the evidence of the polarizing effects of primary challenges is mixed and unclear.

Researchers interested in the polarizing effects of primaries on candidates and legislators has also examined primary "rules." Political parties are private organizations, and nominees are intended to represent the parties' priorities and governing values, but participation in primary elections is not always restricted to party members only. Primary "openness" rules that govern who can participate in a partisan primary are managed by state election law, with some allowances for parties to set rules within those limits. States with "closed" primaries restrict participation in primaries only to individuals who are registered as Republicans or Democrats in their state registration records. States that allow third-party or non-partisan voters to participate in partisan primaries are "partially" open, and states where any voter can participate in any primary are regarded as "open" primaries. I discuss finer details of primary rules in later chapters. Researchers seeking to exploit state-level variation in primary rules hypothesize that states with more restrictive participation criteria might select more ideologically extreme primary nominees, and states with more relaxed rules might select relatively moderate nominees. This is because primary voters are commonly believed to hold more ideologically consistent policy views than other constituents, so candidate polarization will respond to the polarization among the voting public (Jacobson 2012). However, the consensus among recent studies finds little evidence supporting the hypothesis that primary rules affect polarization in congress or candidate divergence more broadly. This is because there is little consensus in public opinion research that partisans who participate in primaries are much different from partisans who do not participate in primaries, either demographically or ideologically (Geer 1988; Hill 2015; Jacobson 2012; Norrander 1989; Sides et al. 2018), though these studies cover many years, and the dynamics of primary voting might have changed. And even recent studies that find that primary voters hold more ideologically consistent views find no evidence that closed primaries nominate candidates that are more ideologically off-center (Hill 2015). This finding appears to hold for the House, Senate, and state legislatures through the past several decades (Hirano et al. 2010; McGhee et al. 2014; Rogowski and Langella 2015). Even reforms that drastically change the primary rules, such as California's recent shift to a blanket primary where candidates in all parties compete for the same limited number of positions on the general election ballot, do not nominate legislators whose voting records are much more moderate than before (Bullock and Clinton 2011).

These studies are incomplete in important ways that bear on the key substantive questions underlying this project. Most of these studies evaluate primaries' effects on representation by examining roll-call votes only. Since roll-call votes are only observable for incumbents, many of these analyses cannot measure candidate *divergence* because they cannot compare incumbents to non-incumbents nor two open-seat candidates. Some notable studies examine non-incumbent candidates for general election using candidate surveys (Ansolabehere, Snyder, and Stewart 2001; Burden 2004), but these studies are also limited because they do not observe the positions of candidates who lose the primary nomination. Without observing primary losers, we have no way of knowing if the general election candidate was relatively moderate or ideological in comparison to other primary candidates. It is much rarer for a study to measure primary candidate positioning as the key outcome variable using a method that covers incumbents, challengers, and open-seat candidates (Rogowski and Langella 2015).

1.2.3 Vote shares do not identify policy ideology

Another important drawback of the existing research on primaries and ideological representation is the way these studies handle voters' policy preferences. The strategic positioning dilemma pits two constituencies in a district against each other: the nominating constituency (district-party group) that contains constituents from one party's base, and the general election constituency that contains constituents from both major parties and with no party affiliation. The former is theorized to prefer ideologically faithful candidates who adhere closely to a partisan policy platform, while the latter prefers moderate candidates in the general election. Studies routinely acknowledge this distinction in theory, but they often abandon the distinction between the two groups in applied studies, instead operationalizing the preferences of all three constituencies—the general constituency and two partisan primary constituencies—using the same measure: the district-level presidential vote.

This project argues that the presidential vote is not a suitable for the study of primary representation for the simple reason that votes are not equivalent to policy preferences or policy ideology. Votes are choices that voters make under constraints, namely, the distance between the voter and the presidential candidates. Even in simple models where ideology is the only factor influencing vote choice, observing a voter's choice of candidate contains very little information about their ideological location. In the aggregate, Republican voters in a district may be ideological moderates or ideological conservatives, and the fact that they vote Republican does not inform us on the ideological distribution of Republican voters. Similarly, a district's vote outcome captures how all of its constituents vote *on average*, but because partisans tend to vote foremost for their preferred party even in the face of strong policy disagreements with the candidate (e.g. Barber and Pope 2019), aggregate vote shares for a district could easily be more affected by the *number* of Republicans and Democrats in a district rather than the exact location of their ideological preferences. Using the terminology by Tomz and Van Houweling (2008), studying vote shares rarely presents a "critical test" of theories of voting because the same observable vote outcome can arise from many underlying voter preference configurations.

Stated differently, the observed vote share in a district does not uniquely identify any important features of the underlying preferences of voters. Figure 1.1 demonstrates the problem using a simple theoretical model of ideological voting for president. We begin by demonstrating the the basic mechanics of the scenario in the two left-side panels. In this scenario, we consider one congressional district that contains many constituents. Every constituent has a policy ideal point represented on the real number line, with larger values indicating greater policy conservatism. Every constituent also identifies with either the Republican Party or the Democratic Party. The top-left panel breaks voters into Democratic and Republican Party affiliations and shows the probability distribution of ideal points within each partisan base, which in this example are both Normal distributions with a scale of 1. Republican-identifying constituents hold policy preferences that are more conservative than Democratic constituents on average: the median Republican and Democrat are respectively located at 1 and -1.[4] There is enough within-party variation that some Democratic constituents are more conservative than some Republican constituents, despite their party affiliation.

[4] Because these are Normal distributions, the median and the mean are equivalent. I refer to the median instead of the mean because medians are more directly relevant to spatial models of voting.

The bottom-left panel combines the two partisan distributions into one distribution for the entire constituency. We assume at first that both partisan constituencies are equally sized, so the composite distribution is a simple finite mixture of the two distributions.[5] The midpoint between two presidential candidates is shown at policy location 0. Assuming all constituents vote according to single-peaked and symmetric utility functions over policy space, constituents are indifferent between candidates if they have ideal points equal to 0, vote for the Democratic candidate if they have ideal points less than 0 (shown in darker gray), and vote for the Republican candidate if they have ideal points greater than 0 (shown in lighter gray). The aggregate election result, therefore, is equal to the cumulative distribution function of the combined distribution evaluated at the candidate midpoint. In the bottom-left panel, the vote share for the Democrat is 50%, with some Democrats voting for the Republican candidate, and some Republicans voting for the Democratic candidate.

The panels on the right side of Figure 1.1 show how slight changes to one party's preference distribution affects the aggregate distribution of preferences in the combined constituency and, as a result, the presidential vote share in the district. The composite distribution is again shown in gray, with dark and light shades indicating vote choice as in the bottom-left panel. The underlying partisan distributions are outlined only with red and blue lines to reduce visual clutter. The modifications to the underlying partisan preferences are simple, but even these simple changes reveal the fundamental problem with using district voting as a proxy for policy ideology in the voting population. In each panel, I intervene on only one feature of the Democratic Party ideal point distribution, leaving the Republican distribution untouched (median of 1, standard deviation of 1). Intervening on just one component of one party's

[5] Analytically, if $f_p(x)$ is the probability density of ideal points x in party p, then the composite density $f_m(x)$ is a weighted sum of the component densities: $f_m(x) = \sum_p w_p f_p(x)$, where w_p is a mixture weight representing the proportion of the total distribution contributed by party p, with weights constrained to sum to 1. In this first example, both partisan constituencies are equally populous, so both parties have weight $w_p = \frac{1}{2}$. If parties had different population sizes within the same district, w_p would take values in proportion to those population sizes.

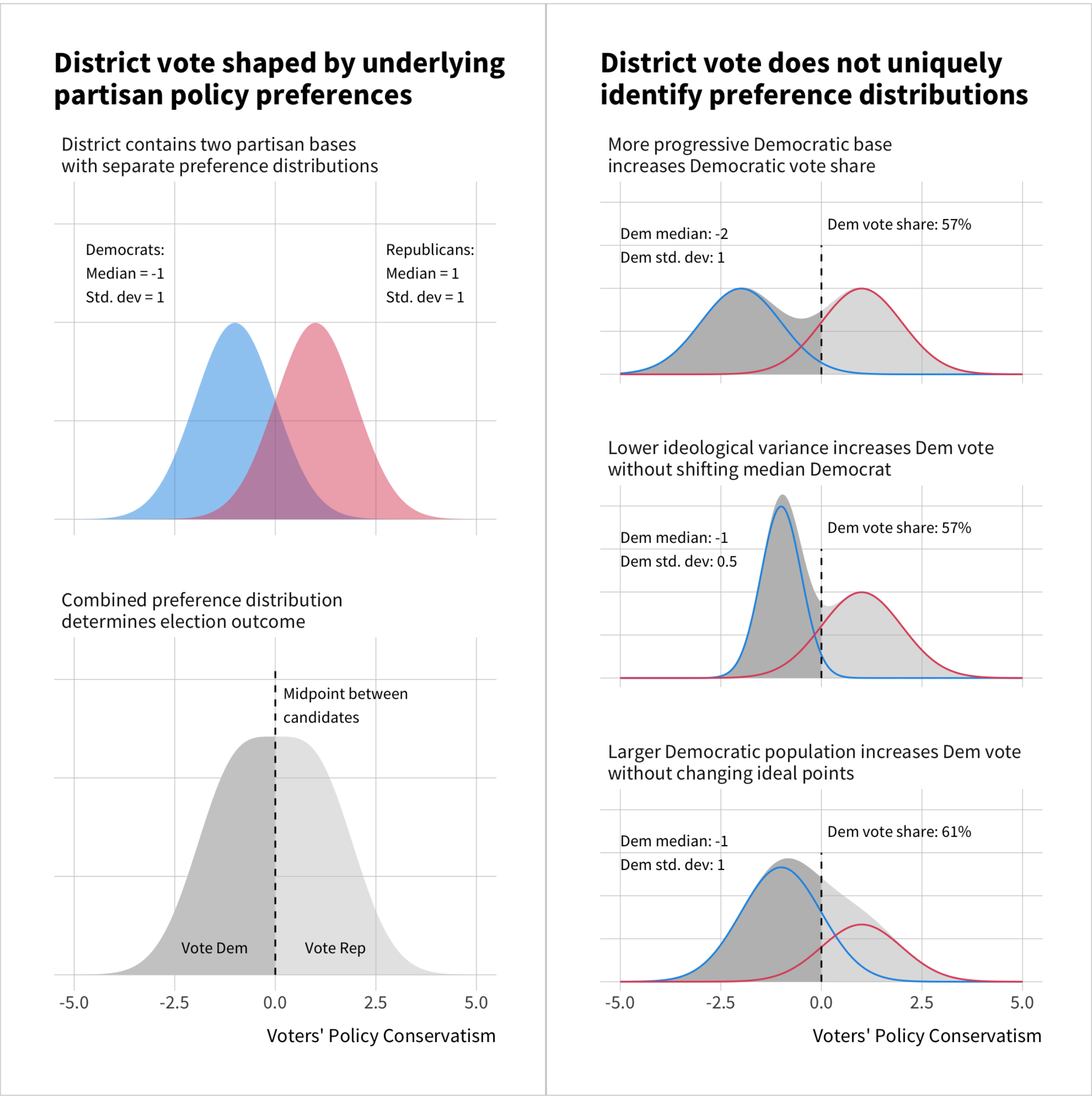


Figure 1.1: Demonstrating how district vote shares from a single election are insufficient to identify underlying policy-ideological features of the district. The left side shows how the policy preference distributions for two parties in a district (top panel) combine to form an aggregate preference distribution for the district as a whole (bottom panel). The right side shows how the Democratic vote share is affected by changes to either the locations, the scales, or the population sizes of the underlying partisan distributions.

distribution is meant to keep the demonstration simple, bearing in mind that the problem is much more complex in the real world, where we can imagine multiple simultaneous changes to both parties at once. The interventions highlight two classes of problems. First, we can perform multiple modifications of the underlying partisan distribution that obtain the same aggregate vote share. This proves that the district vote does not uniquely identify the characteristics of the underlying voter distributions. And second, we can alter the district vote outcome by changing party *sizes* without any change to the ideal point distribution within either party. This proves that vote shares may vary across districts even if partisan ideal points distributions are the same.

In the top-right panel, I shift the location of the Democratic ideal point distribution to the left, from a median of -1 to -2. This location shift results in a greater number of Democratic constituents with ideal points left of the candidate midpoint, increasing the Democratic vote share in the district from 50% to 57%. In the middle-right panel, I shrink the scale of the Democratic ideal point distribution from a standard deviation of 1 to a standard deviation of 0.5. Lower ideal point variance within the Democratic base has the exact same effect on the vote as shifting the location: more Democratic voters left of the midpoint, which increases the Democratic vote share to 57%. This means that compared to a district with a 50% presidential vote split, we would not be able to attribute the increased Democratic vote to a constituency that is *more progressive on average* (location) or simply *less heterogeneous* in its policy preferences (scale). The bottom-right panel in the figure shows how we obtain a different district vote without changing the underlying ideological distribution in either party whatsoever, instead changing only the relative population size of each partisan base. The Democratic base in the final panel is unchanged compare to the original distribution laid out in the top-left: median of -1 and standard deviation of 1. The only difference is that the district contains an unequal balance of partisan voters, two Democratic constituents to every one Republican constituent. This results in an increased Democratic vote from 50%

to 61%—ironically, the largest impact on the overall district vote despite not changing the ideological distribution of either party.

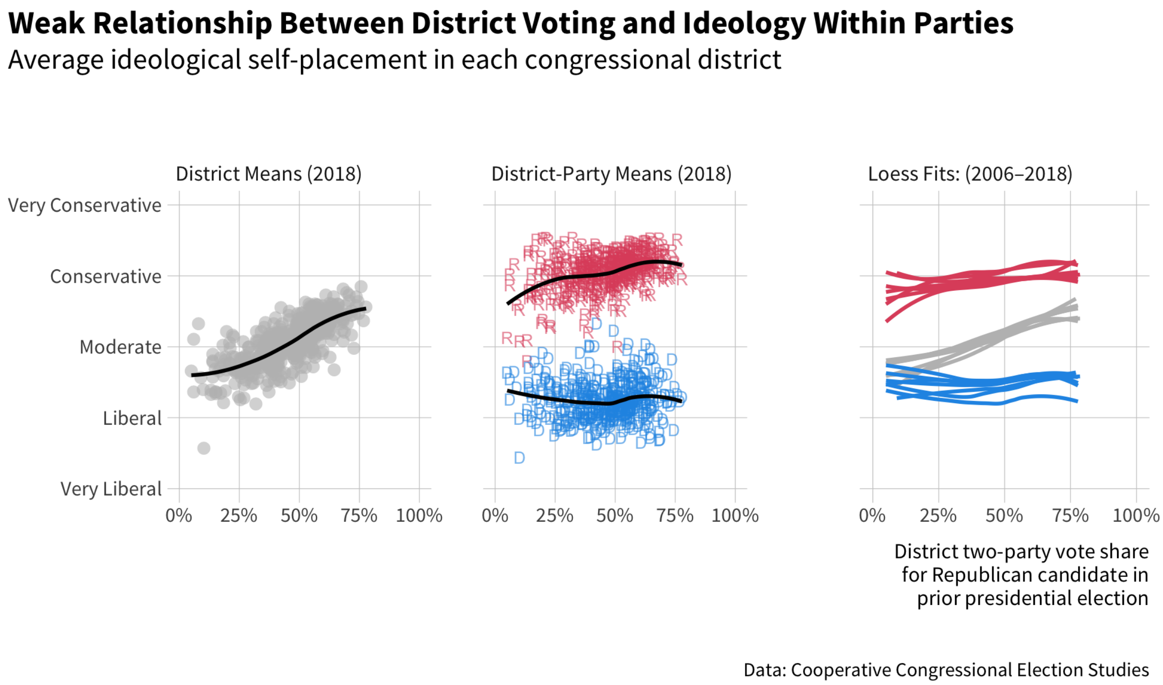
To review the lessons of Figure 1.1, observing a Democratic vote share greater than 50% reveals very little about the underlying distribution of voters. In every panel, we observe an increase in the Democratic vote compared to our baseline scenario, but the the median voter in either party does not need to change in order for vote shares to be affected. Since the Republican distribution is identical in every panel, inferring that Republicans are less conservative in districts with greater Democratic voting would be incorrect in every case. For the Democratic constituents, inferring a more progressive Democratic median voter from greater Democratic voting would be wrong in two of the three cases.

It is worth repeating that the scenario laid out in Figure 1.1 is a vast oversimplification of the real electorate. This is intentional, as it shows how intractable the problem becomes even in an artificial setting where we can take many variables as given. This scenario contains no complicating elements such as non-partisan or third-party identifiers, non-policy voting, random sources of utility or utility function heterogeneity across different voters, differential turnout between partisan bases, and so on, that we might incorporate directly into a formal model. It also does not take into account the inconveniences of real election data, where short-term forces impose additional shocks to vote shares that are unrelated to underlying voter preferences.

The conceptual difference between district vote shares and aggregate ideology appears in real data as well, as shown in Figure 1.2. The figure shows ideological self-placement responses to the Cooperative Congressional Election Study (CCES) as an approximate measure of a citizen policy ideology. I calculate the average self-placement for all respondents in each congressional district, as well as the average self-placement of Republican and Democratic identifiers as separate subgroups within each district. The first two panels use 2018 data to show that the district vote captures variation in ideological self-placement reasonably well when examining congressional districts as a whole, but it does a poorer job capturing variation in self-placement within each party. The first panel shows that districts that voted more strongly for Democratic presidential candidate in 2016 were more liberal on average, and districts that voted more strongly for the Republican candidate in were more conservative, indicated by a positively sloped loess fit line. The middle panel shows that this pattern does not hold as strongly within parties. Among Republican identifiers within each district, a weaker but still positive relationship holds overall, with more conservative Republicans in districts that voted more Republican. Among Democratic identifiers, however, ideological self-placement is not as strongly related to aggregate voting, with a loess fit that is flatter and even negative at several points. The final panel of loess fits is included to show that this pattern appears in all CCES years and is not particular to 2018 CCES responses: a strong relationship between vote shares and self-placement *on average*, and weak or non-relationships within each party.

The substantive takeaway from Figure 1.2 is further evidence that we should doubt the use of aggregate voting in a district is a reliable proxy of ideological variation within partisan primaries. Because the presidential candidates are the same in each district in each year, we know that this mismatch isn't due to different candidates with different campaign positions in each district. Instead, the observed pattern suggests that any aggregate relationship between ideological self-placement and district voting is driven at least in part by the partisan *composition* of a district—more Republicans or more Democrats—rather than cross-district ideological variation within either party. As a result, studies that use the presidential vote to proxy within-party ideology may simply be measuring the *size* of a partisan group in a district instead of its ideological makeup.

Some researchers have recognized the identifiability problems with district presidential vote shares as a measure of district preferences. Levendusky, Pope, and Jackman (2008) specify a Bayesian structural model to subtract short-term forces on election results and


Weak Relationship Between District Voting and Ideology Within Parties

Average ideological self-placement in each congressional district

District Means (2018)

District-Party Means (2018)

Loess Fits: (2006–2018)

Data: Cooperative Congressional Election Studies

Figure 1.2: Average ideological self placement (vertical axis) and Republican vote share (horizontal axis) in all 435 congressional districts. Mean self-placement is calculated by numerically coding CCES ideological self-placement responses before averaging. The first panel plots average self-placement among all CCES respondents in each congressional districts. The middle panel breaks respondents in each congressional district into Republican and Democratic subgroups before averaging. The final panel plots loess fits for the same relationship measured over all CCES years.

isolate latent partisanship. Kernell (2009) formally proves that using a single election to cardinally place district ideal point medians is never possible, but that estimating the mean and variance of ideal point distributions is possible under distributional assumptions and a formal model of voting. Although these methods are promising innovations over the common practice of using votes as a proxy for policy preferences, I have uncovered no studies of primary representation in the intervening years that have incorporated these methods. Furthermore, the methods estimate the median policy preference for a district as a whole. They do not describe separate partisan constituencies within a district, which is the essential missing ingredient.

I stress that this measurement problem is more than methodological nitpicking. The theoretical consequences are systemic. The literature's dependence on the presidential vote as a proxy for district preferences has prevented scholars from incorporating key theoretical constructs into empirical studies of primaries: the ideological preferences of partisan voters. Without serviceable measures of partisan policy preferences, we can say very little about the role of primary elections in the broader democratic order of U.S. politics. This affects our knowledges of topics beyond party nominations as well. To study how politicians weigh the opinions of various subconstituencies, which the study of U.S. politics is obviously interested in (Bartels 2009; Clinton 2006; Cohen et al. 2009; Fenno 1978; Gilens and Page 2014; Grossman and Hopkins 2016; Phillips 1995; Pitkin 1967), research must be able to measure the policy preferences of subconstituencies directly. The technology to estimate subconstituency preferences using survey data is admittedly quite new, and this district intends to continue this effort by extending existing models, highlighting important methodological considerations for model building and computation, and demonstrating how to use these measures for observational causal inference.

1.3 Project Outline and Contributions

1.3.1 Measuring district-party ideology

This chapter has so far identified a shortcoming in the study of primaries that subconstituency preferences are rarely measured. This project rectifies this shortcoming by measuring district-party ideology for Republican and Democratic party groups in Chapter 2. This allows the project to carry out direct tests of SPD hypotheses that were previously impossible in Chapters 4 and 5.

I estimate district-party ideology this using an item response theory (IRT) approach to ideal point modeling. The model estimates the policy ideology for a typical Democrat and a typical Republican in each congressional district over time. I employ recent innovations in hierarchical modeling to measure individual traits at subnational units of aggregation using geographic and temporal smoothing (Caughey and Warshaw 2015; Lax and Phillips 2009; Pacheco 2011; Park, Gelman, and Bafumi 2004; Tausanovitch and Warshaw 2013; Warshaw and Rodden 2012). The model I build extends these technologies by specifying a more complete hierarchical structure for the bespoke parties-within-districts data context, a more flexible predictive model for geographic smoothing, and advances in Bayesian modeling best-practices from beyond the boundaries of political science (see also Section 1.3.5).

1.3.2 Empirical tests: how district-party ideology matters

After estimating the ideal point model for district-party groups, I apply these estimates in two critical tests of the strategic positioning dilemma.

Chapter 4 studies how district-party ideology affects candidate positioning in primary elections. If the primary constituency exerts a meaningful centrifugal force on candidate positioning, we should expect candidates with more ideological partisan constituencies to take more ideological stances, all else equal.

Chapter 5 studies how district-party ideology affects candidate selection in primary elections. If the primary constituency exerts a credible threat against candidates taking overly moderate campaign positions, we should expect more ideological constituencies to select more ideological candidates, all else equal.

An important institutional factor at play in each of these empirical settings is the moderating effects of primary openness. Past studies have explored whether primaries that are closed to non-partisan and cross-partisan participation lead to the election of more extreme party nominees. District-party ideology is missing from these studies, but it matters for our theoretical expectations about the effects of primary rules. For instance, we should not expect a relatively partisan constituency to nominate an extremist candidate solely because the primary is closed to non-party members. Past studies have either ignored ideological variation across districts or used unsuitable proxy measures that do not measure district-party ideology. Including primary rules in Chapters 4 and 5 will provide a more faithful test of the primary rules hypothesis.

This project is not rooting for or against the veracity of the strategic positioning dilemma as a model of primary representation. The theory is intuitive and reasonable in its predictions for rational elite behavior, but its assumptions about voter competence and its empirical track record are less supportive of the theory. I wish for the empirical components of this project to be theory *testing* rather than advocacy for or against an idea in current political thought.

1.3.3 Causal inference with structural models

The strategic positioning dilemma is a story about the causal effects of district-party ideology on candidate positioning and candidate selection. Testing the theory requires a serious engagement with causal inference methods. Unfortunately, the observational data at work are difficult to manipulate in support of causal claims. District-party ideology is not randomly assigned, so we require methods for identifying unconfounded variation by design or adjusting for confounding with careful modeling.

One inherent limitation of the district-party ideology estimates is that they come from a measurement model. The measurement model smooths estimates with a hierarchical regression, where partial pooling improves the estimate for one unit "borrowing information" from other units. This shrinks estimates toward one another, imposing correlations between estimates that share a common cause. To leverage exogenous variation for design-based causal inference, this variation would likely have to come predominantly through exogenous shocks to raw survey data, which is challenging to conceive of considering that many surveys must be pooled to achieve feasible estimates at the district-party level.

Given these data limitations, this project turns to causal identification through a conditional independence assumption (Rubin 2005), also known as “selection on observables.” Although selection on observables is a common approach to quantitative research, many analyses are not careful about their modeling choices, controlling for variables that do not improve causal identification or using modeling approaches that impose fragile or implausible functional assumptions on the data. One guiding ethic for the methodological contributions in this project is to take observational causal modeling more seriously than the existing research on primary representation by setting up empirical analyses that aspire to do the following:

- clearly state the potential outcomes model that links treatments, outcomes, and confounders.
- clearly state the causal estimand implied by a causal structure.
- clearly state the assumptions required to identify estimands and how modeling approaches relate to identification assumptions.
- use modeling approaches that are flexible enough to absorb confounding effects without too much dependence on strict functional forms.

I hope to satisfy these aims by invoking more explicit causal models of potential outcomes (Rubin 2005) and using “structural causal models” (SCMs) to guide model specification choices (e.g. Pearl 1995). The SCM approach makes heavy use of causal diagrams, or “directed acyclic graphs” (DAGs), to visualize a causal structure and identify causal claims. Causal diagrams as heuristic devices for causal inference are not new to political science in general (Gerring 2001), but combining causal diagrams with the formal exactitude of the current causal inference tradition is less common in political science. Furthermore, SCMs and causal diagrams are less common in the literature on primaries and representation, which has not been as explicit about causal assumptions and empirical designs, with some notable exceptions (Fowler and Hall 2016; Hall 2015).

This project's approach to causal inference has two stand-out contributions to the study of primary representation that would be impossible but for this approach. First, Chapter 4 contains a detailed discussion of the causal effect of district party ideology on candidate positioning *as mediated by* aggregate district partisanship. I lay out the causal structure in causal graphs, discuss identification assumptions required to estimate the causal quantity of interest, and implement a sequential-*g* modeling approach to estimate it (Acharya, Blackwell, and Sen 2016). Chapter 5 explores flexible modeling with machine learning (ML) as a way to reduce dependence on fragile model assumptions. The chapter discusses *regularization-induced confounding*, a statistical bias in a treatment effect estimate that arises when regularized estimators, such as those used in common ML methods, under-correct for strong confounding by injecting too much shrinkage into a statistical model. I show how to correct this bias using Neyman orthogonalization, a two-stage modeling approach that de-biases causal estimates by reparameterizing the structural causal model (Hahn et al. 2018). Regularization-induced confounding is a serious problem for high-dimensional causal inference, but it has been discussed almost nowhere in political science (Ratkovic 2019).

Selection on observables is a fragile assumption for causal identification, which leads many researchers to speak in "scientific euphemisms" about causality instead of invoking explicit causal language (Hernán 2018). I adopt the position that this "taboo against explicit causal inference" is harmful to the larger aims of a research program because it obscures the dependence of research findings on causal assumptions, whose transparency is essential for credible causal inference, and leads work to be misinterpreted by future audiences who tend to interpret findings as causal regardless of author intent (Grosz, Rohrer, and Thoemmes 2020). No study will ever prove the existence of a causal effect. Researchers should be transparent about causal assumptions so that future readers and researchers have clearer ideas about how to improve previous work. As such, this work will invoke causal language, highlight identification assumptions, and discuss threats to identification assumptions openly.

1.3.4 Bayesian causal modeling

Another important methodological contribution in its Bayesian approach to causal inference. The key independent variable of interest, district-party ideology, is estimated using a Bayesian measurement model. It is not observed exactly, but it is estimated up to a probability distribution. Using those estimates in subsequent analysis requires some accounting for the uncertainty in those estimates. I do this by propagating the Bayesian framework from the measurement model forward into the causal models. Operationally, this is done by taking the posterior distribution from the measurement model and using it as a prior distribution in subsequent models, recovering a joint probability distribution that captures uncertainty in causal effects and its relationship to the uncertainty in the underlying data.

Although the Bayesian view of causal inference is not new (Rubin 1978a), it appears almost nowhere in political science. Political scientists occasionally use Bayesian technology for analytical or computational convenience (e.g. Horiuchi, Imai, and Taniguchi 2007; Carlson 2020; Ornstein and Duck-Mayr 2020; Ratkovic and Tingley 2017), but rarely are the epistemic contours of Bayesian analysis explicitly credited for adding value to a causal analysis (Green et al. 2016; in economics, see Meager 2019).

Chapter 3 explores a Bayesian approach to causal inference in political science at length. It lays out a probabilistic model of potential outcomes adapted from Rubin (1978a) and discusses how to interpret causal inference research designs through a Bayesian updating framework. I give pragmatic guidance for thinking about priors and specifying Bayesian causal models, and I demonstrate the modeling approaching by replicating and extending a few published analyses in political science, noting where the Bayesian approach leads to different conclusions and interpretations about the findings.

I apply Bayesian approaches to causal modeling in Chapters 4 and 5 by combining multistage models into one posterior distribution, which is natural for applied Bayesian modeling where causal effects can be summarized by marginalizing over “design-stage” parameters (Liao and Zigler 2020). Bayesian estimation is also valuable in Chapter 5 to quantify uncertainty in machine learning methods. This is done using a Bayesian neural network model, which automatically penalizes model complexity using prior distributions and quantifies treatment effect uncertainty in the posterior distribution (Beck, King, and Zeng 2004; MacKay 1992).

1.3.5 Bayesian best practices

Another important contribution of the modeling exercise is the detailed discussion of Bayesian modeling and computational implementation it contains. Classic Bayesian texts for political and social sciences are written for an outdated computational landscape where Metropolis-Hastings and Gibbs sampling algorithms were state-of-the-art estimation approaches (Gill 2014; Jackman 2009). Recent years have seen rapid progress in the development and understanding of Hamiltonian Monte Carlo algorithms, which are faster, more statistically reliable, and easier to diagnose (Betancourt 2017, 2019; Duane et al. 1987; Neal 2012), but they also require renewed attention to the way researchers specify and implement Bayesian models (Betancourt and Girolami 2015; Bürkner and others 2017; Carpenter et al. 2016). Furthermore, this new generation of applied Bayesian modeling has updated best practices for specifying priors, modeling workflow, and model evaluation that (to my knowledge) have no precedent in the current political science awareness (Betancourt 2018; Gabry et al. 2019; Gelman, Simpson, and Betancourt 2017; Lewandowski, Kurowicka, and Joe 2009; Vehtari, Gelman, and Gabry 2017; Vehtari et al. 2020). One contribution of this project is to highlight the evolving landscape for Bayesian thinking and Bayesian workflow, which has not received its due attention as a new generation of political scientists explores Bayesian analysis.

— 2 —

Hierarchical IRT Model for District-Party Ideology

To study how partisan constituencies are represented in primary elections, we require a measure of the partisan constituency's policy preferences. This chapter presents the statistical model that I use to estimate the policy ideal points of district-party publics.

This chapter proceeds in three major steps. First, I review the theoretical basis for ideal point models, which can be traced to spatial models of policy choice from classic formal theory work in American political science such as Downs (1957). I connect these formal models to statistical models of policy ideal points (in a style that follows Clinton, Jackman, and Rivers 2004) as well as their connection to Item Response Theory (IRT) models from psychometrics and education testing (e.g. Fox 2010).

Second, I specify and test the group-level model that I build and employ in my analysis of district-party publics. This discussion includes details that are relevant to Bayesian estimation, including identification restrictions on the latent policy space, specification of prior distributions, and model parameterizations that expedite estimation with Markov chain Monte Carlo (MCMC). I begin with a static model for one time period, and then I describe a dynamic model that smooths estimates across time using hierarchical priors for model parameters (Caughey and Warshaw 2015). I test both the static and the dynamic models by fitting them to simulated data and determining how well they recover known parameter values.

Lastly, I describe how I fit the model to real data. This section describes data collection, data processing, and model performance, and a descriptive analysis of the estimates.

2.1 Spatial Models and Ideal Points

Ideal points are constructs from *spatial* models of political choice. These models exist under formal theory—they simplify scenarios in the political world into sets of actors whose behaviors obey utility functions that conform to mathematical assumptions. Spatial models invoke a concept of "policy space," where actor preferences and potential policy outcomes are represented as locations along a number line. A canonical example is a left-right continuum, where progressive or "liberal" policies occupy locations on the left side of the continuum, while conservative policies are on the right side. Actors are at least partially motivated by their policy preferences, so they strive to achieve policy outcomes that are closest to their own locations. Depending on the structure of the game, these actors often face constrained choices; they can't achieve their most-preferred policy, so they settle on something that is as close as they can get.

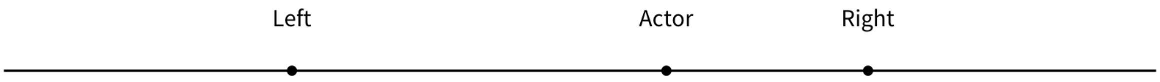


Figure 2.1: An Actor and two policy outcomes (Left and Right) represented as locations in ideological/policy space

Figure 2.1 plots a simple example of an Actor's choice over two policies in one-dimensional policy space. The "Left" outcome is a more progressive policy outcome than the "Right" outcome, indicated by their locations on the line. The Actor has a location herself, which corresponds to her most-desired policy outcome. There is no policy located exactly at the Actor's preferred location, but the Actor is closer to the Right policy than to the Left.

Supposing that the Actor could make an error-free choice over which policy to implement, it appears she would prefer the Right outcome to the Left outcome.

Formal models are more careful to specify the assumptions governing these scenarios, which can be complicated in many cases. For example, suppose that locations along the left-right continuum can be assigned values on the real number line. Figure 2.1 shows a one-dimensional number line, but policies can be generally represented as locations in multidimensional $\mathbb{R}^d$ space. The Actor's location is synonymous with her most preferred policy: her "ideal point." This is the point where the Actor's utility, in an economic utility model, is maximized with respect to policy considerations. Utility implies that the Actor has a utility function that is defined over the policy space, which depends on the distance between her ideal point and a potential policy outcome. Outcomes nearer to the Actor's ideal point are generally more preferred than farther outcomes, but this too is subject to assumptions about the shape of the Actor's utility function. Typically utility functions are assumed to be single-peaked and symmetric around an Actor's ideal point, so a closer policy is always more preferred, all else equal. The notion of an ideal point is similar to a "bliss point" in microeconomics: the optimal quantity of a good consumed such that any more or less consumption would result in decreased utility. Whether an Actor can choose the closest policy to herself depends on the structure of the game: the presence and strategy profiles of other Actors, the sequence of play, and the presence of other non-policy features of Actors' utility functions.

Formal models of ideal points are distinct from statistical models of ideal points. Formal models are primarily theoretical exercises; they explore the incentives and likely actions of Actors in specific choice contexts, building theoretical intuitions that can be applied in the study of real-world politics with real data. Statistical models, on the other hand, explicitly or implicitly *assume* a formal model as given and estimate its parameters using data. Data could come from legislators casting voting on bills, judges ruling on case outcomes, survey respondents stating their policy preferences (as in this project), and other situations. Researchers are typically interested in parameter estimates for the Actors' ideal points, although sometimes the parameters about the policy alternatives are substantively interesting.

Having distinguished formal and statistical models, I now show a derivation of a statistical model from a formal model. This exercise model will serve as a theoretical basis for the class of statistical models explored in this dissertation. I begin with notation to describe an arbitrary number of actors indexed $i \in \{1, \ldots, n\}$ making an arbitrary number of policy choices (bills, survey items, etc.) indexed $j \in \{1, \ldots, J\}$. Every Actor has an ideal point, or a location in the policy space, represented by θ_i. Every task is choice between a Left policy located at L_j and a Right policy located at R_j.

The utility that an Actor receives from a Left or Right choice is a function of the distance between her ideal point and the respective choice location. Utility is maximized if an Actor can choose a policy located exactly on her ideal point, and utility is "lost" for choices farther and farther from her ideal point. The functional form of utility loss is an assumption made by the researcher. Some scholars assume that utility loss follows a Gaussian curve, while others choose a quadratic utility loss (Clinton, Jackman, and Rivers 2004). For this analysis, I assume a quadratic utility loss.[1]

The choice of quadratic loss implies a utility function over the *squared distance* between an Actor and a choice location. The utility Actor i receives from choosing Left or Right are given by utility functions $U_i\left(L_j\right)$ and $U_i\left(R_j\right)$, respectively. With quadratic utility loss, these utility functions take the form

$$\begin{aligned} U_i\left(R_j\right) &= -\left(\theta_i - R_j\right)^2 + u_{ij}^{\mathsf{R}} \\ U_i\left(L_j\right) &= -\left(\theta_i - L_j\right)^2 + u_{ij}^{\mathsf{L}}, \end{aligned} \tag{2.1}$$

[1] Researchers typically avoid linear losses for technical reasons: a linear utility loss function is non-differentiable at the ideal point because function comes to a point. This prevents the researcher from using differential calculus to find a point of maximum utility.

where u_{ij}^{R} and u_{ij}^{L} are the idiosyncratic error terms for the Right and Left alternatives, respectively. I sometimes refer to the quadratic utility loss as the "deterministic" component of the Actor's utility function, while the idiosyncratic error terms are "stochastic" components.

With these utility functions laid out, Actor *i*'s decision can be a comparison of the utilities received by choosing Right or Left. Let y_{ij} indicate the Actor's choice of Right or Left, where Right is coded 1, and Left is coded 0. The model so far implies that $y_{ij} = 1$ (Actor chooses Right) if their utility is greater for Right than for Left.

$$y_{ij} = 1 \iff U_i\left(R_j\right) > U_i\left(L_j\right) \tag{2.2}$$

To visualize this choice, I represent the deterministic components of Equation (2.2) in Figure 2.2, omitting the stochastic utility terms. The parabola represents *i*'s fixed utility loss for any choice along the ideological continuum, owed to her distance from that choice. The vertex of the parabola is at the Actor's location, indicating that she would maximize her spatial utility if she could choose a policy located exactly at her ideal point. Dashed lines below the Left and Right alternatives represent the utility loss owed to the Actor's distance from those specific choices. In the current example, the Actor is closer to Right than to Left, so she receives greater utility (or, less utility *loss*) by choosing Right instead of Left.

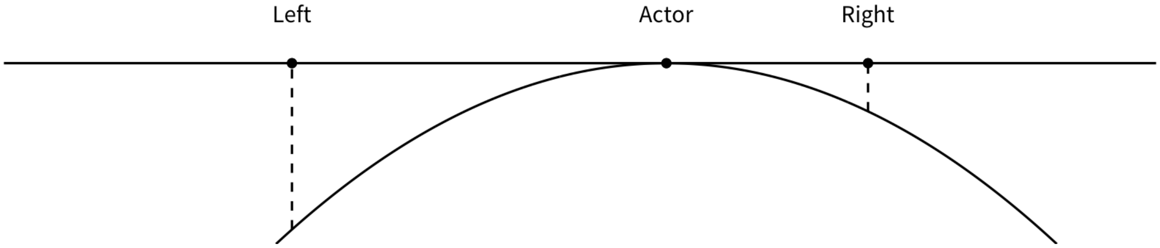


Figure 2.2: A representation of quadratic utility loss over policy choices

It is important to remember that Figure 2.2 shows only the deterministic component of choice task *j*; random error components u_{ij}^{R} and u_{ij}^{L} are omitted. With idiosyncratic utility error incorporated, Equation (2.2) implies that even though the Actor's distance to Right is smaller than her distance to Left, there remains a nonzero probability that i chooses Left. This probability depends on the instantiated values of the idiosyncratic error terms for each choice. These error terms represent the accumulation of several possible, non-ideological shocks to utility: systematic decision factors that are not summarized by ideology, issue-specific considerations that do not apply broadly across all issues, random misperceptions about the policy locations, and so on. Supposing that these idiosyncratic terms follow some probability distribution, Equation (2.2) can be represented probabilistically:

$$\begin{aligned}\Pr\left(y_{ij} = 1\right) &= \Pr\left(U_i\left(R_j\right) > U_i\left(L_j\right)\right) \\ &= \Pr\left(-\left(\theta_i - R_j\right)^2 + u_{ij}^{\mathsf{R}} > -\left(\theta_i - L_j\right)^2 + u_{ij}^{\mathsf{L}}\right) \\ &= \Pr\left(\left(\theta_i - L_j\right)^2 - \left(\theta_i - R_j\right)^2 > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right)\end{aligned} \tag{2.3}$$

The intuition for Equation (2.3) is that the Actor will choose the policy alternative that is nearest to her *unless* idiosyncratic or non-policy factors overcome her ideological considerations. Supposing that the Actor is closer to Right than to Left, $\left(\theta_i - L_j\right)^2$ will be greater than $\left(\theta_i - R_j\right)^2$, capturing the Actor's deterministic inclination to prefer Right over Left. The only way for i to choose Left would be if the idiosyncratic utility of Left over Right exceeded the Actor's deterministic inclinations.

Equation (2.3) can be rearranged to reveal an appealing functional form for i's choice probability. First, expand the polynomial terms on the left side of the inequality…

$$\begin{aligned}\Pr\left(y_{ij} = 1\right) &= \Pr\left(\left(\theta_i - L_j\right)^2 - \left(\theta_i - R_j\right)^2 > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right) \\ &= \Pr\left(\theta_i^2 - 2\theta_i L_j + L_j^2 - \theta_i^2 + 2\theta_i R_j - R_j^2 > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right) \\ &= \Pr\left(2\theta_i R_j - 2\theta_i L_j + L_j^2 - R_j^2 > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right)\end{aligned} \tag{2.4}$$

From here, there are two factorizations that reveal convenient expressions for important constructs in the model.

$$\begin{aligned}
\Pr\left(y_{ij} = 1\right) &= \Pr\left(2\theta_i R_j - 2\theta_i L_j + L_j^2 - R_j^2 > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right) \\
&= \Pr\left(2\theta_i R_j - 2\theta_i L_j + \left(R_j - L_j\right)\left(R_j + L_j\right) > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right) \\
&= \Pr\left(2\left(R_j - L_j\right)\left(\theta_i - \frac{R_j + L_j}{2}\right) > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right)
\end{aligned} \tag{2.5}$$

The first manipulation is to decompose $L_j^2 - R_j^2$ into the two factors $\left(R_j - L_j\right)\left(R_j + L_j\right)$. The second manipulation is to factor $2\left(R_j - L_j\right)$ out of the left-side of the inequality. We perform these manipulations because the resulting terms are appreciably more interpretable than before. First, note that $\frac{R_j+L_j}{2}$ is a formula for the midpoint between the Left and Right locations. This means that the expression $\theta_i - \frac{R_j+L_j}{2}$ intuitively conveys which policy alternative is closer to the Actor. If the Actor is closer to Right than to Left, θ_i will be greater than the midpoint, and vice versa if she were closer to Left. Second, the $2\left(R_j - L_j\right)$ term captures how far apart the policy alternatives are from one another, increasing as the distance between Right and Left increases. Together, the left side of the inequality succinctly describes the deterministic component of the Actor's ideological choice: is she closer to the Left or Right policy, and by how much?[2]

The final manipulation is to simplify the terms above, which results in a convenient parameterization for statistical estimation.

$$\begin{aligned}
\Pr\left(y_{ij} = 1\right) &= \Pr\left(2\left(R_j - L_j\right)\left(\theta_i - \frac{R_j + L_j}{2}\right) > u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right) \\
&= \Pr\left(\iota_j\left(\theta_i - \kappa_j\right) > \varepsilon_{ij}\right),
\end{aligned} \tag{2.6}$$

This results in the "discrimination parameter" $\iota_j = 2\left(R_j - L_j\right)$, the "midpoint" or "cutpoint" parameter $\kappa_j = \dfrac{R_j + L_j}{2}$, and a joint error term $\varepsilon_{ij} = u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}$.[3] Parameterizing the model in

[2] Ansolabehere, Snyder, and Stewart (2001) and Burden (2004) use candidate midpoints as predictors in regression analyses to estimate the impact of candidate ideal points in House elections.

[3] The names for these parameters are adapted from item-response theory (IRT), an area of psychometrics that is similarly interested in inferring latent traits from observed response data. I discuss the connection between this model and the IRT model in the next section.

this way expresses the utility comparison in a simpler, linear form. Similar to Equation (2.5) above, $\theta_i - \kappa_j$ shows how far the Actor is from the midpoint between Left and Right, and ι_j behaves as a "slope" on this distance: the distance from the midpoint has a *stronger influence* when the policy alternatives are farther from one another, since more utility is lost over larger spatial distances. I explore the intuitions of this functional form more thoroughly in the following section.

A complete statistical model is obtained by making a parametric assumption for the distribution of ε_{ij}. Assuming that ε_{ij} is a draw from a standard Normal distribution,[4] Equation (2.6) implies a probit regression model for the probability that Actor i chooses Right on choice j:

$$\begin{aligned}\Pr\left(y_{ij} = 1\right) &= \Pr\left(\iota_j\left(\theta_i - \kappa_j\right) > \varepsilon_{ij}\right) \\ &= \Pr\left(\iota_j\left(\theta_i - \kappa_j\right) - \varepsilon_{ij} > 0\right) \\ &= \Phi\left(\iota_j\left(\theta_i - \kappa_j\right)\right),\end{aligned} \tag{2.7}$$

where $\Phi(\cdot)$ is the cumulative Normal distribution function. Many IRT models assume that ε_{ij} follows a standard Logistic distribution, (for example Londregan 1999), resulting in a logistic regression model rather than a probit model.[5] As I show below, the probit model facilitates the group-level model much more easily than the logit model.

[4] This implies that $\mathrm{E}\left(u_{ij}^{\mathsf{L}}\right) = \mathrm{E}\left(u_{ij}^{\mathsf{R}}\right)$ and that $\mathrm{Var}\left(u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right) = 1$. For a given choice j, imposing a scale restriction on the error variance is not problematic because the ideological scale is latent and can be arbitrarily stretched. Any non-unit variance for item j can be compensated for by scaling the discrimination parameter ι_j (i.e. multiplying both sides of the inequality established in Equation (2.6) by some scale factor). The important assumption is that the error variance of a choice j is equal *across individuals*: $s_{ij} = s_j$ for all i.

[5] A technical point of difference between the probit and logit model is the way parameters are scaled to yield the final line of Equation (2.6). If it is assumed that ε_{ij} is a Logistic draw with scale s_j, this implies that $\mathrm{Var}\left(u_{ij}^{\mathsf{L}} - u_{ij}^{\mathsf{R}}\right) = \dfrac{s_j^2\pi^2}{3}$, where it is assumed that $s_j = 1$ for the standard Logistic model.

2.2 Item Response Theory

Scholars of ideal point models have noted their similarity to models developed under item response theory (IRT) in psychometrics (for example, Londregan 1999). IRT models have a similar mission as ideal point models: measuring latent features in the data given individuals' response patterns to various stimuli. The canonical psychometric example is in education testing, where a series of test questions is used to measure a student's latent academic "ability" level. This section connects ideal point models to IRT in order to explain their important theoretical and mathematical intuitions.

2.2.1 Latent Traits

The first important feature to note about IRT models is that they are *measurement models*. The goal of a measurement model is to use observed data $\mathbf{y}$ to estimate some construct of theoretical interest $\boldsymbol{\theta}$, supposing that there is a distinction between the two. The observed data $\mathbf{y}$ are affected by $\boldsymbol{\theta}$, but there is no guarantee of a one-to-one correspondence between the two because $\boldsymbol{\theta}$ is not directly observed. We can represent a measurement model with general notation $\mathbf{y} = f(\boldsymbol{\theta}, \boldsymbol{\sigma})$, where $\boldsymbol{\sigma}$ represents some vector of auxiliary model parameters to be estimated in addition to $\boldsymbol{\theta}$ by fitting the model to observed data.

In an educational testing context, students take standardized tests intended to measure their academic "ability" levels. Analysts who score the tests cannot observe a student's ability directly—it is unclear how that would be possible. They do, however, observe the student's answers to known test questions. IRT models provides a structure to infer abilities from the student's pattern of test answers. The context of policy choice is similar. It is impossible to observe any individual's political ideology directly, but we theorize that it affects their responses to survey items about policy choices. The IRT setup lets us summarize an individual's policy preferences by analyzing the structure of their responses to various policy choices.

It is crucial to note that the only way to estimate a latent construct from observed data is for the model to impose assumptions about the functional relationship between the latent construct and the observed data. In this sense, the estimates can be sensitive to the model's assumptions. While this is always important to acknowledge, it is also valuable to note that model-dependence is an ever-present consideration even for simpler measurement strategies, such as an additive index that sums or averages across a battery of variables. In fact, additive indices are special cases of measurement model where key parameters are assumed to be known and fixed, which is problematic if there is any reason to suspect that item responses are correlated across individuals.[6] In this way, measurement models *relax* the assumptions of simpler measurement strategies, even if the underlying mathematics are more intensive.

2.2.2 Item Characteristics and Item Parameters

Measurement models relax assumptions about the data's functional dependence on the construct of interest. Item response theory focuses this effort on the items to which subjects respond. Different items may reveal different information about the latent construct; the design of the model governs how those item differences can manifest (see Fox 2010 for a comprehensive review of IRT modeling).

Consider a simple model where a student i is more likely to answer test questions j correctly if she has greater academic ability θ_i. Analogously, a citizen who is more conservative is more likely to express conservative preferences for policy question j. Keeping the probit functional form from above, we can represent this simple model with the equation:

$$\Pr\left(y_{ij} = 1\right) = \Phi\left(\theta_i\right), \tag{2.8}$$

[6] Midpoint and discrimination parameters would be sources of this correlation. Additive indices are similar to a model where all all midpoint and discrimination are respectively equal to 0 and 1 by assumption.

where θ_i is scaled such that the probability of a correct/conservative response is 0.5 at $\theta_i = 0$. This model makes the implicit assumption that knowing θ_i is sufficient to produce exchangeable response data; there are no systematic differences in the difficulty level of the test questions or the ideological nature of the policy choices that would affect the propensity of subjects to answer correctly/conservatively on average. This implicit assumption is often unrealistic. Just as some test questions are naturally more difficult than others, some policy questions present more extreme or lopsided choices than others, leading citizens with otherwise equivalent θ values to vary systematically in their response probability across items. Although the "ability-only" model seems unrealistic when posed as such, political science is replete with additive measurement scales that omit all item-level variation: indices of policy views, the racial resentment scale, survey-based scales of political participation, and more.

Rather than assume that all items behave identically for all individuals, IRT explicitly models the systematic variation at the item level using *item parameters.* IRT models have different behaviors based on the parameterization of the item effects in the model. The simplest IRT model is the "one-parameter" model,[7] which includes an item-specific intercept κ_j.

$$\Pr\left(y_{ij} = 1\right) = \Phi\left(\theta_i - \kappa_j\right) \tag{2.9}$$

IRT parlance refers to the κ_j parameter as the item "difficulty" parameter. In the testing context, if a student has higher ability than the difficulty of the question, the probability that they answer the test item correctly is greater than 0.5. This probability goes up for students with greater ability relative to item difficulty, and it goes down for items with greater difficulty relative to student ability. In a policy choice context, the difficulty parameter is better understood as the "cutpoint" parameter, the midpoint between two policy choices where the respondent is indifferent between the choice of Left or Right on item j. These

[7] One-parameter logit models are often called "Rasch" models, whereas their corresponding probit models are often called "Normal Ogive" models (Fox 2010).

cutpoints are allowed to vary from item to item; some policy choices present alternatives that are, on average, more conservative or liberal than others. For instance, the choice of *how much* to cut capital gains taxes will have a more conservative cutpoint than a question of whether to cut capital gains taxes at all. If there were no systematic differences across items, it would be the case that $\kappa_j = 0$ for all j, and the one-parameter model would reduce to the simpler model in Equation (2.8).

The "two-parameter" IRT model is more common, especially in the ideal point context. The two-parameter model introduces the "discrimination" parameter ι_j, which behaves as a slope on the difference between θ_i and κ_j.

$$\Pr\left(y_{ij} = 1\right) = \Phi\left(\iota_j\left(\theta_i - \kappa_j\right)\right), \tag{2.10}$$

Intuitively, the discrimination parameter captures how well a test item differentiates between the responses of high- and low-ability students, with greater values meaning more divergence in responses. In the ideal point context, it captures how strongly a policy question divides liberal and conservative respondents.[8]

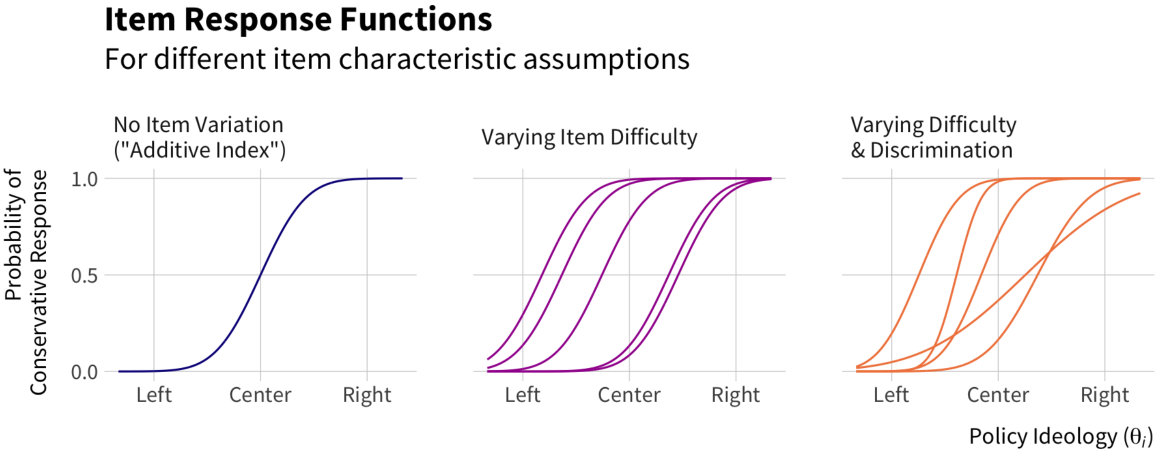


Figure 2.3: Examples of item characteristic curves under different item parameter assumptions

[8] Two-parameter IRT models are sometimes written with ι_j is distributed through the equation: $\iota_i\theta_i + \alpha_j$, where $\alpha_j = \iota_j\kappa_j$. Although this parameterization more closely follows a linear slope-intercept equation, it loses the appealing interpretation of κ_j as the midpoint between policy choices.

Figure 2.3 shows how response probabilities are affected by the parameterization of item effects. Each panel plots how increases in subject ability or conservatism (the horizontal axis) result in increased response probability (the vertical axis), where the shape of the curve is set by values of the item parameters. These curves are commonly referred to as *item characteristic curves* (ICCs) or *item response functions* (IRFs). The leftmost panel shows a model with no item effects whatsoever; any item is theorized to behave identically to any other item, and response probabilities are affected only by the subject's ability (ideology). The middle panel shows a one-parameter model where item difficulties (cutpoints) are allowed to vary systematically at the item level. Difficulty parameters behave as intercept shifts, so they convey which value of θ yields a correct response with probability 0.5, but they do not affect the *elasticity* of the item response function to changes in θ. The final panel shows item response functions from the two-parameter IRT model, where item difficulties (intercepts) and discriminations (slopes) are allowed to vary across items.

2.2.3 IRT Interpretation of the Ideal Point Model

How do we interpret our statistical model of ideal points in light of item response theory? Recall the statistical model that we derived from the utility model above. An Actor i faces policy question j, with a Right alternative located at R_j and a Left alternative located at L_j. The Actor chooses the alternative closest to her ideal point θ_i, subject to idiosyncratic utility shocks summarized by ε_{ij}. Letting y_{ij} indicate the outcome that Actor i chooses the Right position on policy question j, the probability that $y_{ij} = 1$ is given by the two-parameter model in Equation (2.10) (or (2.6) above).

The behavior of the item parameters can be understood by remembering that they are functions of the Left and Right choice locations. For instance, the cutpoint parameter κ_j represents an intercept shift for an items response function and is equal to $\frac{R_j + L_j}{2}$. Suppose that $\theta_i - \kappa_j = 0$, which occurs if the item cutpoint falls directly on an Actor's ideal point. In such a case, the Actor would be indifferent (in expectation) to the choice of Left or Right, and the probability of choosing Right would collapse to 0.5.[9] The value of κ_j increases by moving either the Right or Left alternatives to the right (increasing R_j or L_j), subject to the constraint that $R_j \geq L_j$. Larger values of the item cutpoint imply a lower probability that the Actor chooses Right, since κ_j has a non-positive effect on the conservative response probability.[10] The opposite intuition holds as the Left position becomes increasingly progressive, resulting in larger values of κ_j that imply a higher probability of choosing Right, all else equal.

The discrimination parameter behaves as a "coefficient" on the distance between the Actor ideal point and the cutpoint, meaning that the Actor's choice is more elastic to her policy preferences as ι_j increases.[11] Because $\iota_j = 2(R_j - L_j)$, the discrimination parameter grows when the distance between the Right and Left alternatives grows larger, which happens when R_j increases or L_j decreases.

In a special case that Right and Left alternatives are located in exactly the same location, the result is $\kappa_j = \iota_j = 0$, leading all Actors to choose Right with probability 0.5. This result represents a situation where policy preferences are not systematically related to the choice whatsoever, and only idiosyncratic error affects the choice of Right or Left. Although the model implies that this result is *mathematically* possible, it is not realistic to expect any of the policy choices in this project to induce this behavior.

2.2.4 IRT in Political Science

A section reviewing IRT in political science:

[9] This holds in logit and probit models, since $\text{logit}^{-1}(0)$ and $\Phi(0)$ are both equal to 0.5.

[10] Formally we can show this by taking the derivative of the link function with respect to the cutpoint: $\frac{\partial \iota_j \left(\theta_i - \kappa_j\right)}{\partial \kappa_j} = -\iota_j$, where ι_j is constrained to be greater than or equal to zero

[11] Again we can demonstrate this by noticing that the derivative of the link function with respect to the discrimination parameter is $\frac{\partial \iota_j \left(\theta_i - \kappa_j\right)}{\partial \iota_j} = \left(\theta_i - \kappa_j\right)$. The derivative's magnitude depends on the absolute value of this distance, and its sign depends on the sign of the difference.

- ideal points (Poole and Rosenthal, CJR, Londregan, Jeff Lewis, Michael Bailey, Martin and Quinn)
- citizen ideology with survey data (Seth Hill multinomial and individual, Tausanovitch and Warshaw small area, Caughey and Warshaw groups within areas)
- other latent modeling (Levendusky, Pope, and Jackman 2008)

2.3 Modeling Party-Public Ideology in Congressional Districts

This section outlines my group-level ideal point model for party publics. It begins by describing the connection between the individual-level IRT model and the group-level model and its implication for the parameterization of the model. I then lay out the hierarchical model for party-public ideal points in its static form (Section 2.3.1) and its dynamic form (Section 2.4). I discuss technical features of model implementation, including choices for model parameterization, model identification, prior distributions, and model testing methods such as prior predictive checks and posterior predictive checks.

So far we have modeled individual responses to policy items according to their own individual ideal points, but this project is concerned with the average ideal point of a *group* of individuals. In the group model, we assume that individual ideal points are distributed within a group g, where groups are define as the intersection of congressional districts d and political party affiliations p.

As before, we observe a binary response from individual i to item j, which we regard as a probabilistic conservative response with probability π_{ij}, which is given a probit model.

$$y_{ij} \sim \text{Bernoulli}(\pi_{ij}) \tag{2.11}$$

$$\pi_{ij} = \Phi\left(\iota_j\left(\theta_i - \kappa_j\right)\right) \tag{2.12}$$

Following Fox (2010) and Caughey and Warshaw (2015), it is helpful to reparameterize the IRT model to accommodate a group-level extension. This parameterization replaces item "discrimination" with item "dispersion" using the parameter $\sigma_j = \iota_j^{-1}$ and rewriting the model as

$$\pi_{ij} = \Phi\left(\frac{\theta_i - \kappa_j}{\sigma_j}\right). \tag{2.13}$$

Caughey and Warshaw (2015) describe the dispersion parameter as introducing "measurement error" in π_{ij} beyond the standard Normal utility error from ε_{ij} above.

The group model begins with the notion that there is a probability distribution of ideal points within a group g, where a "group" is a partisan constituency within a congressional district. Supposing that individual deviations from the group mean are realized by the accumulation of a large number of random forces, we can represent an individual ideal point as a Normal draw from the group,[12]

$$\theta_i \sim \text{Normal}\left(\bar{\theta}_{g[i]}, \sigma_{g[i]}\right) \tag{2.14}$$

where $\bar{\theta}_{g[i]}$ and $\sigma_{g[i]}$ are the mean and standard deviation of ideal points within i's group g.

While it is possible to continue building the model hierarchically from (2.14), it would be far too computationally expensive to estimate every individual's ideal point in additional to the group-level parameters—every individual ideal point is essentially a nuisance parameter. Instead, we rewrite the model by aggregating individual-level survey response data to the group level, expressing the grouped outcome data as a function of the group parameters. Let $s_{gj} = \sum_{i \in g}^{n_{gj}} y_{ij}$, the number of conservative responses from group g to item j, where n_{gj} is the total number of responses (trials) to item j by members of group g. Supposing these trials were collected independently across groups and items (an assumption that is relaxed later),

[12] Notation for Normal distributions will always describe the scale parameter in terms of standard deviation σ instead of variance σ^2. This keeps the notation consistent with the way Normal distributions are expressed in Stan code.

we could model the grouped outcome as a binomial random variable,

$$\begin{aligned} s_{gj} &\sim \text{Binomial}\left(n_{gj}, \bar{\pi}_{gj}\right) \\ \bar{\pi}_{gj} &= \Phi\left(\frac{\bar{\theta}_g - \kappa_j}{\sqrt{\sigma_g^2 + \sigma_j^2}}\right), \end{aligned} \tag{2.15}$$

where $\bar{\pi}_{gj}$ is the "average" conservative response probability for item j in group g, or the probability that a randomly selected individual from group g gives a conservative response to item j. Our uncertainty about any random individual's ideal point relative to the group mean is included in the model as group-level variance term. If individual ideal points are Normal within their group, this within-group variance can simply be added to the probit model as another source of measurement error, with larger within-group variances further attenuating $\bar{\pi}_{ij}$ toward 0.5. Caughey and Warshaw (2015) derive this result in the supplementary appendix to their article.

The current setup assumes that every item response is independent, conditional on the group and the item. This assumption is violated if the same individuals in a group answer multiple items—one individual who answers 20 items is less informative about the group average than 20 individuals who answer one item apiece. While this too could be addressed by explicitly modeling each individual's ideal point (extending the model directly from Equation (2.14)), I implement a weighting routine that downweights information from repeated-subject observations while adjusting for nonrepresentative sample design, as I will describe in Section 2.4.2.

2.3.1 Hierarchical Model for Group Parameters

The group model described so far can be estimated straightforwardly if there are enough responses from enough individuals in enough district-party groups. In practice, however, a single survey will not contain a representative sample of all congressional districts, and certainty not a representative sample of partisans-within-districts. I specify a hierarchical model for the group parameters in order to stabilize the estimates in a principled way. The hierarchical model learns how group ideal points are related to cross-sectional (and eventually, over-time) variation in key covariates, borrowing strength from data-rich groups to stabilize estimates for data-sparse groups, and even imputing estimates for groups with no survey data at all. This section describes the multilevel structure using traditional notation for hierarchical models; later in Section 2.5 I describe how I parameterize the model for the estimation routine.

I posit a hierarchical structure where groups g are "cross-classified" within districts d and parties p. This means that groups are nested within districts and within parties, but districts and parties have no nesting relationship to one another. Districts are further nested within states s. I represent this notationally by referring to group g's district as $d[g]$, or the g^{th} value of the vector $\mathbf{d}$. Similarly, g's party is $p[g]$. For higher levels such as g's state, I write $s[g]$ as shorthand for the more-specific but more-tedious $s[d[g]]$.

I use this hierarchical structure to model the probability distribution of group ideal points $\bar{\theta}_g$. I consider the group ideal point as a Normal draw from the distribution of groups whose hypermean is predicted by a regression on geographic-level data with parameters that are indexed by political party. This regression takes the form

$$\bar{\theta}_g \sim \text{Normal}\left(\mu_{p[g]} + \mathbf{x}_{d[g]}^{\top}\beta_{p[g]} + \alpha_{s[g]p[g]}^{\texttt{state}}, \sigma_p^{\texttt{group}}\right) \tag{2.16}$$

where $\mu_{p[g]}$ is a constant specific to party p,[13] $\mathbf{x}_d$ is a vector of congressional district-level covariates with party-specific coefficients β_p. State effects $\alpha_{sp}^{\texttt{state}}$ are also specific to each party. The benefit of specifying separate parameters for each party is that geographic features (such as racial composition, income inequality, and so on) may be related to ideology in ways that are not identical across all parties. This is an important departure from the structure laid out

[13] Or "grand mean," since all covariates are eventually be centered at their means.

by Caughey and Warshaw (2015), which estimates the same set of geographic effects for all groups in the data.

The state effects are regressions on state features as well,

$$\alpha_{sp}^{\texttt{state}} \sim \text{Normal}\left(\mathbf{z}_s^\top \gamma_p + \alpha_{r[s]p}^{\texttt{region}}, \sigma_p^{\texttt{state}}\right), \tag{2.17}$$

where state-level covariates $\mathbf{z}_s$ have party-specific coefficients γ_p. Each state effect is a function of a party-specific region effect $\alpha_{[s]rp}^{\texttt{region}}$ for Census regions indexed r, which is a modeled mean-zero effect to capture correlation within regions.

$$\alpha_{rp}^{\texttt{region}} \sim \text{Normal}\left(0, \sigma_p^{\texttt{region}}\right) \tag{2.18}$$

2.4 Dynamic Model

lol tbd

2.4.1 Identification Restrictions

Ideal point models, as with all latent space models, are unidentified without restrictions on the policy space. The model as written can rationalize many possible estimates for the unknown parameters, with no prior basis for deciding which estimates are best. A two-parameter model such as this requires some restriction on the polarity, location, and scale of the policy space.

- Location: the latent scale can be arbitrarily shifted right or left. We could add some constant to every ideal point, and the response probability would be unaffected if we also add the same constant to every item cutpoint.

- Scale: the latent scale can be arbitrarily stretched or compressed. We could multiply the latent space by some scale factor, and the response probability would be unaffected if we also multiply the discrimination parameter by the inverse scale factor.

- Polarity: the latent scale could be reversed. We could flip the sign of every ideal point, and the response probability would be unaffected if we also flip the sign of every item parameter.

These properties are present with every statistical model, but covariate data typically provide the restrictions necessary to identify a model.[14] Because the response probability is a function of the interaction of multiple parameters in a latent space, however, data alone do not provide the necessary restrictions on the space to provide a unique solution. Absent any natural restriction from the data, I provide my own restrictions on the polarity, location, and scale of the policy space.

The polarity of the space is fixed by coding all items such that conservative responses are 1 and liberal responses are 0. This ensures that increasing values on the link scale always lead to an increasing probability of a *conservative* item response. Additionally I impose a restriction that all discrimination parameters are positive, which implies that shifting any ideal point farther to the *right* of an item cutpoint increases the probability of a conservative response, all else equal.

The location of the space is set by restricting the sum of the J item cutpoints to be 0. If $\tilde{\kappa}_j$ were an unrestricted item cutpoint, the restricted cutpoint κ_j used in response model would be defined as

$$\kappa_j = \tilde{\kappa}_j - \frac{\sum\limits_{j=1}^{J} \tilde{\kappa}_j}{J}, \tag{2.19}$$

which is performed in every iteration of the sampler. This restriction on the sum of the cutpoint parameters also implies a restriction on the mean of the cutpoints, since $\frac{0}{J} = 0$.

Lastly, I set the scale of the latent space by restricting the product of the J discrimination parameters to be equal to 1. I implement this by restricting the log discrimination parameter to

[14] We could imagine shifting, stretching, or reversing the sign of a covariate to reveal the same mathematical behaviors. All of these transformations would result in the same predictions as long as the parameters are also transformed to compensate.

have a sum of 0, which achieves an equivalent transformation.[15] Letting $\tilde{\iota}_j$ be the unrestricted discrimination parameter, we obtain the restricted ι_j as follows.

$$\iota_j = \exp\left(\log\left(\iota_j\right)\right) \tag{2.20}$$

$$\log\left(\iota_j\right) = \log\left(\tilde{\iota}_j\right) - \frac{1}{J}\sum_{j=1}^{J}\log\left(\tilde{\iota}_j\right) \tag{2.21}$$

Item discrimination is then reparameterized as dispersion, $\sigma_j = \iota_j^{-1}$. These restrictions on the item parameters are sufficient to identify $\bar{\theta}_g$.

2.4.2 Weighted Outcome Data

The group-level model learns about group ideal points by surveying individuals within groups, but the model currently assumes that all y_{gj} are independent conditional on the item. If the same individuals answer multiple items, this assumption is violated. Additionally, we cannot assume that responses are independent in the presence of nonrepresentative survey designs. This section describes an approach for weighting group-level data that adjusts for both issues. The corrections are lifted from Caughey and Warshaw (2015) with slight modifications.

First, the sample size in each group-item "cell" gj must is adjusted for survey design and multiple responses per individual. Let $n^*_{g[i]j}$ be the adjusted sample size for i's group-item cell, defined as

$$n^*_{g[i]j} = \sum_{i=1}^{n_{g[i]j}} \frac{1}{r_i d_{g[i]}}, \tag{2.22}$$

where r_i is the number of responses given by individual i, and $d_{g[i]}$ is a survey design correction for i's group. The effective sample size decreases when respondents answer multiple questions ($r_i > 1$) or in the presence of a sample design correction ($d_g > 1$). The design correction, originally specified by Ghitza and Gelman (2013), penalizes information collected

[15] A quick demonstration using three unknown values a, b, and c. If $a \times b \times c = 1$, then $\log(a) + \log(b) + \log(c) = \log(1)$, which is equal to 0.

from groups that contain greater variation in their survey design weights. It is defined as

$$d_{g[i]} = 1 + \left(\frac{\text{sd}_{g[i]}\left(w_i\right)}{\text{mean}_{g[i]}\left(w_i\right)} \right)^2, \tag{2.23}$$

where sd(·) and mean(·) are the within-group standard deviation and mean of respondent weights w_i. If all weights within a cell are identical, their standard deviation will be 0, resulting in a design correction equal to 1 (meaning, no correction). Larger within-cell variation in weights increases the value of d_g, thus decreasing the effective sample size within a cell. The intuition of this correction is to account for increased variance of weighted statistics compared to unweighted statistics, given a fixed number of observations (Ghitza and Gelman 2013, 765).

To obtain the weighted number of successes in cell gj, I multiply the cell's weighted sample size by its weighted mean. The weighted mean $\bar{y}^*_{gj}$ adjusts for the respondent's survey weight w_i and number of responses r_i and is defined as

$$\bar{y}^*_{g[i]j} = \frac{\sum\limits_{i=1}^{n_{g[i]j}} \frac{w_i y_{ij}}{r_i}}{\sum\limits_{i=1}^{n_{g[i]j}} \frac{w_i}{r_i}}. \tag{2.24}$$

The weighted number of successes in each cell, in turn, is

$$s^*_{gj} = \min\left(n^*_{gj}\bar{y}^*_{gj}, n^*_{gj}\right). \tag{2.25}$$

where I take the minimum to ensure that the number of successes does not exceed the adjusted sample size.

It is likely that many values of n^*_{gj} and s^*_{gj} will be non-integers. Ordinarily this would be a problem for modeling a Binomial random variable, since a Binomial is an integer-valued count of successes given an integer number of trials and a success probability. For this reason, many statistical programs will return an error if a floating-point argument is bassed to the Binomial probability mass function. Whereas Caughey and Warshaw (2015) calculate

$\lceil n^*_{gj} \rceil$ and $\lfloor s^*_{gj} \rfloor$ to obtain integer data for estimation, I instead implement a custom Binomial quasi-likelihood function that returns log probabilities for weighted data (see Section 2.5.2).

2.5 Bayesian Estimation and Computation

I implement the model using Stan, a programming language for high-performance Bayesian analysis that extends and interfaces with `C++` (Carpenter et al. 2016). Stan implements an adaptive variant of Hamiltonian Monte Carlo (HMC), an algorithm that efficiently collects posterior samples by "surfing" a Markov proposal trajectory along the gradient of the posterior distribution. Because the algorithm uses the posterior gradient to generate proposals, the algorithm concentrates proposals in regions of high transition probability and performs better in high dimensions than conventional Gibbs sampling algorithms. Although it possible to estimate Stan models using front-end software packages such as `brms` for R (Bürkner and others 2017), complicated models must be programmed with raw Stan code, which can be intensive. This section describes instances where the model *as programmed in Stan* departs from the model *as written* above. Although these alterations do not change the statistical intuitions of the model, they are essential for the model's computational stability by protecting against biased MCMC estimation and floating point arithmetic errors. These contributions should be highlighted here because they are crucial to ensuring valid inferences and are substantive improvements on previous software implementations of group-level IRT models.

2.5.1 Non-Centered Parameterization

Hierarchical models have posterior distributions whose curvatures present difficulties for sampling algorithms (Betancourt and Girolami 2015; Papaspiliopoulos, Roberts, and Sköld 2007). To improve the estimation in Stan, I program the hierarchical models using a "non-centered" parameterization rather than a "centered" parameterization. Whereas the centered parameterization considers $\bar{\theta}_g$ as a random draw from a hierarchical distribution (Equation (2.16) above), the non-centered parameterization defines $\bar{\theta}_g$ as a deterministic function of its conditional hypermean and a random variable.

$$\bar{\theta}_g = \mu_{p[g]} + \mathbf{x}_{d[g]}^{\top}\beta_{p[g]} + \alpha_{s[g]p[g]}^{\text{state}} + u_g\tau_{p[g]}, \tag{2.26}$$

$$u_g \sim \text{Normal}\left(0, 1\right) \tag{2.27}$$

where $u_g\tau_{p[g]}$ behaves as a group-level error term. It is composed of a standard Normal variate u_g and a scalar parameter τ_p that controls the scale of the error term. The non-centered model is algebraically equivalent to centered model in the likelihood, but it factors out (or "unnests") the location and the scale from the hyperprior. The non-centered parameterization improves MCMC sampling by de-correlating the parameters that compose the hierarchical distribution. Hierarchical models using a centered parameterization, on the other hand, are vulnerable to estimation biases due to poor posterior exploration (Betancourt and Girolami 2015).[16] This is a crucial extension to the estimation approach developed by Caughey and Warshaw (2015), whose model implements all hierarchical components using the centered parameterization.

Equation (2.27) is an incomplete implementation of the non-centered form; to complete the parameterization, I apply it too all hierarchical components in the regression, including

[16] Stan's HMC algorithm is programmed to diagnose poor posterior exploration by detecting "divergent transitions" during sampling. Because Stan's HMC algorithm uses the gradient of the posterior distribution to propose efficient transition trajectories through the parameter space, it adaptively builds expectations about the probability density of the next Markov state. Areas of high curvature in the posterior gradient can lead to "divergences" in the HMC algorithm: transitions where the log density of a state differs substantially from what Stan anticipated when it proposed the transition. Markov chains with many divergent transitions have a high risk of being severely biased, since the divergences indicate that the Markov chain is failing to efficiently navigate the parameter space (Betancourt and Girolami 2015). The non-centered parameterization smooths out these problematic regions of posterior density, safeguarding against biased MCMC estimates.

the state and region effects.

$$\begin{aligned}\bar{\theta}_g = \mu_{p[g]} &+ \mathbf{x}_{d[g]}^{\top}\beta_{p[g]} + u_g^{\texttt{group}}\tau_{p[g]}^{\texttt{group}} \\ &+ \mathbf{z}_{s[g]}^{\top}\gamma_p + u_{s[g]p[g]}^{\texttt{state}}\tau_{p[s]}^{\texttt{state}} \\ &+ u_{r[g]p[g]}^{\texttt{region}}\tau_{p[g]}^{\texttt{region}}\end{aligned} \tag{2.28}$$

The full model places the hypermean regressions and error terms for groups, states, and regions in one deterministic equation. It contains "error terms" for each level of hierarchy—groups, states, and regions—where all parameters are indexed by party.

2.5.2 Log Likelihood for Weighted Data

One important implementation consideration for the group IRT model is the presence of weighted, non-integer response data. As described in Section 2.4.2, grouped data require reweighting to account for nonrepresentative sample designs and repeated observations within individual members of a group. The resulting data are likely to take non-integer values, which would cause the built-in Binomial likelihood function to fail. Whereas Caughey and Warshaw (2015) round their data to conform to Stan's Binomial likelihood function, my approach rewrites the likelihood function to accept non-integer data. This allows me to maintain the precision in the underlying data while still correcting the issue at hand.

To explain how this works in Stan, some context on Bayesian computation is helpful. It is usually sufficient for Bayesian estimation with Markov chain Monte Carlo to calculate the posterior density of model parameters only up to a proportionality constant,

$$p(\Theta \mid \mathbf{y}) \propto p(\mathbf{y} \mid \Theta)p(\Theta), \tag{2.29}$$

where Θ and $\mathbf{y}$ generically represent parameters and observed data. For computational stability, especially in high dimensions where probability densities get very small, these calculations are done on the log scale.

$$\log p(\Theta \mid \mathbf{y}) \propto \log p(\mathbf{y} \mid \Theta) + \log p(\Theta), \tag{2.30}$$

MCMC algorithms calculate the right-side of this proportionality at each iteration of the sampler to decide if proposed parameters should be accepted into the sample or rejected. In Stan, this calculation is passed to the *log density accumulator*, a variable containing the sum of the log likelihood and log prior density at every sampler iteration (Carpenter et al. 2016).

In the current case, it is the probability of the data $\log p(\mathbf{y} \mid \Theta)$ that presents a problem. The Binomial log likelihood function as written in Stan will not accept non-integer data, so I rewrite the kernel $K(\cdot)$ of the Binomial log likelihood:

$$\log K\left(p\left(s^*_{gj} \mid \bar{\pi}_{gj}\right)\right) = s^*_{gj} \log \bar{\pi}_{gj} + \left(n^*_{gj} - s^*_{gj}\right) \log\left(1 - \bar{\pi}_{gj}\right) \tag{2.31}$$

where the weighted number of trials n^*_{gj} and the weighted number of successes s^*_{gj} can take non-integer values. This is the same approach that Ghitza and Gelman (2013) take in a frequentist maximum likelihood context.[17] I pass the results of Equation (2.31) directly to the log density accumulator. This maneuver accumulates the log probability of the response data, which Stan uses to evaluate the acceptance probability of MCMC samples. Other sampling statements that add prior density to the accumulator are unaffected by my approach to the log likelihood.

2.5.3 Optimized IRT Model

The matrix expansion trick would go here, if I did it.

[17] They describe this as “simply the weighted log likelihood approach to fitting generalized linear models with weighted data” (Ghitza and Gelman 2013, 765).

2.5.4 Prior Distribution

The Bayesian modeling paradigm requires a prior probability distribution over the model parameters, which can be a benefit and a drawback of the approach. The primary benefit is the ability to encode external information into a model. This enables the researcher to stabilize parameter estimates and downweight unreasonable estimates, enabling the researcher to guard against overfitting and smooth estimates from similar groups. This is especially valuable in data-sparse settings where parameters are unidentified or weakly identified, such as in hierarchical models where some groups contain more data than others (Gelman and Hill 2006). Bayesian estimation has fundamental computational advantages for ideal point models, since MCMC generates posterior samples of latent variables just as it would for any other model parameter. This allows the researcher to escape certain pathologies of optimization algorithms in high-dimensional parameter spaces with many incidental nuisance parameters (Lancaster 2000, @clinton-jackman-rivers:2004:ideal). The drawback of Bayesian modeling is that prior specification is additional work for the researcher, which can be complicated especially in situations where a model is sensitive to the choice of prior.

This section provides describes and justifies priors used in the group ideal point model. The discussion here is more detailed than in a typical paper describing a Bayesian ideal point model for several reasons. Firstly, the norms of the typical Bayesian workflow are evolving toward more rigorous checking of prior distributions and their implications (Betancourt 2018; Gabry et al. 2019). These prior checks allow researchers to explore and demonstrate the consequences of their prior choices in transparent ways, but most Bayesian analyses in political science lack these explicit prior checks. Authors often declare their prior choices without explicitly justifying these choices, which can make prior specification feel opaque or even arbitrary to non-Bayesian readers. Secondly, and more specifically to this project, the nonlinearities introduced by a probit model present particular challenges for specifying priors. Some of my choices depart from those in previous Bayesian ideal point models for important theoretical and practical reasons that I explain below. Thirdly, model parameterization is important for effective Bayesian computation (see Section 2.5.1), and although reparameterization does not affect the likelihood of data given the parameters, parameterization naturally affects the choice of priors. This exploration of priors is a crucial component of the model-building process for this project and is uncommon in other Bayesian works in political science, so it is important to justify these choices with sufficient detail.

Before discussing priors for the group ideal point model, it is helpful to discuss some general principles for working with prior distributions. They are not *universal* principles, but they are *theoretical* in the sense that they provide a pre-data orientation for prior distributions. They are heuristic principals in the sense that they provide powerful shortcuts to good analysis decisions based on lightweight signals about the problem at hand.[18]

This discussion of priors begins with the orientation laid out by Gelman, Simpson, and Betancourt (2017) that "the prior can often only be understood in the context of the likelihood." Although prior information is generally regarded as information that a researcher has before encountering data—and therefore before making any modeling decisions—in practice it is often the case that priors are chosen with reference to a specific analysis model. For example, we may have prior expectations about the proportion of Republicans who express conservative preferences on a given policy question (e.g. that the proportion is most likely above 50 percent), but if we model the proportion with a probit model, we typically specify priors on regression coefficients rather than the proportion parameter itself. This means that researchers must consider their priors as embedded in the specific data model at hand. This further implies that the *parameterization* of a model can affect the researcher's prior for some ultimate quantity of interest, even if the parameterization does not affect the likelihood of

[18] Gelman (2017) holds that theoretical statistics ought to be the "theory of applied statistics," in the sense that statistical theory ought to be informed by "what we actually do" and should thus work to formalize aspects of workflow that begin merely as "good practice."

the data given the parameters(Gelman 2004). The consequences of model parameterization are explored further in Chapter 3.

- weak information
    - between structural and regularizing. (gabry et al?)
    - They achieve regularization by encoding structural information, plus information befitting a *class* of problems. Short of information tailored to a *specific* problem
- families of priors
    - less reliance on the actual prior param values
    - features of a CDF
    - entropy in the distribution
    - shape of the log probability (normal vs T)
- prediction-focused (gelman et al prior/likelihood)

Subjective v. objective

- the actual degree of belief is zero
- not really the issue
- priors provide practical stability

General thoughts about priors

- WIPS and the evolution of thinking about this
    - likelihood is important weak information
    - constraining values to what is reasonable
    - but not so informed that we're downweighting reasonable values unless when context demands

        * sometimes it does, like identifiability, strong regularization/separation

- parameterization
    - normal(a, b) or a + bu, u ~ normal(0, 1)

Throughout this discussion is a common underpinning that the data model itself provides important structure for choosing priors. This is a pragmatic view. While many discussions of priors focus on the fact that they are philosophically essential for posterior inference, this pragmatic view emphasizes their practical implications for regularization, model stabilization, and computational efficiency and tractability.

2.5.5 Understanding the Probit Model

- priors and likelihoods
    - probit is a nonlinear model
    - data aren't additively related to y, but some other thing
    - the prior is specified in reference to some likelihood

How the probit model works

- there is a latent scale
- covariates typically have linear, additive effects
- error is assumed Normal
- the latent scale is identified as having location zero and scale 1
- this means the predicted probability of a 1 is the probability that the index > 0, which is equivalent to the normal CDF at the predicted index value
- coefficients are uncertain, so uncertainty in posterior data are owed to probabilistic uncertainty about y given an index value, and baseline uncertainty about location on the index given covariates

How Bayesian modeling with probit works:

- because we know the Normal distribution, we know the region of quantile space that reasonably produces our outcome data
- combination of data and parameters shouldn't realistically lead us beyond the quantiles that produce probabilities between 1% and 99% (justify)
- it isn't crazy that some of our predictions are highly determined, so we don't want to be too restrictive, but broadly speaking, a priori, you know (justify better)

Divergence from past work

- Vague priors:
  - Clinton, Jackman, Rivers
  - Treier and Hillygus
  - Tausanovitch and Warshaw
- Caughey and Warshaw
  - noncentering
  - lognormal parameterization (enable pooling by leveraging transformed parameters)

2.5.6 Item Parameters

I specify priors on the unscaled cutpoint and discrimination parameters that are Normal and LogNormal, respectively. In order to model their joint distribution, I specify a multivariate Normal distribution for the cutpoint and logged discrimination parameter,

$$\begin{bmatrix} \tilde{\kappa}_j \\ \log\left(\tilde{\iota}_j\right) \end{bmatrix} \sim \text{Normal}\left(\boldsymbol{\mu}, \boldsymbol{\Sigma}\right) \tag{2.32}$$

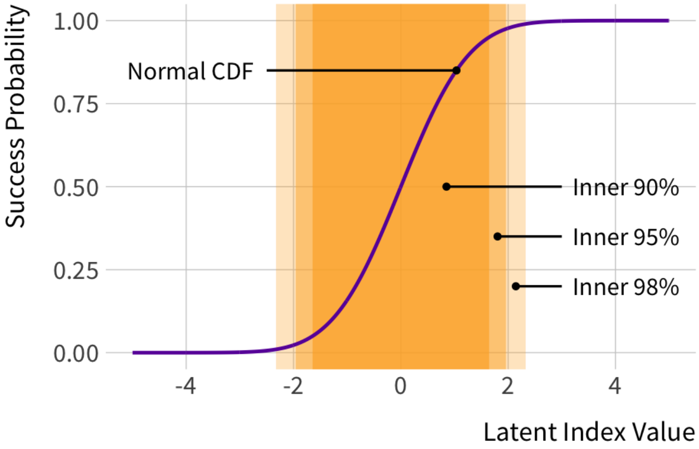


Figure 2.4: The region of the probit model's latent index that maps to response probabilities between 1 and 99 percent.

where $\boldsymbol{\mu}$ is a 2-vector of means and $\boldsymbol{\Sigma}$ is a 2×2 variance–covariance matrix. Whereas Caughey and Warshaw (2015) specify independent priors for all item cutpoint and discrimination parameters separately, my hierarchical model partially pools the item parameters toward a common distribution. This allows estimates to borrow precision from one another rather than "forgetting" the information learned from one item when updating the prior for the next item. The discrimination parameter, which has a product of 1 when scaled, is logged so that it has a mean of 0 on the log scale. This simplifies the prior specification of the mean vector $\boldsymbol{\mu}$, which is a standard multivariate Normal with no off-diagonal elements.[19]

$$\boldsymbol{\mu} \sim \text{Normal}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & 0 \\ 0 & 1 \end{bmatrix}\right) \tag{2.33}$$

I build a prior for the variance–covariance matrix $\boldsymbol{\Sigma}$ by decomposing it into a diagonal

[19] Although I use a joint prior, the assumptions about the parameters' marginal distributions are similar to Caughey and Warshaw (2015). Their choice to restrict discrimination parameter to have a product of 1 and a LogNormal distribution is identical to my choice to restrict log discrimination parameters to have a sum of 0 and a Normal prior. The benefit of my parameterization is that, by specifying the Normal family directly on the logged discrimination parameter, it is much simpler to build the joint hierarchical prior for all item parameters simultaneously.

matrix of scale terms and a correlation matrix. First I factor out the scale components.

$$\boldsymbol{\Sigma} = \begin{bmatrix} \sigma_{\tilde{\kappa}}^2 & \rho\sigma_{\tilde{\kappa}}\sigma_{\tilde{\iota}} \\ \rho\sigma_{\tilde{\kappa}}\sigma_{\tilde{\iota}} & \sigma_{\tilde{\iota}}^2 \end{bmatrix} \tag{2.34}$$

$$= \begin{bmatrix} \sigma_{\tilde{\kappa}} & 0 \\ 0 & \sigma_{\tilde{\iota}} \end{bmatrix} \boldsymbol{S} \begin{bmatrix} \sigma_{\tilde{\kappa}} & 0 \\ 0 & \sigma_{\tilde{\iota}} \end{bmatrix} \tag{2.35}$$

The resulting matrix $\boldsymbol{S}$ is a 2 × 2 correlation matrix, meaning it has a unit diagonal and off-diagonal correlation terms (denoted ρ).

$$\boldsymbol{S} = \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \tag{2.36}$$

I then specify priors for the scale terms, $\sigma_{\tilde{\kappa}}$ and $\sigma_{\tilde{\iota}}$, and the correlation matrix $\boldsymbol{S}$ separately. This approach is also known as a "separation strategy" for covariance matrix priors (Barnard, McCulloch, and Meng 2000).[20] The scale terms $\sigma_{\tilde{\kappa}}$ and $\sigma_{\tilde{\iota}}$ are given weakly informative Half-Normal$\,(0, 1)$ priors, which provide weak regularization toward zero but whose scale is wide enough that the data are likely to dominate the prior. I give $\boldsymbol{S}$ a prior from the LKJ distribution, which is a generalization of the Beta distribution defined over the space of symmetric, positive-definite, unit-diagonal matrices, such as a correlation matrix (Lewandowski, Kurowicka, and Joe 2009).[21]

$$\boldsymbol{S} \sim \text{LKJcorr}(\eta = 2) \tag{2.37}$$

[20] Although inverse-Wishart priors are often chosen for covariance matrices because they ensure conjugacy of the multivariate Normal distribution, recent work by Bayesian statisticians suggests that the separation strategy for covariance matrices is superior. The inverse-Wishart distribution has certain restrictive properties such as prior dependency between scales and correlations (large and small scales imply large and small correlations, respectively) that many Bayesian statisticians find undesirable compared to priors specified using the more flexible separation strategy (Akinc and Vandebroek 2018; Alvarez, Niemi, and Simpson 2014). Furthermore, the conjugacy of the inverse-Wishart is irrelevant for this model because conjugacy does not provide the same computational benefit for Hamiltonian Monte Carlo samplers as it does for Gibbs samplers or analytic posterior computation.

[21] For a matrix $\boldsymbol{S}$ that follows an LKJ distribution with shape parameter η, the density of $\boldsymbol{S}$ is a function of its determinant: $\text{LKJcorr}(\boldsymbol{S} \mid \eta) = c \times \det(\boldsymbol{S})^{\eta - 1}$ with proportionality constant c that depends on the dimensionality of $\boldsymbol{S}$.

The LKJ distribution has one shape parameter η, which can be interpreted like a shape parameter for a symmetric Beta distribution. Setting $\eta = 1$ yields a flat prior over all correlation matrices, where increasing values of $\eta 1$ concentrate prior density toward the mode, which is an identity matrix. The chosen value of $\eta = 2$ provides weak regularization against extreme correlations near -1 and $+1$. Although it would have been sufficient to specify a prior for ρ instead of the entire matrix $\boldsymbol{S}$, this convenience only arises in small (in this case, 2×2) correlation matrices. Larger matrices (such as those that would result from a more complex IRT model specification) would require explicit priors for a larger number of off-diagonal parameters. The LKJ prior can be generally applied to larger correlation matrices, so I choose it for the sake of building a more flexible and extensible model.

Figure 2.5 plots several details of the item prior. The top row shows the prior densities for the terms in the decomposed variance–covariance matrix $\boldsymbol{\Sigma}$. The left panel shows the Half-Normal prior density for the scale terms. The right panel shows the marginal distribution of ρ, the off-diagonal parameter in the matrix $\boldsymbol{S}$ that controls the covariance of items in the joint prior, generated from the LKJ correlation matrix prior. The bottom panel shows the distribution of item parameter values simulated from the multivariate Normal distribution implied by the joint hierarchical prior. Each point represents a simulated item as a combination of cutpoint values (on the horizontal axis) and log-discrimination values (on the vertical axis). Points are colored according to the number of nearby points, which informally conveys the prior density of items with particular cutpoint and discrimination values.

2.5.7 Ideal Point Parameters

For the hierarchical model that smooths group estimates, the model is parameterized to ease the specification of priors. First I standardize all covariates to have a mean of zero. This ensures that the constant μ_p in the hierarchical model for $\bar{\theta}_g$ (See Equation (2.16)) can be interpreted as a "grand mean" for party p, the average group ideal point for party p when

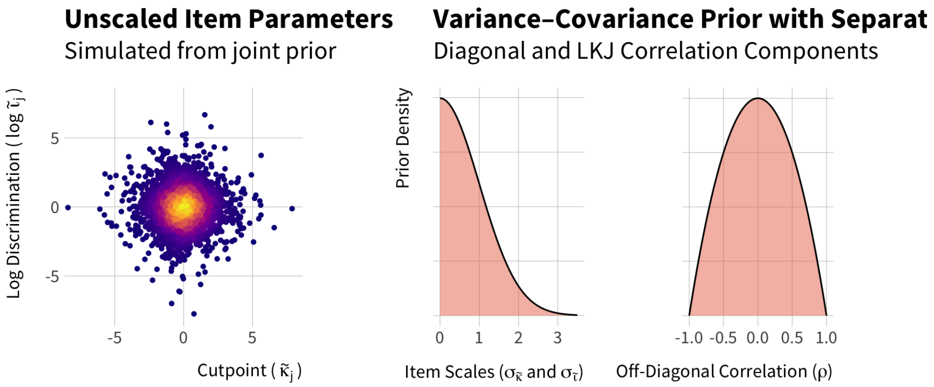


Figure 2.5: Components of the joint hierarchical prior for the unscaled item parameters. Left panel shows prior values for unscaled item parameters from the joint prior. Remaining panels show priors for decomposed covariance matrix components: including the standard deviation that form the matrix diagonal (middle) and the off-diagonal correlation from the LKJ prior (right).

all covariates are at their means. I then give this grand mean a Normal $(0, 1)$ prior, which implies a flat prior on the probability scale. Substantively this represents an assumption where the predicted probability of a conservative response for the typical item

average Democratic constituency and the average Republican constituency "could be anything." Because the latent scale is identified by restricting the item parameters, the relaxed prior for the average ideal points prevents the ideal point priors from interfering with the identification of the scale.

I set priors for the coefficients in the hierarchical model by

I give coefficients Normal $(0, 0.5)$ priors. Substantively, this represents a prior where a typical draw, expected to be one standard deviation away from the mean, would change the probability of a conservative response from 0.5 to 0.8413447 (if above) or to 0.3085375 (if below). Constants are given less informative Normal $(0, 1)$ priors, whose density is rather flat when transformed from the link scale to the probability scale, as shown in Figure **??**. Given that 95 percent of the standard Normal distribution falls between the quantiles -1.96 and 1.96, our priors should not give much weight to coefficients large enough to cause the response probability to leap from one end of that scale to the other.

Within-group standard deviations, as well as the scale parameters in the non-centered error terms (τ), are given $\text{LogNormal}\left(0, 1\right)$ priors.

2.6 Testing the Model with Simulated Data

2.7 Ideal Point Estimates for District-Party Publics

2.7.1 Data

2.7.2 Posterior Analysis

2.8 Testing the Model with Simulated Data

2.9 Data Sources

Describe data

2.10 Model Results

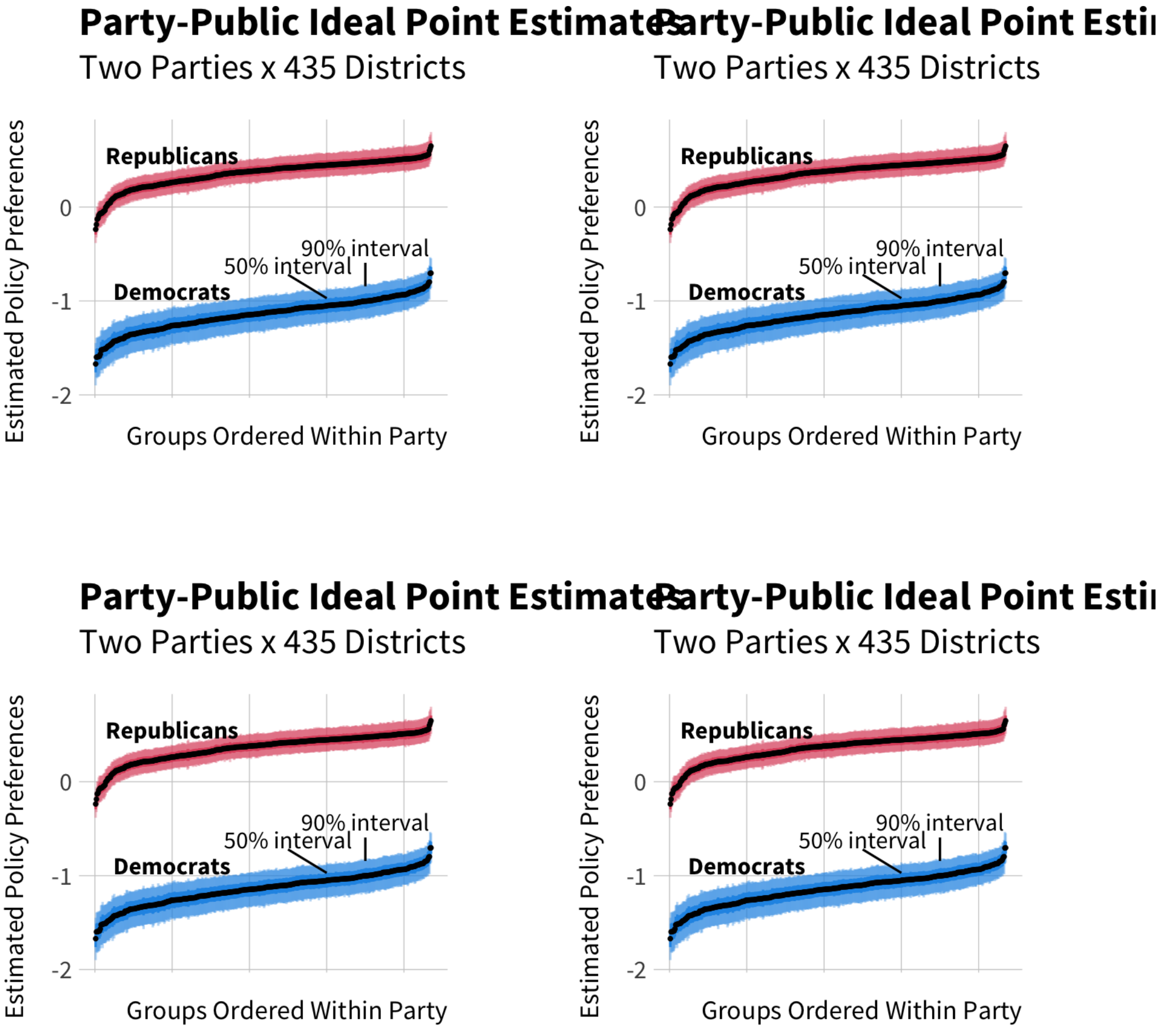
The model was estimated using a remote server at the University of Wisconsin–Madison.[22] I generated posterior samples using MCMC on 5 Markov chains. Each chain was run for 2,000 iterations, divided into 1,000 warmup iterations to tune Stan's adaptive HMC algorithm and 1,000 post-warmup iterations saved for analysis.[23] Following the advice of Link and Eaton (2011), I stored every post-warmup sample with no thinning of chains, resulting in a total of 5,000 samples per parameter across all chains.[24]

Here we can reference Figure 2.6.

[22] A Linux server ("Linstat") maintained by Social Science Computing Cooperative.

[23] The algorithm was initialized with an `adalt_delta` parameter of 0.9 and a `max_treedepth` of 15.

[24] The chains mix well and exhibit little autocorrelation, which is owed to the fact that Hamiltonian Monte Carlo algorithms are much more efficient at proposing transitions and thus exploring a parameter space.



Party-Public Ideal Point Estimates

Two Parties x 435 Districts

Party-Public Ideal Point Esti

Two Parties x 435 Districts

Party-Public Ideal Point Estimates

Two Parties x 435 Districts

Party-Public Ideal Point Esti

Two Parties x 435 Districts

Figure 2.6: Posteriors

— 3 —

Bayesian Causal Models for Political Science

Before I employ the estimates of party-public ideology obtained in Chapter 2, this chapter discusses a Bayesian view of causal inference. This framework addresses two major themes in the empirical problems that I confront later in the project, as well as several other minor themes.

First, this project views causal inference as a problem of posterior predictive inference. Causal models are tools for inferences about missing data: what we would observe if a treatment variable were set to a different value. The unobserved data are "unobserved potential outcomes" in the Rubin causal framework or "counterfactual outcomes" in the Pearl framework (Pearl 2009; Rubin 2005). Causal inference can be Bayesian if the target of interest is the probability distribution of unobserved potential outcomes (or the probability distribution of any causal estimand), conditioning on the observed data. In this chapter, I will argue that this is what researchers are implicitly trying to obtain, even if implicitly, nearly all of the time.

Second, the Bayesian approach incorporates uncertainties about the key independent variable in this project. Causal estimands (to use the Rubin terminology) are comparisons of potential outcomes are two hypothetical values of a treatment, usually a unit's outcome under an *observed* treatment versus an *unobserved* treatment. The data in this project frustrate the typical structure of a causal estimand because the treatment value of interest—policy ideology in a district-party public—is not observed. Instead, it is estimated up to a probability distribution specified by the measurement model in Chapter 2. Uncertainty about the effect of setting district-party ideology to some new value θ' therefore contains multiple sources of uncertainty: statistical uncertainty about the estimated causal effect, and measurement uncertainty about the original value of θ before a causal intervention. Bayesian analysis provides the statistical machinery to quantify uncertainty in these causal effects as if they were any other posterior quantity, by marginalizing the posterior distribution over any nuisance parameters.

This chapter unpacks these issues according to the following outline. First, I review the notation and terminology for causal modeling in empirical research, where data and causal estimands are posed in terms of "potential outcomes" or "counterfactual" observations. I then describe a Bayesian reinterpretation of these models, which uses probability distributions to quantify uncertainty about causal effects and counterfactual data, conditional on observed data. Because Bayesian modeling remains largely foreign to political science, I spend much of the chapter explaining what a Bayesian approach to causal inference means with theoretical and practical justifications: how priors are inescapable for many causal claims, how priors provide valuable structure to improve the estimation of causal effects, and practical advice for constructing and evaluating Bayesian causal models. Finally, I provide examples of Bayesian causal modeling by replicating and extending published studies in political science, showing where priors add value to causal inference.

3.1 Overview of Key Concepts

3.1.1 Causal models

As an area of scientific development, *causal inference* refers to the formal modeling of causal effects, the assumptions required to identify causal effects, and research designs that make these assumptions plausible. Scientific disciplines, especially social sciences, have long been interested in substantiating causal claims using data, but the rigorous definition of the full causal model and identifying assumptions distinguishes the current causal inference movement from other informal approaches.

Contemporary approaches to causal inference span several fields, most notably in economics, epidemiology, and computer science. The dominant modeling approach to causal inference in political science is rooted in a notation of *potential outcomes* (Rubin 1974, 2005). This "Rubin model" formalizes the concept of a causal effect by first defining a space of hypothetical outcomes. The outcome variable Y for unit i is a function of a treatment variable A. "Treatment" refers only to a causal factor of interest, regardless of whether the treatment is randomly assigned.[1] Considering a binary treatment assignment where $A = 1$ represents treatment and $A = 0$ represents control, unit i's outcome under treatment is represented as $Y_i(A = 1)$ or $Y_i(1)$, and the outcome under control would be $Y_i(A = 0)$ or $Y_i(0)$. The benefit of expressing Y in terms of hypothetical values of A allows the causal model to describe, with formal exactitude, the entire space of possible outcomes that result from treatment assignment. The model allows us to define a treatment effect for an individual unit, denoted τ_i, as the difference in potential outcomes when changing the treatment A_i.

$$\tau_i = Y_i(A_i = 1) - Y_i(A_i = 0) \tag{3.1}$$

If τ_i is not 0, then A_i has a causal effect on Y_i. Defining the causal model in terms of unit-level

[1] Some causal inference literatures refer to treatments as "exposures," which may feel more broadly applicable to settings beyond experiments. For this project, I make no distinction between treatments and exposures.

effects provides an exact, minimal definition of a causal effect: A affects Y if the treatment has a nonzero effect for any unit. A causal model may describe more complex features of a causal system, such as whether a unit complies with their treatment assignment, whether the treatment effect are indirectly mediated by other variables, and so on.

The entire space of potential outcomes is a hypothetical device. Although a causal model defines potential outcomes for every unit under every treatment assignment, it is not possible to observe all of these potential outcomes, since a unit can receive only one treatment, and thus can only take a single outcome value. This implies that the individual causal effect (τ_i), while a valid feature of the hypothetical causal model, is never actually observed for any unit. This "fundamental problem of causal inference" is the core philosophical problem in causal inference; the researcher never observes a unit under more than one treatment status, so they can never make causal claims with observed data alone (Holland 1986). Instead, causal claims are possible only by imposing assumptions on the data. These "identification assumptions" specify the conditions under which observed can be used to describe what the data would look like if units received counterfactual treatments (Keele 2015). For observational causal inference, it is common to invoke the following four assumptions. The *consistency* assumption states that when a unit receives a treatment, the outcome that we observed is the potential outcome for that unit under that treatment.

$$\text{Consistency: } Y_i = Y_i(A_i = a) \mid A_i = 1 \tag{3.2}$$

In other words, observing the outcome does not affect the outcome, and there are no hidden versions of treatment that are not defined in the causal model. The *no interference* assumption states that unit i's potential outcome depends only on i's treatment status, not on any other units' treatment status.

$$\text{No Interference: } Y_i\left(A_i\right) = Y_i\left(A_1, A_2, \ldots, A_n\right) \tag{3.3}$$

The consistency and no-interference assumptions are sometimes grouped into a single assumption, called the "stable unit treatment value assumption" (or SUTVA, Rubin 1980). The *conditional independence* assumption (CIA), also known as conditional "ignorability" or conditional "unconfoundedness," states that potential outcomes are independent of treatment status conditional on a unit's membership in a specific stratum of covariates $X_i = x$.

$$\text{Ignorability: } p\left(A_i = a \mid Y_i\left(A_i = 1\right), Y_i\left(A_i = 0\right), X_i = x\right) = p\left(A_i = a \mid X_i = x\right) \tag{3.4}$$

The CIA is essentially an assumption about unit-level expectations under counterfactuals. Within stratum $X_i = x$, any observed Y_i in the treatment group are independent from the $Y_i(1)$ potential outcome values for units in the control group. This assumption is violated by self-selection processes or other confounding affects that sort units into treatment statuses in a way that is correlated with their potential outcomes. The CIA is usually invoked alongside a *positivity* assumption,

$$\text{Positivity: } 0 < p\left(A_i = a \mid X_i = x\right) < 1, \;\; \forall a \in \mathcal{A}, \;\; \forall x \in \mathcal{X} \tag{3.5}$$

where $\mathcal{A}$ and $\mathcal{X}$ are respectively the spaces of possible treatments and covariate strata. Positivity stipulates nonzero probability of treatment statuses within each strata, ensuring that causal inferences are conducted only on units that could conceivably have received other treatment statuses.[2] Conditional independence and positivity assumptions are essential for conducting causal inference using methods of covariate adjustment.

All together, these assumptions enable inferences about potential outcomes, which aren't always observed, using only observed data. For instance, from these assumptions it follows that the average Y values observed at $A = a$ are equal to the average potential outcomes that

[2] See Liao, Henneman, and Zigler (2019) for Bayesian causal estimation for "overlapping populations" under violations of strict positivity.

we would obtain by setting $A = a$ ourselves, within strata $X = x$:

$$\begin{aligned}\mathbb{E}\left[Y_i \mid A_i = a, X_i = x\right] &= \mathbb{E}\left[Y_i(A = a) \mid A_i = a, X_i = x\right] \\ &= \mathbb{E}\left[Y_i(A = a) \mid X_i = x\right]\end{aligned} \tag{3.6}$$

The first line of (3.6) equates observed outcomes given treatment to potential outcomes under an imposed treatment, which is enabled by SUTVA (consistency and no interference). Conditional independence allows us to suppresses the explicit conditioning on $A_i = a$ in the second line, and positivity is implied by referring generically to treatment level a and covariate stratum x.

Implications derived from identification assumptions are typically posed in terms of expectations about potential outcomes, $\mathbb{E}\left[Y_i\left(A_i\right)\right]$, instead of unit-level potential outcomes, $Y_i\left(A_i\right)$, because unit-level causal heterogeneity is unidentified without additional assumptions (Holland 1986). This is why causal quantities of interest, also known as causal estimands, are usually posed in terms of expectations as well. This project will generally discuss *conditional average treatment effects* (CATEs), which are expectations of treatment effects averaged over the population of units within a covariate stratum. Let $\bar{\tau}(a, a', x)$ be the average effect of setting $A = a$, as opposed to some other value a', within stratum $X = x$.

$$\bar{\tau}(a, a', x) = \mathbb{E}\left[Y_i(a) - Y_i(a') \mid X_i = x\right] \tag{3.7}$$

When deriving estimands such as the CATE with identification assumptions, it is important to note that assumptions describe minimally sufficient conditions for *nonparametric* causal identification (Keele 2015). There is no guarantee that linear regression models, or any parametric models, adequately control for confounders. For this reason, it is important to distinguish causal estimands from estimation methods, the latter of which can introduce statistical assumptions that differ from identification assumptions.

In order to separate the major planks of causal inference methods, I center the discussion of causal inference going forward around a three-part hierarchy of causal methodology.

1. Causal model: the hypothetical model that defines all treatments and potential outcomes at the unit level. The causal model is an omniscient view of the causal system, defining individual-level causal effects even though they cannot be observed in real data.
2. Identification assumptions: the assumptions required to identify causal estimands using only the observed data. These assumptions specify how knowledge of observed Y data can be used to make inferences about counterfactual Y data that is not observed. Inferences enabled by identification assumptions are typically posed as expectations about counterfactual data, unless further assumptions are specified that relate individual unit effects to average effects.[3] Because inferences are posed as expectations, they are abstractions that follow from analytical derivations and do not depend on statistical estimation approaches.
3. Estimation methods: How do we estimate the expectations derived from the causal model and identification assumptions? Do these estimation methods introduce additional statistical assumptions on top of the identification assumptions?

I lay out this hierarchy for two main reasons. First, it clarifies why researchers use certain research designs or statistical approaches to overcome particular problems with their data. Statistical assumptions, we will see, can undermine identification assumptions, which is why causal inference scholars tend to promote estimation strategies that rely on as few additional assumptions as possible (Keele 2015). One way to avoid these assumptions is to use research designs that eliminate confounding "by design" rather than through statistical adjustment, such as randomized experiments, instrumental variables, regression discontinuity, and difference-in-differences (for instance, Angrist and Pischke 2008). If researchers cannot design away these difficult assumptions, other methods are available to adjust for

[3] For instance, a *constant additive effects* assumption states that all causal effects are constant for all units and can be combined without interactions (Rosenbaum 2002).

confounders without as many strict assumptions about the functional form of the causal model as are commonly invoked in parametric regression models. Causal inference is not synonymous with the new "agnostic statistics" movement (e.g. Aronow and Miller 2019), but it is animated by a similar motivation to identify statistical methods that rely on as few fragile assumptions as possible. For causal inference problems, these methods include matching, doubly robust models, and machine learning methods for estimating flexible conditional expectation functions that are to varying degrees robust to misspecification, nonlinearities, or non-additivities in the data generating process (Aronow and Miller 2019; Aronow and Samii 2016; Green and Kern 2012; Hill 2011; Samii, Paler, and Daly 2016; Sekhon 2009). This dissertation will employ machine learning methods, in particular Bayesian neural networks (BNNs), to estimate regression functions that rely less on exact, reduced-form model specification choices.

Second, the three-part hierarchy of causal inference clarifies where my contributions around Bayesian causal estimation will be focused. As I discuss below, the "easiest way in" for Bayesian methods is through statistical estimation (level 3), since some flexible estimation methods are convenient to implement using Bayesian technologies (Imbens and Rubin 1997; Ornstein and Duck-Mayr 2020). I push this further by arguing that Bayesian estimation changes the interpretation of the causal model (level 1) by implying a probability distribution over the space of potential outcomes. This probability distribution allows the researcher to say which causal effects and counterfactual data are more plausible than others, which is a desirable property of causal inference that is not available through conventional inference methods. The Bayesian approach also has the power to extend the meaning of identification assumptions (level 2) by construing them also as probabilistic rather than fixed features of a causal analysis (Oganisian and Roy 2020).

3.1.2 Bayesian inference

Bayesian reasoning is a contentious and misunderstood topic in empirical political science, so it is important to establish some essential tenets to the approach before melding it with causal modeling. Bayesian analysis is the application of conditional probability for statistical inference. Its mechanical underpinnings are uncontroversial, essential building blocks of probability theory: how the probability of an event changes by conditioning on other known information. Any controversy surrounding Bayesian methods in political science is better understood as a disagreement over which modeling constructs we choose to describe using probabilities.

Whereas many statistical methods begin with a model of data given fixed parameters, Bayesian inference consists of a joint model for all components in a system. The "joint model" is simply a probability model for more than one event. For example, suppose that we are interested in the joint probability distribution of age and vote choice in a population. The joint distribution of these two variables as $p(Age, Vote)$, which can be equivalently expressed by factoring it in two ways:

$$p(Vote \mid Age)p(Age) = p(Age \mid Vote)p(Vote) \tag{3.8}$$

If we observe an individual's vote choice, how does this affect the probability distribution of age? Probability theory says that we divide the joint probability by the probability of the conditioning event.

$$\begin{aligned} \frac{p(Vote \mid Age)p(Age)}{p(Vote)} &= \frac{p(Age \mid Vote)p(Vote)}{p(Vote)} \\ \frac{p(Vote \mid Age)}{p(Age)} &= p(Age \mid Vote) \end{aligned} \tag{3.9}$$

This maneuver reveals Bayes' theorem: the probability of A given B, expressed in terms of B given A. Bayes' theorem provides a formal method for rationally updating a joint probability distribution by conditioning on known information.

The Bayesian paradigm of applied statistical modeling applies Bayes' theorem to data $\mathbf{y}$ and parameters $\boldsymbol{\pi}$. The joint model for the data and the parameters takes the form

$$p(\mathbf{y}, \boldsymbol{\pi}) = p(\mathbf{y} \cap \boldsymbol{\pi}) = p(\mathbf{y} \mid \boldsymbol{\pi})p(\boldsymbol{\pi}), \tag{3.10}$$

where $p(\mathbf{y} \mid \boldsymbol{\pi})$ represents the probability distribution of the data, conditioning on parameters, and $p(\boldsymbol{\pi})$ represents the probability distribution of parameters, marginalizing over the data. The marginal parameter distribution is usually referred to as a "prior distribution," since it describes the researcher's prior information (or, controversially, prior "beliefs") about the parameter. The joint model provides machinery for learning about parameters by conditioning the parameters on the data,

$$p(\boldsymbol{\pi} \mid \mathbf{y}) = \frac{p(\mathbf{y} \mid \boldsymbol{\pi})p(\boldsymbol{\pi})}{p(\mathbf{y})} \tag{3.11}$$

also known as obtaining the *posterior distribution* of the parameters.

These basic concepts of conditional probability and Bayesian updating are not foreign to political science, but it will be important to establish an interpretation of Bayesian thinking and Bayesian modeling that is productive for causal inference. This project invokes what McElreath (2017b) calls an "inside view" of Bayesian statistics—Bayesian statistics on its own terms. The inside view is a response to an "outside view" of Bayesian statistics as penalized maximum likelihood. Under the outside view, data follow probability distributions, while parameters are fixed. Bayesian estimation, in turn, is a likelihood model with an additional penalty on the parameters, and because the penalty represents the researcher's subjective beliefs, the penalty feels *ad hoc* and anti-scientific. This view of Bayesian statistics is admittedly confusing, and if we take it at face value, it is no wonder that causal inference in political science has largely avoided Bayesian tools other than for computational convenience.

The inside view, as mentioned above, construes a Bayesian model as a joint model for all variables in a system. The use of the word "variables" to encompass both data and parameters is crucial. The technology of a Bayesian model does not regard data and parameters as distinct from one another. They follow the same rules, just as age and vote choice followed the same rules in the above example. Data and parameters are both instantiations of uncertain processes, with the only semantic difference between the two being that observed variables are called "data" while unobserved variables are called "parameters" (McElreath 2017b).[4] Prior distributions and likelihood functions are the same thing: probability distributions that quantify uncertainty about a variable. If I were to observe a new data point from a model, I would be unable to predict its value exactly, but some values are more probable than others, given the parameters that condition the data. The same premise holds for parameters: if I could observe a parameter, I would have been unable to anticipate it exactly, but I could bet that some parameters are more likely than others, given the data that I have already seen. The joint model for all variables encapsulates the probabilistic relationships between data and parameters. Starting with the prior model, $p(\mathbf{y}, \boldsymbol{\pi})$, we can condition the model on chosen parameters to obtain a rationalizable distribution of data, $p(\mathbf{y} \mid \boldsymbol{\pi})$. Or we can condition the model on data to obtain a rationalizable distribution of parameters, $p(\boldsymbol{\pi} \mid \mathbf{y})$. McElreath (2017a) calls these maneuvers "running the model forward" (updating data given parameters) or "running the model backward" (updating parameters given data).

From the inside view, Bayesian updating proceeds by considering a variety of possible scenarios that create data and evaluating which scenarios are consistent with the data. The joint prior model, $p(\mathbf{y} \mid \boldsymbol{\pi})p(\boldsymbol{\pi})$ describes an overly broad set of possible configurations of the world. These configurations contain a distribution of possible parameters, $p(\boldsymbol{\pi})$, and possible data given parameters, $p(\mathbf{y} \mid \boldsymbol{\pi})$. Bayesian updating decides which configurations of the world

[4] The semantic conventions are often sloppier in practice than many researchers would like to think. Many analyses use data that summarize lower-level processes, such as per-capita income in U.S or the percentage of women who vote for the Democratic presidential candidate, which behave like random variables in that their values could differ under repeated sampling. The semantic distinction between data and parameters has a similar spirit to the Blackwell, Honaker, and King (2017) view of measurement uncertainty, where "measurement error" falls on a spectrum between fully observed data and missing data.

are consistent with the data and are therefore more plausible. The plausibility, or posterior probability, of a parameter value is greater if the observed data are more likely to occur under that parameter value versus another value. In turn, the posterior distribution downweights model configurations that are implausible, or inconsistent with the data (McElreath 2020, chap. 2). This is an important distinction from non-Bayesian statistical inference, since there can is no formal notion of "plausible parameters given the data" without a posterior distribution, which necessitates a prior distribution. For causal inference, this means there can be no formal notion of "plausible causal effects" without a probability distribution over causal effects. The mission in the remainder of this chapter is to establish a framework for causal inference in terms of plausible effects and plausible counterfactuals.

The inside view of Bayesian modeling, and the philosophical unity that it brings to statistical machinery and inference, is possible even if using uninformative prior distributions that are indifferent to possible parameters *a priori*. This is how Bayesian methods tend to appear in political science to date, with noninformative priors that exist primarily to facilitate Bayesian computation for difficult estimation problems. The infamy of Bayesian methods, however, is owed to the ability of the researcher to specify "informative" priors that concentrate probability density on model configurations that are thought to be more plausible even before data are analyzed. There are many modeling scenarios where this concentration of probability delivers results that are almost unthinkable without prior structure: multilevel models that allocate variance to different layers of hierarchy, highly parameterized models with correlated parameters such as spline regression, and sparse regressions where regularizing priors are used to shrink coefficients and preserve degrees of freedom to overcome the "curse of dimensionality" (Bishop 2006; Gelman et al. 2013). At the same time, many researchers are skeptical of Bayesian methods because supplying a model with non-data information can be spun as data falsification (García-Pérez 2019). As I elaborate in Section 3.3, it is a mistake to equate flat prior "flatness" with prior "uninformativeness," and there are many legitimate sources of prior information that have nothing to do with subjective beliefs.

3.2 Bayesian Causal Modeling

Having reviewed the basics of causal models and Bayesian inference, we now turn to a framework for Bayesian causal modeling. The distinguishing feature of a Bayesian causal model is that the elemental units of the model, the potential outcomes, are given probability distributions. This probability distribution reflects available causal information that exists outside the current dataset. Bayesian inference proceeds updating our information about causal effects and counterfactual potential outcomes in light of the observed data. The headings under 3.2 introduce this modeling framework at a high level. I provide a probabilistic interpretation and notation for potential outcomes models, describe how a Bayesian modeling framework affects the "hierarchy of causal inference," and provide some broad justification for Bayesian causal modeling.

3.2.1 Probabilistic model for potential outcomes

As with other causal models, we begin at the unit level. Unit i receives a treatment $A_i = a$, with potential outcomes $Y_i\left(A_i = a\right)$. Suppose a binary treatment case where A_i can take values 0 or 1, so the unit-level causal effect is $\tau_i - Y_i\left(1\right) - Y_i\left(0\right)$. Although τ_i is unidentified, it is possible to estimate population-level causal quantities by invoking identification assumptions. For instance, the conditional average treatment effect at $X_i = x$, $\bar{\tau}(X = x) = \mathbb{E}\left[Y_i(1) - Y_i(0) \mid X_i = x\right]$, can be estimated from observed data assuming consistency, noninterference, conditional ignorability, and positivity. Suppressing the unit index i,

$$\begin{aligned} \bar{\tau}(X = x) &= \mathbb{E}\left[Y(A = 1) - Y(A = 0) \mid X = x\right] \\ &= \mathbb{E}\left[Y(A = 1) \mid X = x\right] - \mathbb{E}\left[Y(A = 0) \mid X = x\right] \\ &= \mathbb{E}\left[Y \mid A = 1, X = x\right] - \mathbb{E}\left[Y \mid A = 0, X = x\right] \end{aligned} \tag{3.12}$$

where the third line is obtained by the identification assumptions. The identification assumptions connect *causal estimands* and what I will call *observable estimands*. Causal estimands are the true causal quantities, but they are unobservable because they are stated as contrasts of potential outcomes. Observable estimands are the observable analogs of causal estimands and are equivalent to causal estimands if identification assumptions hold. Other literature refers to observable estimands as "nonparametric estimators" (Keele 2015), but I steer clear of this language because the gap between observable estimands and estimators is important for understanding the contributions of the Bayesian causal approach.

The transition to a Bayesian probabilistic model begins with an acknowledgment that no estimate of the observable estimand, $\mathbb{E}\left[Y \mid A = a, X = x\right]$, will be exact. The assumptions identify causal effects only in an infinite data regime, where the observable estimand is known exactly. Inference about causal effects from finite samples, however, requires further statistical assumptions that link the observable estimand to an estimator or model. Let $f(A_i, X_i, \boldsymbol{\pi}) + \varepsilon_i$ be a model for Y_i consisting of a function $f(\cdot)$ treatment A_i, covariates X_i, and parameters $\boldsymbol{\pi}$, and an error term ε_i. We let the systematic component $f(\cdot)$ be a plug-in estimator for $\mathbb{E}\left[Y \mid A = a, X = x\right]$. This setup is similar to any modeling assumption that appears in observational causal inference to link an estimator to the observable estimand, including parametric models for covariate adjustment, propensity models, matching, and more (Acharya, Blackwell, and Sen 2016; Sekhon 2009).[5] We use the statistical model to generate a CATE estimate, $\hat{\bar{\tau}}(X = x)$, by differencing these model predictions over the

[5] Although researchers are focusing more attention on estimation methods that focus on these statistical assumptions themselves, either by model ensembles/averages or "robust" models for propensity and response.

treatment.

$$\begin{aligned} \bar{\tau}(X = x) &= \mathbb{E}\left[Y \mid A = 1, X = x\right] - \mathbb{E}\left[Y \mid A = 0, X = x\right] \\ \hat{\bar{\tau}}(X = x) &= \mathbb{E}\left[f(A_i = 1, X_i 1 = x, \boldsymbol{\pi}) - f(A_i = 0, X_i = x, \boldsymbol{\pi})\right] \end{aligned} \tag{3.13}$$

Where the second line includes $\hat{\bar{\tau}}$ to indicate that $f(\cdot)$ is an estimator of $\bar{\tau}$.

The Bayesian approach, inspired largely by Rubin (1978a), confronts the problem with a joint model for data and parameters: $p\left(Y, \boldsymbol{\pi}\right) = p\left(Y \mid f(A, U, \boldsymbol{\pi})\right) p\left(\boldsymbol{\pi}\right)$. The data are distributed conditional on the statistical model prediction $f(\cdot)$, which conditions on the model parameters $\boldsymbol{\pi}$. The parameters also have a prior distribution $p\left(\boldsymbol{\pi}\right)$, or a distribution marginal of the data. These models for data and parameters are added statistical assumptions on top of causal identification assumptions. The data model is similar to any estimation approach that uses a probability model for errors (e.g. any MLE method or OLS with Normal errors). The prior model has no analog in OLS or unpenalized MLE, but this added statistical assumption will be leveraged as a major benefit as we explore Bayesian causal estimation below.

The joint generative model is sufficient to characterize the probability distribution for the conditional average treatment effect as defined in Equation (3.13),

$$p(\bar{\tau}(X = x)) = \int p\left[f(A = 1, X = x, \boldsymbol{\pi}) - f(A = 0, X = x, \boldsymbol{\pi}) \mid \boldsymbol{\pi}\right] p\left(\boldsymbol{\pi}\right) \, \mathrm{d}\boldsymbol{\pi} \tag{3.14}$$

which is the probability distribution of model contrasts for $A = 1$ versus $A = 0$. This distribution of model contrasts contains two sources of uncertainty: uncertainty about the data given parameters, and uncertainty over the parameters themselves. Integrating over $\boldsymbol{\pi}$ in Equation (3.14) marginalizes the distribution with respect to the uncertain parameters. Because the marginalized parameters are distributed according to the prior $p\left(\boldsymbol{\pi}\right)$, the expression in (3.14) represents a prior distribution for the CATE. This is an inherent feature of the Bayesian approach: probability distributions of causal quantities even before data are observed.

Conditioning on the observed data returns the posterior distribution for the CATE…

$$p(\bar{\tau}(X = x) \mid Y) = \int p\left[f(A = 1, X = x, \boldsymbol{\pi}) - f(A = 0, X = x, \boldsymbol{\pi}) \mid \boldsymbol{\pi}, Y\right] p\left(\boldsymbol{\pi} \mid Y\right) \, \mathrm{d}\boldsymbol{\pi} \tag{3.15}$$

where we would integrating over the posterior distribution of the parameters, instead of the prior distribution, returns a probability distribution for the CATE $\bar{\tau}$ that reflects Bayesian updating from data Y.

3.2.2 Why Bayesian causal modeling?

The Bayesian causal approach is sensible for causal inference because it facilitates *direct inference* about treatment effects given the data: which effect sizes are more likely or less likely than others. While confidence intervals are often misused to make probabilistic statements about parameters, the posterior distribution and posterior intervals actually enable the researcher to state the probability of positive treatment effects, negligible treatment effects, and more. Making positivistic statements about plausible causal effects is a natural way to think about the scientific aims of any discipline engaged causal inference: "the world probably works in this way, given the evidence." This is the view espoused by Don Rubin himself, the namesake of the Rubin causal model commonly employed in causal political science, who writes in the context of causal inference that "a posterior distribution with clearly stated prior distributions is the most natural way to summarize evidence for a scientific question" (Rubin 2005, 327). It is not formally coherent to invoke similar language under a non-Bayesian inference paradigm, since we cannot statistically describe the plausibility of a causal effect without a posterior belief, which entails a prior belief. This is why non-Bayesian methods formally conduct inference about the plausibility of *data* given fixed parameters, and inferences about parameters must be done indirectly with an additional layer of decision theory. Statistical decisions under non-Bayesian analysis can be awkward—for instance, using a p-value to measure whether the data are inconsistent with a null hypothesis that the researcher usually does not find credible at the outset, and usually with no corresponding measure of the relative plausibility of an alternative hypothesis (Gill 1999). Restated more formally, researchers routinely perform statistical inference by estimating $p(\mathbf{y} \mid H_0)$ (for null hypothesis H_0), when they are are probably more interested in $p(\boldsymbol{\pi} \mid \mathbf{y})$ or even $p(H_a \mid \mathbf{y})$ (for alternative hypothesis H_a).

The notion of direct inference is especially valuable when the observed data represent the whole population, which is common for observational causal inference in political science. Under a "frequentist" inference approach, estimators inherit their statistical properties from the sampling distribution of the estimator: the theoretical probability distribution of estimates in an infinite number of independent samples from the population. Many causal questions in political science conflict with this inferential framework, however, because the data come from events that have no possibility of future sampling. Instead, the data come from one-off events in history and often contain the entire population of relevant units, so the epistemic uncertainty about statistical inferences bears little resemblance to frequentist uncertainty about a sampling mechanism (Western and Jackman 1994).[6] The foundations of uncertainty in Bayesian inference are probability distributions that represent imperfect pre-data information about the generative processes underlying the variables in a model. Whether this imperfect information corresponds sampling randomness or other epistemic uncertainty can be subsumed in the Bayesian framework (Rubin 1978a).

Another reason why Bayesian methods make sense for causal modeling is because causal models, at their core, are models for counterfactual data. Because the Bayesian model is

[6] To be sure, there is non-Bayesian theory for statistical uncertainty without resampling, namely "design-based uncertainty" where the source of uncertainty is the treatment assignment mechanism itself (Abadie et al. 2020; Keele, McConnaughy, and White 2012). This framework is rarely invoked in political science except among researchers on the cutting edge of "agnostic" causal inference. For more on agnostic statistics, see Aronow and Miller (2019). It is also common for Bayesian statisticians to be interested in frequentist properties of their methods such interval coverage (Rubin 1984), which partially motivates an interest in "objective Bayesian inference" (Berger and others 2006; Fienberg and others 2006).

a *generative* model for parameters and data, the model contains all machinery required to directly quantify counterfactual potential outcomes using probability distributions. To see this in action, we can "run the model forward" to create a predictive distribution for Y given the model parameters. Denote these simulated observations as $\tilde{Y}$ to distinguish them from the data observed Y. If we marginalize this predictive distribution with respect to the prior parameters, we obtain a "prior predictive distribution"—the distribution of data we would expect under the prior (Gelman et al. 2013).

$$p\left(\tilde{Y} \mid A = a, X = x\right) = \int p\left(\tilde{Y} \mid A = a, X = x, \boldsymbol{\pi}\right) p\left(\boldsymbol{\pi}\right) \,\mathrm{d}\boldsymbol{\pi} \tag{3.16}$$

We update this distribution by conditioning on observed data, delivering a "posterior predictive distribution"—the distribution of data that we expect from the posterior parameters.

$$p\left(\tilde{Y} \mid Y, A = a, X = x\right) = \int p\left(\tilde{Y} \mid A = a, X = x, \boldsymbol{\pi}\right) p\left(\boldsymbol{\pi} \mid Y\right) \,\mathrm{d}\boldsymbol{\pi} \tag{3.17}$$

These predictive distributions are the basis for out-of-sample inference in Bayesian generative models,[7] and they are the basis for counterfactual inference as well. Invoking the causal identification assumptions, we generate counterfactual data as predictive distributions as well, setting the treatment $A = a$ to some other value $A = a'$. Denote these counterfactual predictions $\tilde{Y}'$, which I will subscript i to show that this model implies a probability distribution for individual data points as well as aggregate treatment effects.

$$p\left(\tilde{Y}_i' \mid Y, A_i = a', X_i = x\right) = \int p\left(\tilde{Y}_i' \mid A_i = a', X_i = x, \boldsymbol{\pi}\right) p\left(\boldsymbol{\pi} \mid Y\right) \,\mathrm{d}\boldsymbol{\pi} \tag{3.18}$$

Stated more simply: if causal models define a space of potential outcomes, then Bayesian causal models are probabilistic representations of the potential outcome space. Probability densities over potential outcomes are defined in the prior and in the posterior, and they can

[7] Simulations of this sort are possible under any likelihood-based model that posits a generative probability distribution for the data, but Bayesian predictive distributions marginalize over the parameter distribution instead of conditioning on fixed parameters. This makes Bayesian predictive distributions a more complete accounting of statistical and epistemic sources of uncertainty.

be defined all the way to the unit level if the generative model contains a probability statement for unit data.[8] The Bayesian view of causal inference, where the statistical model is nothing more remarkable than a missing data model for unobserved counterfactuals, is at least as old as Rubin (1978a).[9] Bayesian methods for causal inference have appeared in political science only sporadically in the decades since (Green et al. 2016; Horiuchi, Imai, and Taniguchi 2007; Ornstein and Duck-Mayr 2020). Unlike these rare examples of Bayesian causal inference, however, this chapter will contain more practical guidance for thinking Bayesianly about causal modeling, more synthesis of Bayesian causal innovations from other fields, and more examples of Bayesian causal modeling in practice.

It is common for advocates of Bayesian inference to celebrate the fact that the posterior distribution quantifies uncertainty in all parameters simultaneously, but this is especially useful for causal methods that entail multiple estimation steps. These multi-stage procedures include instrumental variables, propensity score weighting, synthetic control, causal mediation approaches, and structural nested mean models (Acharya, Blackwell, and Sen 2016; Angrist and Pischke 2008; Blackwell and Glynn 2018; Imai et al. 2011; Xu 2017). Bayesian approach to these methods combines all estimation stages into one model, estimating the ultimate treatment effect by marginalizing the posterior distribution with respect to the "design stage" parameters (Liao 2019; McCandless, Gustafson, and Austin 2009; Zigler and Dominici 2014). The combined modeling approach diverges from methods that use only

[8] Some modeling approaches can estimate average causal effects with group-level statistics only, eliding the unit-level model altogether. This can weaken the model's dependence on parametric assumptions for units, falling back onto more dependable parametric assumptions for the statistics, e.g. the Central Limit Theorem for group means. A model of this type will naturally stop short of defining probability distributions for counterfactual units, but it does define probability distributions for counterfactual means. In some cases, such as binary outcome data, means in each group are sufficient statistics for the raw data, so the unit level model is implied by the group-level model. See Section 3.3.9 for explanation and examples.

[9] In more general modeling contexts beyond causal inference,Jackman (2000) makes a similar argument that all estimates, inferences, and goodness-of-fit statistics can be unified as functions of missing data, with Bayesian posterior sampling as a natural way to describe our information about these functions. This has a similar spirit as McElreath (2017b)'s "inside view" of Bayesian stats, where all data and parameters are unified as functions of probabilistic variables in a joint generative model.

point estimators, which ignore design stage uncertainty by conditioning the final-stage effects on design stage estimates or must derive post-hoc variance estimators to account for design stage uncertainty. It is reminiscent of "uncertainty propagation" methods insofar as uncertainty from design estimates are pushed forward to later estimates—for example, the way Kastellec et al. (2015a) simulate measurement uncertainty in MRP estimates of public opinion in later analyses. Unlike uncertainty propagation, which is mechanically similar to a "posterior cut" (Plummer 2015), the complete Bayesian model effectively treats the design stage estimates as priors and updates all model parameters using information from all stages of the model (Liao and Zigler 2020; Zigler 2016; Zigler et al. 2013).

The combined modeling approach is important for this project because the key independent variable, district-party public ideology, is estimated from a Bayesian measurement model. In order to understand district-party ideologies effects in primary elections, it is essential to pass posterior uncertainty from this measurement model into the causal analyses. However, building a combined model for district-party ideology and all downstream effects would be logistically overwhelming, so I approximate the full model by drawing ideal points in causal analyses from a multivariate prior distribution that reflects the measurement model's posterior samples. For instance, to understand the causal effect of district-party ideal point $\bar{\theta}_g$ on some outcome measure y_g, our causal model contains a model for the outcome,

$$y_g \sim \mathcal{D}\left(\theta_g, \ldots\right), \tag{3.19}$$

where $\mathcal{D}\left(\cdot\right)$ is some distribution, and we place a multivariate Normal prior on the vector of all district-party ideal points $\bar{\boldsymbol{\theta}}$ as

$$\bar{\boldsymbol{\theta}} \sim \text{MultiNormal}\left(\bar{\bar{\boldsymbol{\theta}}}, \bar{\Sigma}\right) \tag{3.20}$$

where $\bar{\bar{\boldsymbol{\theta}}}$ and $\bar{\Sigma}$ are the estimated mean vector and variance–covariance matrix of the ideal point samples from the measurement model. The multivariate Normal prior is justified on the ground that the original ideal point model smoothed ideal point estimates using a hierarchical Normal prior, and estimating the variance–covariance matrix from the samples is a simple method to summarize systematic relationships between ideal points as a function of their geographic attributes. This approximation avoids the difficulties of writing an overly complicated model while still giving ideal points a prior that resembles their posterior distribution from the IRT model.

One final justification for Bayesian causal modeling is that prior information is everywhere. This is a longer discussion that I untangle in Section 3.3, but to preview, priors matters for the way researchers think about their modeling decisions, and they affect the inferences that researchers draw from data, even if they wish to avoid explicit Bayesian thinking about their analyses.

3.2.3 Bayesian modeling and the hierarchy of causal inference

This section interprets the Bayesian causal inference framework in light of the "hierarchy of causal inference" described in Section 3.1.1. The hierarchy helps us account for the ways that Bayesian methods have already been invoked for causal inference in political science and in other fields, and it helps us understand how the Bayesian statistical paradigm reinterprets causal inference more broadly. To review, the hierarchy consisted of three parts:

1. The causal model: definition of potential outcome space, causal estimands expressed in terms of potential outcomes.
2. Identification assumptions: linkage from causal estimands expressed as potential outcomes to observable estimands expressed using observed data.
3. Estimation: Methods for estimating observable estimands with finite data.

We began our discussion of the Bayesian causal model above by considering a plug-in estimator for an observable estimand that came from a Bayesian statistical model. Bayes was invoked as "mere estimation," so we began our understanding of Bayesian causal modeling at level 3 of the hierarchy. As only an estimation method, a Bayesian estimator (such as a posterior expectation value) doesn't obviously change the meaning of the observable estimand or the causal estimand. After all, the estimator exists in the space of real data, unlike a causal estimand that belongs to the hypothetical space of potential outcomes. Being merely an estimator, we could evaluate the Bayesian model for its bias and variance like any other estimator.

The realm of "mere estimation" is where many Bayesian causal approaches appear in political science and other fields. The estimation benefits of Bayes tend to fall into three categories: priors provide practical stabilization or regularization, posterior distributions are convenient quantifications of uncertainty, or MCMC provides a tractable way to fit a complex model. We could characterize the use of Bayesian methods for these purposes as practically valuable but theoretically dispensable, in the sense that researchers might prefer non-Bayesian means to the same ends. For instance, Green and Kern (2012) adapt Bayesian Additive Regression Trees (or BART, Chipman et al. 2010) to measure treatment effect heterogeneity in randomized experiments. (See Hill 2011 for a non-political science introduction to causal inference with BART.) The advantages of a regression tree model for treatment heterogeneity is that it can explore arbitrary interactions among covariates while controlling overfitting, but the fact that BART is Bayesian is an afterthought. We observe a similar pattern in the use of Gaussian process models for fitting the running variable in regression discontinuity designs by Ornstein and Duck-Mayr (2020) and in the development of augmented LASSO estimators for sparse regression models by Ratkovic and Tingley (2017; 2017).[10] These authors use priors to regularize richly parameterized functions, posterior distributions to characterize uncertainty, and MCMC to estimate models, but the theoretical

[10] See Tibshirani (1996) for a general introduction to the LASSO shrinkage estimator using the L1 penalized optimization. Park and Casella (2008) implement a Bayesian LASSO using Laplace priors for regression coefficients.

implications of Bayesian causal estimation are not a major focus.

What does it mean for Bayesian estimation to have theoretical implications for causal inference? This brings our focus to level one of the causal inference hierarchy: the model of potential outcomes Any estimation method that invokes Bayesian tools requires priors for model parameters. Because causal estimates are functions of model parameters, prior densities on model parameters imply that some causal effects can be more or less likely even before considering any data. If the statistical model contains a unit-level data model, which is the case for most regression approaches, this implies that unit-level potential outcomes also have prior probability densities: some potential outcomes at the unit level are more or less likely, marginal of any observed data. This is a decisive philosophical departure from a non-Bayesian approach to causal modeling, where potential outcomes and causal effects are simply defined in a space of outcomes. As with causal effects, the benefit of prior distributions for unit outcomes is that that we can obtain, describe, and conduct direct inference using the posterior distribution for unit counterfactuals. The drawback is that researchers must specify priors for model parameters and understand how they impact the implied priors for unit counterfactuals. A few recent methodology papers in political science have invoked this idea of unit counterfactual estimation in a Bayesian framework, which is especially intuitive for synthetic control estimation (Carlson 2020; see Ratkovic and Tingley 2017 for a regression application). But these papers do not highlight the notion of direct priors for counterfactuals, so skeptical applied researchers have little guidance for understanding what it means to have priors on counterfactual data, either theoretically or practically.

The theoretical notion of prior densities in the potential outcome space is only interesting if it makes sense to see priors as important for causal estimation. I have already argued that priors enable direct probabilistic inference on causal effects and unit counterfactuals, which is the essential modeling goal for causal inference in the first place. It is natural to ask if it is sensible to use flat or uninformative priors achieve to enable direct probabilistic inference while minimizing the uncomfortable feeling of specifying priors for unobservable counterfactuals, avoiding the need to think hard about priors at all. As I discuss in future sections, however, flat priors obscure rather than avoid these complexities. This is because even simple modeling scenarios that begin with flat priors usually contain quantities of interest whose priors cannot also be flat. As a result, it is the researcher's job to decide how to parameterize a problem and how the chosen priors affect the important quantities in the analysis.

Priors on important modeling constructs do not have to be an inconvenience. There are many scenarios where priors can actually relax assumptions, which building robustness checks directly into a statistical model. This is how Bayesian inference affects layer two of the hierarchy: identification assumptions. By their nature, identification assumptions can never be validated by consulting the data, so most causal inference research projects simply condition the analysis on the identification assumption holding. If we invoke a measurement model of treatment effects akin to Gerber, Green, and Kaplan (2004),

$$\text{Estimated Effect} = \text{True Effect} + \text{Bias} + \text{Error}, \tag{3.21}$$

such that the estimated effect equals the true effect only if it is estimated with zero random error and zero systematic bias. Identification assumptions imply a prior that the bias is precisely zero, but in many applied research contexts, this assumption may be unreasonable to sustain with 100% certainty. A Bayesian approach to identification assumptions allows the researcher to relax their model of treatment effects by specifying a different prior on the bias term, or the sensitivity parameters that compose the bias term, that is consistent with the researcher's reasonable expectations for the remaining bias in the research design (Oganisian and Roy 2020). This bears resemblance to sensitivity testing approaches in which the researcher evaluates the treatment effect estimates by stipulating a range of fixed values for a key sensitivity parameter, and then evaluating the treatment effect estimate conditional on each fixed value (Acharya, Blackwell, and Sen 2016; Imai et al. 2011). Constructing these identification assumptions as priors lets the researcher conduct inference about treatment effects by *marginalizing over* their priors for design parameters, rather than conditioning on fixed design parameter values with little thought to which values are plausible or implausible. One recent political science example of this approach is Leavitt (2020), who frames the parallel trends assumption in a difference-in-differences (DiD) analysis as a prior over unobserved trends. This introduces an additional layer of "epistemic uncertainty" into the DiD analysis that would ordinarily be assumed to be zero by design. I elaborate on the value of priors for unidentifiable quantities in Section 3.4.1.

A few papers in political science and related fields invoke Bayesian causal models that occupy more than one level of this causal inference hierarchy at once. One important figure to note is Rubin himself, who has advocated for a "phenomenological Bayes" approach to causal inference since his pioneering papers on causal inference (Imbens and Rubin 1997; Rubin 1978a, 1978b, 2005). The fundamental tenet of the so-called "phenomenological" approach is the use of parametric models to generate posterior distributions for unobserved potential outcomes, which are used to construct causal estimates as functions of these unit-level posterior predictions. Posterior distributions for units are essential to the approach because statistical models may differ in their parameterization, making model parameters difficult to compare, but counterfactual data from the posterior distribution can always be compared to observed data regardless of the model parameterization. This is especially useful for studies with more complex missing data structures, such as studies with noncompliance, because Bayesian methods can always return a posterior distribution even for quantities that are not point-identified from data (Imbens and Rubin 1997). As such, the Rubin example invokes a Bayesian framework for estimation (level three) as well as a philosophical posture that the posterior distribution directly characterizes the plausibility of counterfactual data (level one). A recent political science example invoking a Bayesian approach for missing data under treatment noncompliance is Horiuchi, Imai, and Taniguchi (2007).

Another important frontier for Bayesian methods in causal inference is meta-analysis. A fundamental modeling dilemma for meta-analysis is the choice between “fixed effects” and “random effects” approaches to meta-analysis (Borenstein et al. 2011). Fixed effects meta-analysis assumes that all studies contain imperfect estimates of the same underlying treatment effect, while random effects admit the possibility of between-study heterogeneity in the true underlying effect. Bayesian approaches to meta-analysis can compromise between the two extremes, relaxing the fixed effects assumption by allowing between-study variation while incorporating prior information that rules out extreme heterogeneity more aggressively than the conventional random effects approach. A common example from Bayesian pedagogy is a meta-analysis of parallel experiments across schools by Rubin (1981). The approach simultaneously estimates study-specific treatment effects, cross-study variance in treatment effects, and a population average treatment effect. Meager (2019) creates a similar model to summarize the experimental evidence on micro-credit expansion in developmental economics. Like the noncompliance approaches above, these meta-analyses engage in levels one and three by constructing the problem explicitly as Bayesian learning of experimental effects and engaging assertively with Bayesian modeling assumptions using hierarchical priors in the meta-analytical model. In political science, Green et al. (2016) perform a fixed effects analysis to aggregate noisy treatment effects, invoking a notion of Bayesian learning from parallel experiments but stopping short of a richer statistical model for cross-study variation. As I show in Section 3.3.9, the researcher’s assumptions about cross-study variation is highly consequential for meta-analytic inference, and a Bayesian framework provides inimitable benefits for theorizing about those assumptions and understanding how they affect inferences.

For the remainder of this chapter, I explore several ways Bayesian modeling changes our view of causal inference at all three levels of hierarchy: causal modeling, identification assumptions, and statistical estimation. Many points discussed below touch on more than one level of the hierarchy, so the chapter cannot be neatly divided according to the hierarchy.

3.3 Understanding Priors in Causal Inference

What are priors for?

What do priors do?

This is because a prior's flatness is only relative to the parameterization of a model, and functions of parameters are not likely to have flat densities even if the underlying parameters have flat densities

3.3.1 Information, belief, and data falsification

Data falsification versus unavoidable choice: imagine a study with a posterior distribution $p(\mu \mid \mathbf{y})$ that is proportional to the likelihood times the prior.

$$p(\mu \mid \mathbf{y}) \propto p(\mathbf{y} \mid \mu)p(\mu) \tag{3.22}$$

Rewrite the right side in product notation for n observations of y_i for units indexed by i, letting $l(y_i) = p(y_i \mid \mu)$

$$p(\mu \mid \mathbf{y}) \propto \prod_{i=1}^{n} l(y_i)p(\mu) \tag{3.23}$$

Suppose that we express this proportionality on the log scale, where the log posterior is proportional to the log likelihood plus the log posterior.

$$\begin{aligned} \log p(\mu \mid \mathbf{y}) \propto \sum_{i=1}^{n} \log l(y_i) + \sum_{i=1}^{n} \log p(\mu) \\ \propto \log l(y_1) + \log p(\mu) + \log l(y_2) + \log p(\mu) + \ldots + \log l(y_n) + \log p(\mu) \end{aligned} \tag{3.24}$$

The setup in (3.24) highlights a few appealing intuitions. First, it shows how each observation "adds information" to the log posterior distribution. Data that are more likely to be observed given the parameter (larger $l(y_i)$ values) increase the posterior probability that parameter. We also see that the prior probability "adds information" to the posterior in the same way data add information, captured by the addition of each $p(\mu)$ term. Parameters that are more probable in the prior are more probable in the posterior.

The proportionality (3.24) also reveals how the posterior "learns" from flat priors. A flat priors implies that prior probability $p(\mu)$ is constant for all potential values of μ. Because (3.24) is a proportionality, this lets us disregard $p(\mu)$ entirely by factoring it out of the proportionality, leaving us with an expression that the log posterior is proportional to the likelihood of the data only if the prior is flat.

$$\log p(\mu \mid \mathbf{y}) \propto \log l(y_1) + \log l(y_2) + \ldots + \log l(y_n) \tag{3.25}$$

If $p(\mu)$ is not flat, however, and $p(\mu)$ varies across values of μ, we can no longer ignore $p(\mu)$ in (3.24) shows that $p(\mu)$ varies across values μ. Not only does this prevent us from dropping $p(\mu)$ from the proportionality, but it also reveals how the prior "adds information" to the posterior by the same mechanism that observations do: adding to the log posterior distribution. This general expression where both data and priors contribute to the posterior distribution has led some researchers to argue the Bayesian inference with non-flat priors is analytically indistinguishable from data falsification (García-Pérez 2019). We can highlight this behavior by obscuring each $p(\mu)$ term with a square $\square$.

$$\log p(\mu \mid \mathbf{y}) \propto \log l(y_1) + \square + \log l(y_2) + \square \ldots + \log l(y_n) + \square \tag{3.26}$$

Behind each square is *some contribution* to the log posterior. The fact that it adds information to the log posterior is unaffected by whether the hidden term is the probability of an additional observation $l(y_i)$ or the prior probability of a parameter value $p(\mu)$.

3.3.2 Priors are not for de-confounding

3.3.3 Flatness is a relative, not an absolute, property of priors

Formally, a flat prior is a probability distribution that assigns equal probability density to all possible values of a parameter. It is common for researchers to prefer flatter priors in situations where they lack specific prior information about model parameters. In cases where a prior distribution is perfectly flat, the posterior mode will be equivalent to the maximum likelihood estimator regardless of the sample size. For this reason, many researchers who find themselves exploring Bayesian methods use flat or nearly-flat priors as a "default choice". It is likely that a researcher exploring Bayesian modeling for causal inference might make a similar choice, in the interest of "letting the data speak." A common criticism of this practice is that flat priors understate the researcher's actual prior information. Although this sounds obviously true, it is easy to see why an applied researcher would be unbothered by it—what's the harm with a vague prior? Instead, I argue that the default use of flat priors can often *misspecify* prior information in many common modeling scenarios. Researchers must set prior distributions on parameters, but they usually have prior information about outcome data, which are functions of parameters. If researchers are not mindful of the functional relationship between raw parameters and other quantities of interest in the analysis, they will not the pernicious effects that "default priors" can have on model behavior.

To understand the consequences of prior choices, it is essential to understand the *implied prior*. Suppose we have a parameter π and a function of that parameter, $h(\pi)$. If π is a prior density, then $h(\pi)$ has an implied prior density that is affected by the density of π and the composition of the function $h(\cdot)$. Consider a simple example where π is distributed $\text{Normal}\left(0, 1\right)$, and $h(\pi) = 3 + 2\pi$. The implied prior for $h(\pi)$ is $\text{Normal}\left(3, 2\right)$, because a linear transformation of a Normal random variable results in another Normal random variable. Figure 3.1 shows the probability density of π and the implied density of $h(\pi)$. Any function of a parameter will an implied probability density, but there is no general guarantee that the implied density will be easy to understand like the example in Figure 3.1.

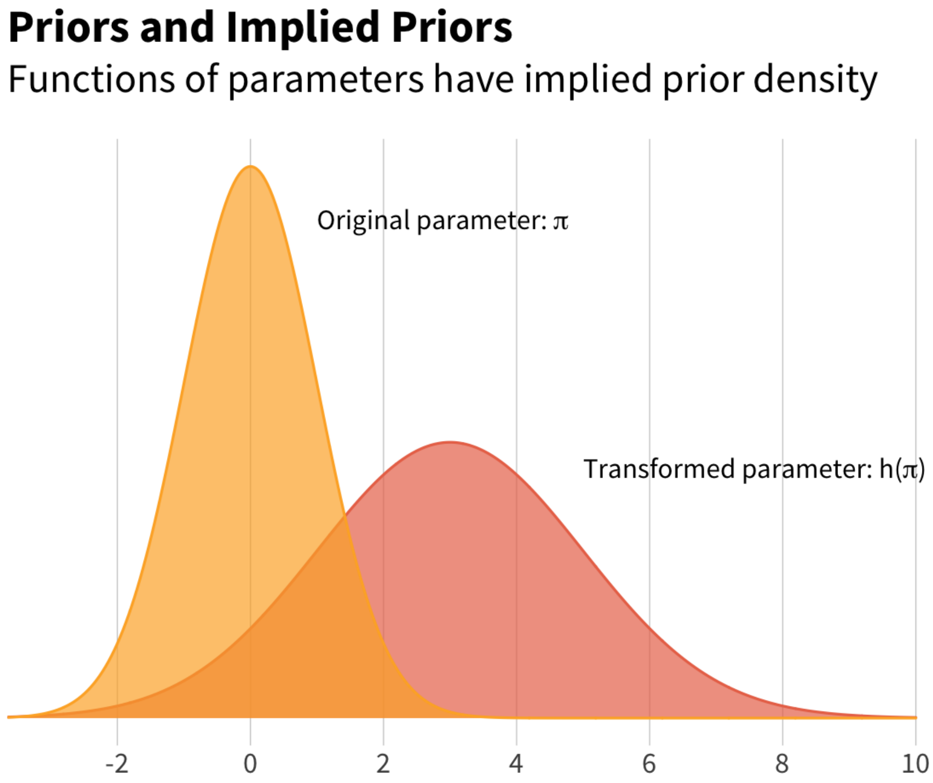


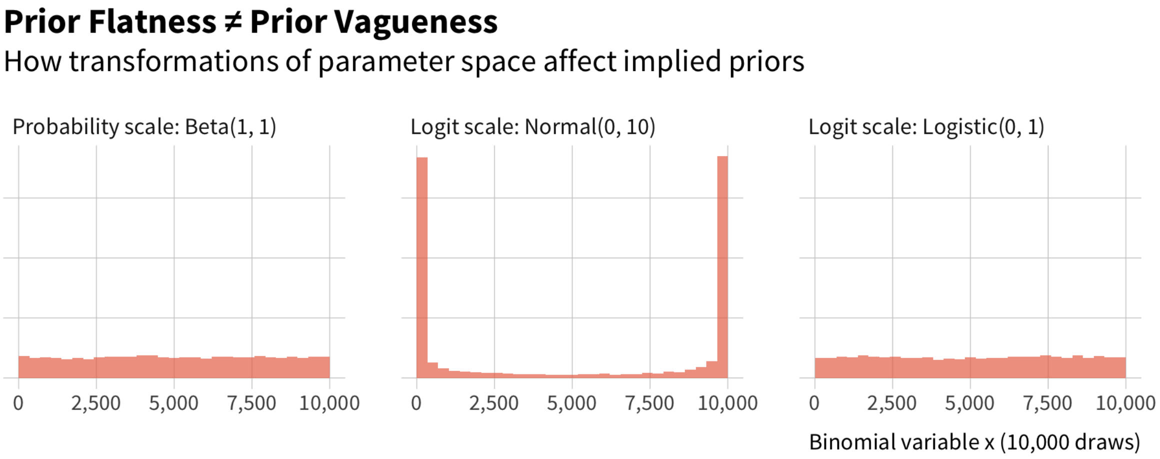
Figure 3.1: Demonstration of centered and non-centered parameterizations for a Normal distribution. The non-centered parameterization is statistically equivalent, but the location and scale are factored out of the distribution.

In order to understand a model, the researcher should understand the consequences of priors on the model's predictive distribution for new data, because prior distributions in parameter space yield implied priors in outcome space. This highlights an ever-present, pragmatic problem in building any model: a researcher has prior information about the *world*, but they must set priors on model parameters. This requires the researcher to solve backward for priors in parameter space that resemble reasonable expectations about the behavior of data. In order to understand and anticipate the consequences of priors, it is important to understand the data's functional dependence on model parameters and how those functions transform probability densities. It is thus a general principal of model-building that "the prior can often only be understood in the context of the likelihood" (Gelman, Simpson, and Betancourt 2017). The close relationship between priors and the data model expose where flat priors create problematic model behaviors.

It is helpful to highlight an example where flat priors, believed "by default" to be reasonable and conservative, create problematic or nonsensical results (Seaman III, Seaman Jr, and Stamey 2012). Consider a binomial outcome variable that counts x successes out of n independent trials, each with success probability α. We want to estimate α, and we don't have specific prior information about it. We represent ignorance about α using a flat $\text{Beta}\left(1, 1\right)$ density. Not consider the identical data but estimating α with a logit model likelihood instead, where $\alpha = \text{logit}^{-1}\left(\eta\right)$. In the logit model, α is a deterministic function of the parameter η, so we place the prior on η. We are ignorant about its value as well, so we follow a default instinct and give it a prior with a wide variance, $\text{Normal}(0, 10)$ on the logit scale, and we might have considered scale values larger than 10 as well to represent more ignorance.

If we take both of these models and generate prior simulations for $x \sim \text{Binom}(n, \alpha)$, depicted as histograms in Figure **??**, the implied prior distributions for x do not resemble one another at all. The first panel shows the implied prior for x when α has a flat Beta prior, resulting in a distribution for x that is also flat. The middle panel shows the implied prior for x from the logit model, where $\text{logit}\left(\alpha\right) = \eta \sim \text{Normal}\left(0, 10\right)$, resulting in a very different prior for x where most prior probability is concentrated on very small or very large values of x. Why does this prior for x appear so informative when we used such a vague prior for η? Because η had a wide prior on the logit scale, and the probability scale is a nonlinear transformation of the logit scale, the resulting prior for x is also affected by the nonlinear mapping between parameter spaces. Only a thin range of logit-scale values map to probabilities that we routinely encounter in political science: logit sclae values between -3 and 3 correspond to probabilities between approximately 0.047 and 0.953. Because the

Normal $(0, 10)$ prior places most probability density outside of reasonably values on the logit scale, most of our implied probabilities are unreasonably small or large as well. In order to obtain a flat prior for x using a logit model, we would actually use the prior $\eta \sim$ Logistic $(0, 1)$, shown in the third panel of Figure **??**. The standard Logistic prior creates a flat density on the probability scale because the inverse link function for a logit model is the cumulative distribution function for a standard Logistic distribution.[11]



- [x] Introduce implied prior
- [x] outcome vs. parameters
- principal: info vs. shape
- example, horiuchi
- lesson for causal inference: we should expect to get different results because thinking explicitly about priors shows us where implied priors aren't flat (and that's fine) or where implied priors ARE flat (and that's batshit).
- we might have vague priors in one space that are informative in another, or one space is easier for specifying priors

Flat priors are problematic for many analyses because they lead to implied priors that have strange behaviors that researchers would be uncomfortable confronting.

[11] This same intuition holds for a standard Normal prior in a probit model, demonstrated in Section 2.5.4.

More complex functions of parameters, especially nonlinear functions, can produce in implied priors with strange shapes that are difficult to anticipate using intuition alone. [horiuchi]

Outcome space != parameter space. Flat priors about outcomes is not the same thing as flat priors about parameters. Information != shape. The mapping from geometric space to statistical information is entirely dependent on the scale of a parameter space, and different model parameterizations invoke different scales.

The primary resistance to Bayesian inference in applied research is the need to set a prior at all. To many researchers, the prior distribution is an additional assumptions that is never feels justified because it is external to the data. Often researchers wish to sidestep this choice altogether, preferring a "flat" prior that prefers all parameters equally.

We have seen so far that the parameterization of a model has consequences for prior specification. Reparameterization may result in an algebraically equivalent likelihood

The incoherence of flatness:

- no universally valid strategy for specifying flat priors because it is always possible to rearrange the data model either by transforming a parameter or otherwise rearranging the likelihood.

Consider an experiment with a binary treatment Z and a binary outcome variable Y. We want to determine the effect of Z by comparing the success probability in the treatment group, π_1, to the success probability in the control group, π_0.

- "no way to conceptualize an uninformative prior because you can always rearrange the problem through a reparameterization or transformation of a parameter"
- examples of transformations having crazy implications/MLE being wild (logit).
- Jeffreys prior: actually a very limited range of priors that satisfy an "invariance" property. My words: such that the "amount of information obtained from data about is invariant

to parameterization of the likelihood, for all possible values of the parameter," or, "the only way for the posterior distribution to be exactly the same, given the same data, for all true parameter values (?), is the Jeffreys prior," or, regardless of the data, I will learn the same thing about the generative model regardless of which equivalent parameterization of the generative model is used.

- is it worth it to think about the theoretical meaning of information
- how does flatness reflect information in nonlinear scales?

Suppose we have some posterior distribution which relies on some parameter vector $\overrightarrow{\alpha}$.

$$p\left(\overrightarrow{\alpha} \mid y\right) \propto p(y \mid \overrightarrow{\alpha})p(\overrightarrow{\alpha}) \tag{3.27}$$

Consider some alternate parameterization of the likelihood parameterized by $\overrightarrow{\beta}$.

Nonlinear transformation of π does not preserve a uniform density over parameters.

Alex meeting takeaways:

- every prior has a "covariant" prior in a different parameterization
- the posteriors will be covariant as well.
- The way you get between them is by transforming the parameter and doing the appropriate Jacobian transformation to the density.
- Jeffrey's priors are a special case of this where the prior is proportional to the determinant of the information matrix. This has the beneficial property of "optimal learning" from the data. For example, flat Beta prior doesn't "hedge toward 50" in quite the same way.

3.3.4 Priors and model parameterization {sec:prior-parameterization}

Priors are defined with respect to a model of the data (the likelihood). We may have priors about the way the world works, but we rarely have priors about model parameters. This is because parameters are an invention in the model. They are mathematical abstractions similar to points and lines, so they only exist when we translate the world into a mathematical language. This means that the mathematical representation of the world is in direct dialog with the choices available to a researcher about how to encode prior information. In the real world, the prior information that I have about the world isn't affected by a mathematical representation of the world. As a researcher, the way I encode prior information depends on the choices I make about that mathematical representation.

One essential feature for understanding prior choices in practice is the *parameterization* of the data model, $p(y \mid \phi)$, for some generic parameter ϕ.[12] We say that a data model has an "equivalent reparameterization" if for some transformed parameter $\psi = f(\phi)$, the function that defines the data model can be rewritten in terms of ψ and return an equivalent likelihood of the data. More formally, the parameterization is equivalent if $p(y \mid \phi) = p(y \mid \psi)$ for all possible y.

In a maximum likelihood framework, equivalent parameterizations are a more benign feature of the modeling framework. Reparameterization may result in likelihood surfaces that have easier geometries for optimization algorithms to explore, but the *value* of the likelihood function is unaffected by the algebraic definition or parameterization of the likelihood function. For instance, a Normally distributed variable x with mean μ could be parameterized in terms of standard deviation σ or in terms of precision $\tau = \frac{1}{\sigma^2}$, but the resulting density is unaffected.

$$\frac{1}{\sigma\sqrt{2\pi}} e^{-\left(\frac{x-\mu}{2\sigma}\right)^2} = \sqrt{\frac{\tau}{2\pi}} e^{-\frac{\tau(x-\mu)^2}{2}} \tag{3.28}$$

The consequence for Bayesian analysis, however, is that the parameterization of the data model determines the set of parameters and their functional relationship to the data.

[12] Bayesian practitioners sometimes refer to the data model as the "likelihood." This can be confusing because the "likelihood function" more traditionally refers to the *product* of the data probabilities under the data model. References to the "parameterization of the likelihood" should be understood as interchangeable with "parameterization of the data model," since the former is determined entirely by the latter.

One example of equivalent reparameterization arises with the different possible ways to write a linear regression model. The first form specifies y_i for unit i as a linear function of x_i with a random error that is mean 0 and standard deviation σ.

$$y_i = \alpha + \beta x_i + \varepsilon_i, \qquad \text{where } \varepsilon_i \sim \text{Normal}\left(0, \sigma\right) \tag{3.29}$$

The second form, more common when viewing linear regression in the framework of generalized linear models, is to express y_i directly as the random variable, with a conditional mean defined by the regression function and standard deviation σ.

$$y_i \sim \text{Normal}\left(\alpha + \beta x_i, \sigma\right) \tag{3.30}$$

Algebraically, these two models are identical. The difference is only a matter of which component has the distributional assumption. In Equation (3.29), the distribution is assigned to ε_i, so y_i is a random variable only by way of ε_i. In Equation (3.30), we assign the distributional assumption directly to y_i, bringing the regression function into the mean rather than "factoring it out" of the distribution.

The linear regression context is one context where the choice of parameterization appears. These two parameterizations are typically called the "centered" and "non-centered" parameterization for a Normal distribution. In the centered parameterization, the random variable is drawn from a distribution "centered" on a systematic component, whereas the non-centered distribution factors out any location and scale information from the distribution, such that the only remaining random variable is a standardized variate. The equations below describe a Normal variable v with mean 3 and standard deviation 2.

$$\text{Centered Parameterization:} \quad v \sim \text{Normal}(3, 2) \tag{3.31}$$

$$\text{Non-Centered Parameterization:} \quad v = 3 + 2z, \qquad \text{where } z \sim \text{Normal}(0, 1) \tag{3.32}$$

Problems of beliefs:

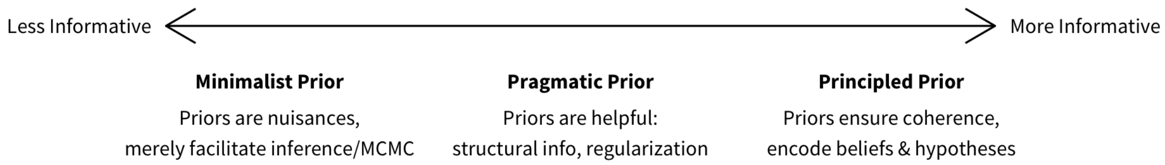


Figure 3.2: A spectrum of attitudes toward priors.

- No degree of belief.
- Parameterization makes this too challenging.
- Prior might change depending on what I ate for lunch.
- "Elicitation" of priors satisfying the wrong audience, or at the very least can be easily misused. We do not want to elicit priors about arcane model parameters. We want to elicit priors about the *world* (Gill Walker)d

Problems of nuisance prior

- parameterization gets you again
- the MLEs are unstable, overfit
- make the regularization argument in-sample

Pragmatic view of priors

- we're between full information and nuisance prior
- Weak information: structure, regularization, identification
- Structural information about parameters
- regularization toward zero (L1, L2), learning by pooling
- stabilizing weakly identified parameters, separation, etc.

Parameters are a *choice*.

- They are part of the *rhetoric* of a model. Sometimes we make pragmatic choices (something is easy to give an independent prior to, but independence isn't always valuable per se). Sometimes we make principled choices (normality, laplace, etc).
- They deserve scrutiny (else just "excrete your posterior") and are a part of the model that you should check and diagnose.
- They aren't merely a nuisance because we can use them to our benefit,
- sometimes when we parameterize a problem to reveal easier things to place convenient priors on

Models are a tool, set it up so that it works.

Constrained parameters in causal mediation?

For instance, consider a simple experiment with a binary outcome variable y_i and binary treatment assignment $z_i \in \{0, 1\}$. Suppose that the treatment effect of interest is a difference-in-means, $\bar{y}_{z=1} - \bar{y}_{z=0}$, estimated from a linear probability model. This linear probability model might be parameterized in two ways. First is a conventional regression setup,

$$y_i = \alpha + \beta z_i + \varepsilon_i \tag{3.33}$$

where α is the control group mean, $\alpha + \beta$ is the treatment group mean, β represents the difference in means, and ε_i is a symmetric error term for unit i. With the model parameterized in this way, the researcher must specify priors for α and β. Suppose that the researcher gives β a flat prior to represent ignorance about the treatment effect. An equivalent *likelihood model* for the data would be to treat each observation as a function of its group mean μ_z.

$$y_i = z_i \mu_1 + \left(1 - z_i\right) \mu_0 + \varepsilon_i \tag{3.34}$$

Although the treatment effect β from Equation (3.33) is equivalent to the difference in means $\mu_1 - \mu_0$ from Equation (3.34), the parameterization of the model affects the implied prior for the difference in means. If the researcher gives a flat prior to both $\mu_{\mathbf{z}}$ terms, the implied prior for the difference in means will not be flat. Instead, it will be triangular, as shown in Figure 3.3. The underlying mechanics of this problem are well-known in applied statistics—if we continue adding parameters, the Central Limit Theorem describes how the resulting distribution will converge to Normality—but it takes the explicit specification of priors to shine a light on the consequences of default prior choices in a particular case. In particular it shows how even flat priors, which are popularly regarded as "agnostic" priors because of their implicit connection to maximum likelihood estimators, do not necessarily imply flat priors about the researcher's key quantities of interest. Rather, flat priors can create a variety of unintended prior distributions that do not match the researcher's expectations. I return to this important idea in the discussion about setting priors for a probit model in Section 2.5.5.

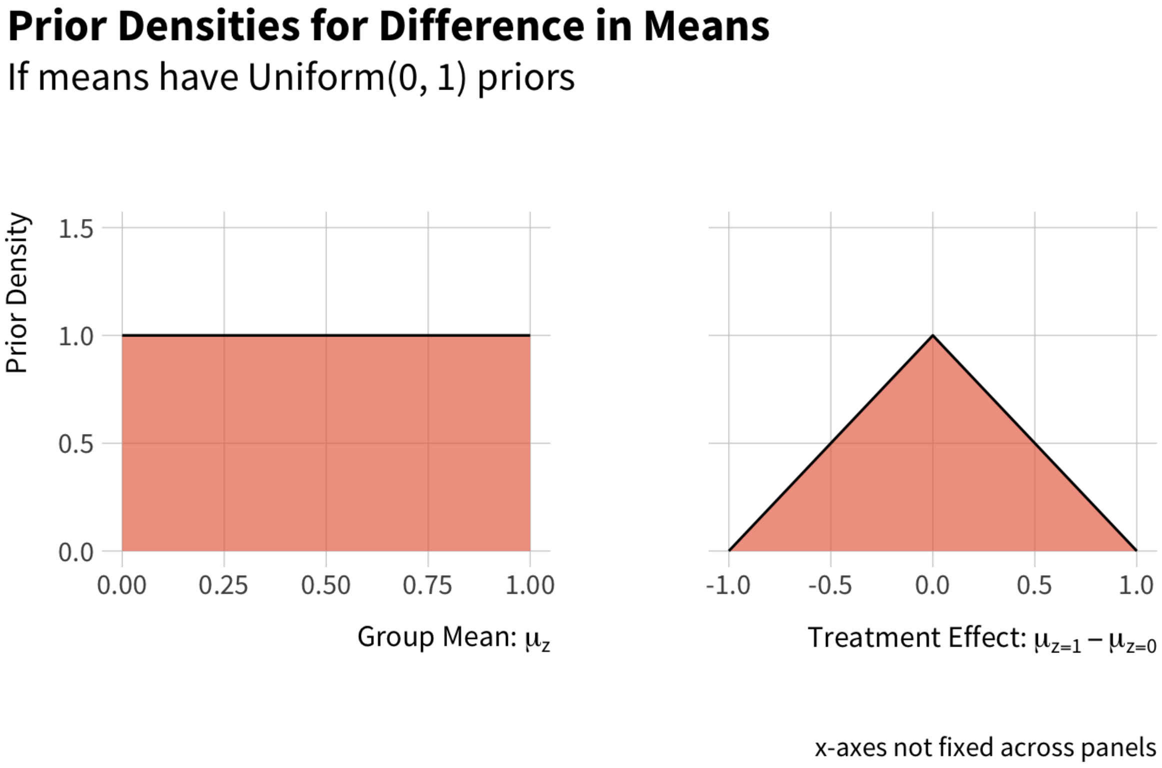


Figure 3.3: Model parameterization affects prior distributions for quantities of interest that are functions of multiple parameters or transformed parameters. The left panel shows that the difference between two means does not have a flat prior if the two means are given flat priors. Note that the x-axes are not fixed across panels.

- equivalent parameterizations

3.3.5 Principled and pragmatic approaches to Bayesian modeling

Various roles that priors take on

- merely facilitate posterior inference
- structural information
- weak information
- regularization / stabilization
- prior knowledge

A general orientation toward priors in this dissertation:

- Not about "stacking the deck" or hazy notions of "prior beliefs"
- information, not belief
- Bayesian view of probability is *more general*, contains information and beliefs. Information is priors, but it's also data. Information is the fundamental unit of uncertainty-quantification
- inference about the thing we care about (counterfactuals)
- structural information when we have it
- Causal inference: "agnosticism" is something valuable generally

Priors are not de-confounders

- downweighting, not upweighting

3.3.6 Statistical Bias

1. posterior isn't about frequency properties (especially in one-off data)

2. What is “bias?”

    - look up in BDA
    - “Requiring unbiased estimates will often lead to relevant information being ignored (as we discuss with hierarchical models in Chapter 5)” (94)

- Why would we want this? Inference makes more sense.
    - What’s the probability of a model/hypothesis, given the data
    - vs. What’s the probability of “more extreme data” (?) given a model that I do not believe.
- posterior probabilities mean what they say they mean
    - conditioning on data (and implicitly the model), this is the distribution of parameters
    - p-values are the “probability of more extreme results.” They condition on the model, but they’re only useful if they do not.
- Proper frequentist analysis is violated as soon as you look at the data.
- Frequency properties are still possible for Bayesian estimators, but we view frequency properties as a byproduct of something more essential (MSE).

3.3.7 Structural Priors and Weak Information

Structure (bounds), regularization (L1, L2), hierarchy

p doesn’t care about your n.

3.3.8 Understanding Log Prior *Shape*

This is low-key pretty big

3.3.9 Models for Means

In many situations, we can get around a data-level model if we want to.

3.3.10 Generalization, Big and Small

Models (likelihoods) are priors

- we restrict the space of all other models
- could think about "flexible" models, but these are just priors over more spaces

Identification assumptions are priors

Generalization to any population is a prior

- priors are not MY data, but ANY data
- parameters describe ANY data
- Lampinen Vehtari 2001: likelihood as "prior for the data" is the basis for all generalization from any finite model

We are always doing violence, but the framework lets us build out more and more general models to structure our uncertainties

Email to David:

> My first reaction to this is like this: it's probably correct to say that Bayesians may have some shared ideas about how to think about generalizing that might differ systematically from non-Bayesians, but I am not sure how much of that is because of Bayes per se so much as just…the types of modeling someone is willing to do. By "modeling" I mean, functional form assumptions are you willing to make about data, which is different from "Bayes" in that the former is something required of all modeling and the latter is only what you can say

about parameters. For example, a functional modeling thing you might do is specify (using some weights or something) how an estimate in one sample might map to another sample, but whether you do that with Bayesian estimation is a separate choice aside from the functional model itself. That being said, even though functional modeling things can be done with Bayes/non-Bayes point of view, it does seem right to say that certain modeling approaches may feel more natural in a Bayesian framework, or that Bayesians might construe a problem slightly differently because they are more used to hierarchical modeling.

That's a pragmatic view of things, but I can give you a more theoretically abstract view, and think of situations where Bayesian lets you do things that non-Bayes can't. It all comes down to how "seriously" you want to take the tenets of Bayesian work and what kind of generalization-based claims you want to make. So I will lay out a series of vignettes that start from a world where Bayes is "not unique" and then gets into worlds where Bayes gets more and more necessary to say what you want to say about the out-of-sample world. I will use the example of estimating some parameter, but you can translate this into learning about a "mechanism" however Erica and Nick are defining that, and you can think about it non-statistically as well even if I'll use statistical modeling language.

I estimate some parameter μ in a study, but it's one instance of a more general phenomenon. If the study were representative of the world, you can imagine that the effect is one instance of the "population effect" and give it a hierarchical prior as such: $\mu \sim D(\theta, \sigma)$ where D() is some distribution, θ is the "general" effect. If I learn about μ (the estimate and standard error σ), I learn about θ! Choice of distribution depends on your assumptions about the stochastic process at work. naturally, but that logic works basically just like a likelihood function choice.

(In fact, Bayes sees priors for parameters as mechanistically no different from likelihoods for data. Which is to say, MLE models like logit are simply hierarchical priors on the data, and regression is estimating the hierarchical parameters of the prior.) This is basically a meta-analysis setup using Bayesian language: if you want tangible examples you can look at the Don Rubin "eight schools experiment" which is about generalizing from parallel studies in schools, or Rachael Meager (sp) has a paper applying this setup to micro-credit experiments in the development econ context: what do we learn about the "overall effect of microcredit expansion" by assimilating information from different studies. In this sense the priors are just ways to structure the meta-analysis.

If the study is unrepresentative or "not externally valid" then it's up to the researcher to specify some approach to modeling the invalidity: $\theta \sim D(f(\theta), \sigma)$, where f() is some function that distorts the representativeness of the study. Which is to say, $f(\theta)$ is the expectation for a study with these distortions. Researcher's task is then to learn the form of f(). These distortions might be like sample bias, the country where the study was performed, or whatever, and all you're really doing is reweighting or adjusting the estimate to make more sense for the target population. If you can estimate parameters that determine f(), viola you can infer the posterior distribution for the true θ. But this is what I mean when I say that none of this is really EXCLUSIVELY Bayesian. Reweighting/adjusting happens in non-Bayes world all the time; the main thing that is different is how you write the model and your ability to say that the population estimate is a "posterior of the true parameter, given the information learned from the data." One example of this kind of thing is maybe the multilevel regression and poststratification: we use national surveys to model the attitudes of different demographic groups, and then we use those model predictions plus census information to project estimates for smaller units, for example states or counties, based on the demographic composition of those units! This example goes the other direction from where Erica and Nick want to go (from representative to unrepresentative) but the technology sounds similar: estimate something in the data that you have, and map it into a space where you do not have data. MRP in political science comes from Bayes world and feels natural there, but nothing saying that it HAS to be Bayesian in its overall approach.

Now we get to a world where Bayes is more necessary. If there's something TRULY Bayesian that really makes no sense withoutBayes, it is the fact that I actually do not need data about f() in order to estimate θ. This is because I have priors even in the absence of data. If all I have is priors about f(), then even if I collect data about ONLY μ and NOTHING about f(), I nonetheless update my information about θ. This is because the parameters are functionally related through f(), so if I learn about the subsample then I learn about the population. Stated differently, learning about μ restricts the space of θ because I can basically "solve backward" using my priors about f. This is the stuff that is very natural in Bayes, and I can think of basically no analog in non-Bayes that lets you do something similar (other than picking point estimates for unknowns and simulating, which doesn't have the same theoretical coherence as a prior/posterior distribution). Of course, this means that inference on f() is subject to the priors that go into f(), which is exactly the kind of thing that non-Bayesians are super afraid of despite majorly misconstruing how this works (IMO). For one, the functional form of f() is the kind of thing non-Bayesians would make assumptions about anyway, so that's not unique to Bayes at all. And secondly, the priors that would go into f() would usually be generic enough that researchers aren't "picking their hypothesis" (a common and frankly stupid stereotype) so much as restricting the space of f() to rule out stuff that's frankly impossible. Happy to give you more concrete examples of the kind of "weakly informative priors" that someone would use in a situation like that if it's a route you want to dig into more. It's this kind of stuff that I think non-Bayesians are under-utilizing: how much extrapolation power you get by being willing to place even weak priors on stuff you can't exactly identify with data. And if you REALLY want to give a nod to Bayesian views of extrapolation, this is the area you'd want to dig into, because it's the stuff that doesn't really make sense without Bayes. You can sort of see Ken and I do this in our voter ID paper (which you can find on my website) though we kind of wimp out of fully placing priors on f().

Here's where things get really abstract because, gun to my head, we can be really scorched-earth and say that all extrapolation falls apart without a Bayesian notion of priors. Think about any model for data: $y \sim D(\theta)$, I think my data come from this distribution, and if I were to go out into the world and collect new data, my estimate for a new data point is characterized by this distribution assumption i.e. this prior for the data. If you try to lay out a formal definition of what "generalization" is, I would say that there is no such thing as generalization without an implicit prior that links your observed data to unobserved things that you want to project into. There are stats theorems out there called "no free lunch" theorems that basically say "all statistical inference is limiting the space of models that link parameters to data, and there is no way to improve your guess for a new data point using a model except to impose prior information on the system by way of the model." So this would be a hard line view of what priors mean in a philosophy of science (not necessarily quantitative or statistical, mind you), but that if you accept that view it trickles down into the more minor examples in a very natural way: the only way to generalize is by using priors to structure the connection between what I do observe and what I do not observe.

3.3.11 Regularization and Prediction Problems

One-Off Data Collection:

1. posterior isn't about frequency properties
2. What is "bias?"
    - look up in BDA
    - "Requiring unbiased estimates will often lead to relevant information being ignored (as we discuss with hierarchical models in Chapter 5)" (94)

- Why would we want this? Inference makes more sense.
    - What's the probability of a model/hypothesis, given the data
    - vs. What's the probability of "more extreme data" (?) given a model that I do not believe.
- posterior probabilities mean what they say they mean
    - conditioning on data (and implicitly the model), this is the distribution of parameters
    - p-values are the "probability of more extreme results." They condition on the model, but they're only useful if they do not.
- Proper frequentist analysis is violated as soon as you look at the data.
- Frequency properties are still possible for Bayesian estimators, but we view frequency properties as a byproduct of something more essential (MSE).

3.3.12 Regularization-Induced Confounding

This is a huge, underappreciated problem in the broad ML-for-causal-inference world

3.4 Bayesian Opportunities

3.4.1 Priors for Imperfect Identifiability

Relatedly: priors over models

"The Bayesian approach also clarifies what can be learned in the noncompliance problem when causal estimands are intrinsically not fully "identified." In par- ticular, issues of identification are quite different from those in the frequen- tist perspective because with proper prior distributions, posterior distribu- tions are always proper." (Imbens and Rubin 1997)

This is where Imbens and Rubin push (also Horiuchi et al. example)

Randomization limits the impact of the Bayesian assumptions

- "Classical random- ized designs stand out as especially appealing assignment mechanisms designed to make inference for causal effects straightforward by limiting the sensitivity of a valid Bayesian analysis." (Rubin 1978)

Casting these identification assumptions as priors has at least two distinct benefits over an approach with fixed sensitivity parameter values. Firstly, it allows the researcher to conduct inference about the treatment effect by *marginalizing over* their priors for design parameters, rather than conditioning on fixed design parameters. By marginalizing over the design parameters, the researcher obtains one posterior distribution that averages over their prior design uncertainty, rather than an arbitrary range of design parameter values that contains no information about which design parameter values are realistic or unrealistic. Secondly, and relatedly, marginalizing over design parameters speaks to the inferential question that is more consistent with the researcher's guiding question: which treatment effects are consistent with the data and our prior information. Sensitivity analysis using fixed parameters, meanwhile, answers a different question that is less useful for the goal of posterior inference about treatment effects: how big of an assumption violation is required in order for treatment effect estimates to be statistically insignificant, with no mention of whether violations of that size are likely or unlikely.

returns a quantity of interest that is more consistent with the researcher's ultimate inferential goal—what treatment effects are consistent with prior information and the data—rather than a quantity

Many identification assumptions can be encoded as functional form assumptions in an estimated model. Consider a regression scenario with outcome Y_i, treatment A_i, and some other covariate U_i.

$$\begin{aligned} \text{True model: } Y_i &= \alpha + \tau A_i + \beta U_i + \varepsilon_i \\ \text{Estimated model: } Y_i &= \hat{\alpha} + \hat{\tau} A_i + e_i \end{aligned} \tag{3.35}$$

If we invoke an identifying assumption that the assignment of A_i is ignorable, then our estimate for the average effect of A_i using the second line of (3.35) is unbiased. This is equivalent to an assumption that the correlation between the treatment A_i and the true error term ε_i is exactly zero. For causal inference purposes, researchers have developed sensitivity tests that recover the treatment effect under ignorability violations, which work by picking a non-zero error correlation value and deriving the treatment under that model (Acharya, Blackwell, and Sen 2016; Imai et al. 2011).

, and building a richer understanding of how inferences depend on modeling assumptions.

Researchers have derived estimators for τ in the presence of a, which can be used in a causal inference context to test the "robustness" or "sensitivity" of inferences to the ignorability assumption (Acharya, Blackwell, and Sen 2016; Imai et al. 2011). If this correlation is zero, the treatment effect estimate is unbiased These are

In a causal inference context, researchers sometimes develop sensitivity tests to test the robustness of their inference to violations of ignorability assumptions (Acharya, Blackwell, and Sen 2016; Imai et al. 2011), exclusion restrictions (Nyhan, Skovron, and Titiunik 2017), and so on. Because identification assumptions are special cases of these more general models with additional nuisance parameters, it would be straightforward in a Bayesian framework to specify prior distributions on nuisance parameters that represent priors for violated assumptions. Unpublished work by Leavitt (2020) explores this approach in a difference-in-difference contexts, introducing an "epistemic" source of variance in his treatment effect estimate by specifying a prior on the parallel trends assumption.

- Oganisian and Roy (2020) "identification" assumptions vs. "statistical" assumptions. Identification assumptions get you to a place where you can express causal effects in terms of expectations about Y given treatment, within confounding strata (perhaps then averaging over confounders). "Statistical" assumptions define how we think we can build $E[Y]$

3.4.2 Application: Weakly Informed Regression Discontinuity

This section presents a reanalysis of Hall's (2015) regression discontinuity study of congressional elections. Portions of the original analysis contain a pathological result where confidence intervals for key parameters of interest contain values that could not possibly occur with nonzero probability. We overcome the pathology using weakly informative priors that contain structural information about the dependent variable only, excluding impossible parameters from the prior but uninformative over possible parameter values. This minor prior intervention successfully guides the posterior distribution away from impossible regions of parameter space, resulting in a posterior distribution that is consistent with the data as well as external structural information about the problem. This intervention does not undermine the main takeaway from the original study, but the Bayesian estimates for the effect of interest are notably smaller and more precisely estimated.

With similar aims to this project, Hall (2015) examines primary elections and their impact on ideological representation in Congress. The study asks if extremist candidates for Congress are more likely or less likely to win the general election contest than candidates who are relatively moderate by comparison. The treatment variable of interest, the ideological extremity of a party's general election nominee, is confounded by several factors. Competitive districts may lead more moderate candidates to run in the first place, creating selection biases for which candidates represent which district. Conversely, voters in electorally "safe" districts may feel freer to nominate more extreme candidates because the likelihood of their party losing the seat in the general election is sufficiently low. Furthermore, the incumbency advantage in general elections confounds this picture if incumbents tend to be more moderate than challengers who are angling to raise their name recognition (Gelman and King 1990).

To identify the effect of candidate ideology, Hall (2015) leverages the vote margin in the primary election as a forcing variable in a regression discontinuity design (RDD). In a primary contest between a relative extremist and a relative moderate, the extremist advances to the general election if their vote share in the primary is greater than the moderate's, i.e. the extremist's *margin* (difference) over the moderate is any greater than 0. If the extremist's primary margin is any less than 0, the moderate advances to the general election instead. This primary margin deterministically assigns congressional candidacies to treatment or control if the extremist wins or loses the primary, respectively. While candidate ideology's effect on general election outcomes may be confounded in the aggregate, the effect can be identified at the threshold (extremist margin of 0). The key identification assumption for a "sharp" regression discontinuity design is that the forcing variable, X_i, and the expected outcome given the forcing variable, $\mathbb{E}\left[Y_i(x) \mid X_i\right]$, are both continuous at the threshold x_0. This assumption identifies *local* treatment as the difference in the limits of the conditional expectations for treatment ($X = 1$) and control ($X = 0$) at the threshold (Calonico, Cattaneo, and Titiunik 2014; Skovron and Titiunik 2015).

$$\lim_{x \downarrow x_0} \mathbb{E}\left[Y_i \mid X_i = x\right] - \lim_{x \uparrow x_0} \mathbb{E}\left[Y_i \mid X_i = x\right] = \mathbb{E}\left[Y_i(1) - Y_i(0) \mid X_i = x_0\right] \tag{3.36}$$

Equation (3.36) implies that the difference in potential outcomes can be identified from observed data only by observing that everything else about units is continuous at the threshold except for the realized treatment value.

Hall (2015) applies the RD design by assuming that the effect of candidate ideology on vote share and win probability in the general election are identified locally where the extremist's margin in the primary election crosses 0. For this example, we concentrate on models that predict win probability, since these are the estimates that contain pathological results that we can avoid with Bayesian methods. Hall estimates RD models using a few different specifications, but I replicate his simplest design, which is a linear probability model (LPM) of the following form. The local linear regression is justified by the limit intuition of the key assumption in (3.36); any nonlinear regression function, as long as it is continuous at the cutoff, converges to linearity at the cutoff in the limit. Data were obtained from Hall's replication materials, available on his website.[13] The outcome y_{dpt} is a binary indicator that takes the value 1 if the general candidate running in district d for party p in election year t wins the general election, and it takes 0 if the candidate loses the general election,

$$\begin{aligned} y_{dpt} &= \beta_0 + \beta_1(\text{Extremist Wins Primary})_{dpt} + \beta_2(\text{Extremist Primary Margin})_{dpt} \\ &\quad + \beta_3(\text{Extremist Wins Primary} \times \text{Extremist Primary Margin})_{dpt} + \varepsilon_{dpt} \end{aligned} \tag{3.37}$$

where *Extremist Primary Margin* is the extremist candidate's margin over the moderate candidate with the highest vote in the primary, and *Extremist Wins Primary* is a binary indicator equaling 1 if that margin exceeds 0, and ε_{dpt} is an error term. When the extremist margin

[13] http://www.andrewbenjaminhall.com/, last accessed July 02, 2020.

exceeds 0, the candidate representing case dpt is the extremist, otherwise the candidate representing dpt is the moderate. The coefficient β_1 represents the intercept shift associated with the extremist primary win, estimating the treatment effect of candidate extremism at the discontinuity. I replicate this LPM using ordinary least squares, and I also create a Bayesian equivalent using an algebraically reparameterization. The Bayesian parameterization has the same linear form, but instead of specifying two lines an interaction term, I subscript the coefficients by w, which indexes the treatment status (*Extremist Wins Primary*),

$$\begin{aligned} y_{dpt} &= \alpha_{w[dpt]} + \beta_{w[dpt]}(\text{Extremist Primary Margin})_{dpt} + \varepsilon_{dpt} \\ \varepsilon_{dpt} &\sim \text{Normal}\left(0, \sigma\right) \end{aligned} \tag{3.38}$$

where α_w is an intercept for treatment status w, and β_w is the slope for treatment w. This parameterization implies two lines, one line for $w = 0$ and another line for $w = 1$. The treatment effect at the discontinuity is the difference between the intercepts, $\alpha_1 - \alpha_0$. This parameterization will be helpful for extending the model below.

I plot the OLS win probability estimates around the discontinuity in Figure 3.4. At the discontinuity, we estimate that extremism decreases a candidate's win probability by 0.53 percentage points, which is the same effect found by Hall (2015). The original publication lacks a graphical depiction of these results. Our visualization of the RD predictions reveal that the confidence set for the parameter estimates that compose the treatment effect contain many values that would be impossible to observe. The point estimate for average moderate candidate win probability at the discontinuity is 0.95, which is a possible number to obtain, but the 95 percent confidence intervals includes values as high as 1.24, which far exceeds the maximum possible value of 1.0.

This pathology is possible in any LPM with finite data, but there are a few pragmatic reasons why we might not worry about it. First, for fully saturated model specifications, predicated probabilities from a model LPM are unbiased estimates of the true probabilities,

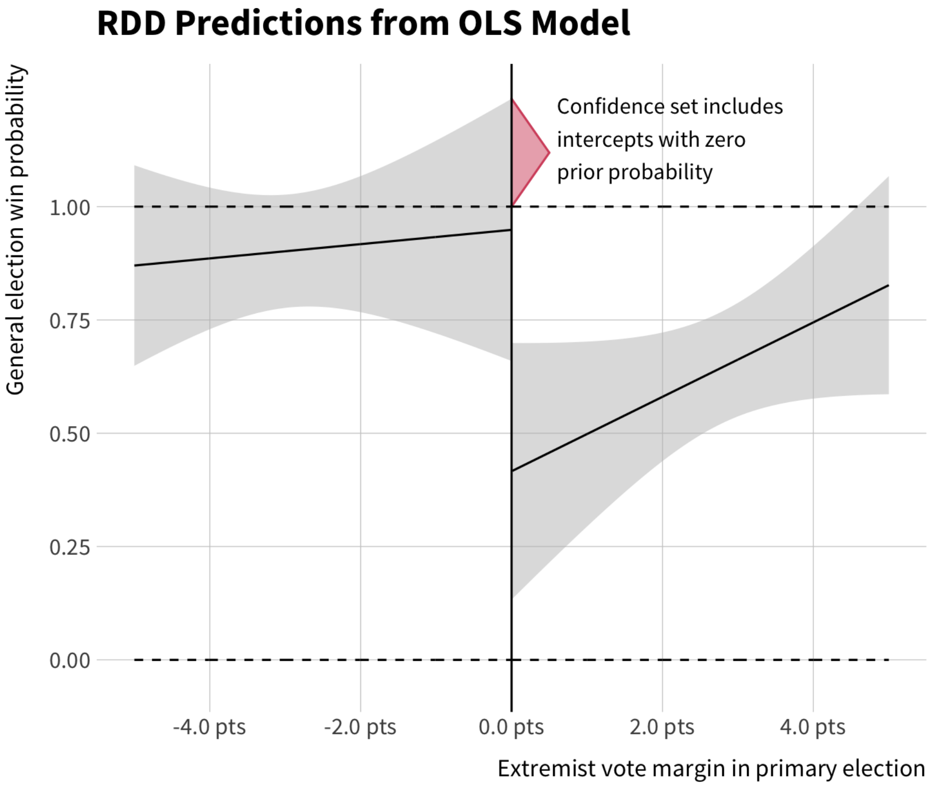


Figure 3.4: OLS estimates of the predicted probability that a candidate wins the general election. Local average treatment effect is estimated at the threshold. Treatment effect is defined by parameter estimates whose confidence sets contain values with no possibility of being true.

and thus are an unbiased estimate of the treatment effect of interest. For frequentist inference, constructing a 95 percent confidence interval on this unbiased estimate might be enough to suit the researcher's needs. In this particular case, however, these reasons may not satisfy our goals. First, the model isn't fully saturated. Because the design employs a local linear regression, the extrapolation of the regression function to the threshold is model-dependent (Calonico, Cattaneo, and Titiunik 2014). It makes sense, then, to build a model that constrains those extrapolations only to regions of parameter space that are mathematically possible for the problem at hand. Furthermore, the repeated sampling intuition of the frequentist approach does not guide our inferences because the data in the analysis are the population of interest. We have no ability to repeatedly sample this data generating process, so our uncertainty about our inferences must come from some other mechanism. Most importantly, because the intercept estimates are essential for defining the treatment effect of interest, the degree to which this one estimate is corrupted presents a significant problem for the inferences we can draw from the analysis.

To visualize just how much posterior probability this model places in impossible regions of parameter space, Figure 3.5 shows a histogram of posterior samples for the treatment effect from the Bayesian version of this model using (improper) flat priors on all parameters. Because flat priors do nothing to concentrate prior probability density away from pathological regions of parameter space, a large proportion of posterior samples contain intercept estimates that do not and cannot represent win probabilities. Of the 8,000 posterior samples considered by this model fit, 36% of the non-extremist intercepts are "impossible" to obtain because they are greater than 1 or less than 0. A small number of MCMC samples for the extremist intercepts take impossible values as well. As a result, just 64% of MCMC samples for the treatment effect is composed of parameters that are mathematically possible. Even invoking the practical benefits of the LPM, such a high level of corruption in the most important quantity of this analysis is cause to rethink the approach.

The Bayesian approach begins with structural prior information about the intercepts estimated at the discontinuity. In particular, we specify a prior that these constants can only take values in the interval $[0, 1]$. We remain agnostic as to which values within that interval are more plausible in the prior. The result is a uniform prior over possible win probabilities, which we apply to both intercept parameters.

$$\alpha_{w=0}, \alpha_{w=1} \sim \text{Uniform}\left(0, 1\right) \tag{3.39}$$

The structural information in this prior is indisputable. We know with certainty that no probability can be less than 0 or greater than 1. Accordingly, this prior concentrates probability

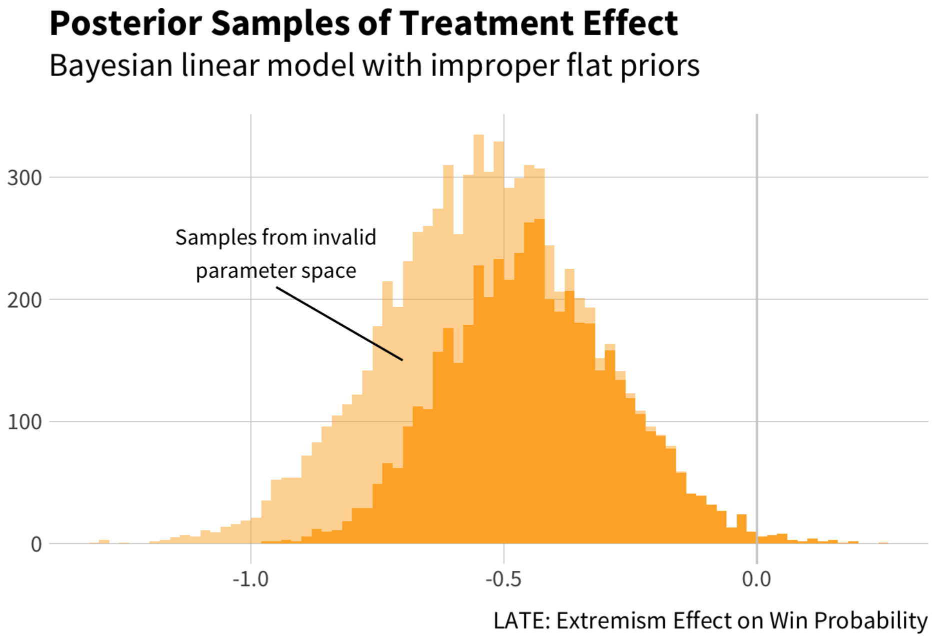


Figure 3.5: Histogram of posterior samples for the treatment effect. Using flat priors for each win probability intercept, a substantial share of the treatment distribution consists of parameters that cannot possibly occur.

density away from treatment effects that cannot be true, while maintaining the local linear specification that is justified by the limiting intuition of the key identification assumption. Because we give flat priors to the individual intercepts rather than the treatment effect itself, the implied prior for the treatment effect inherits the triangular shape introduced above in Figure 3.3, which is vague despite not being flat.

We complete the model by specifying distributions for the outcome data and the remaining parameters.

$$\begin{aligned} y_{dpt} &\sim \text{Normal}\left(\alpha_{w[dpt]} + \beta_{w[dpt]}(\text{Extremist Primary Margin})_{dpt}, \sigma\right) \\ \beta_{\text{w}} &\sim \text{Normal}\left(0, 10\right) \\ \sigma &\sim \text{Uniform}(0, 10) \end{aligned} \tag{3.40}$$

The Normal model for the outcome data in the first line is equivalent to the Normal error term defined in (3.38). The priors for the β_w slopes and residual standard deviation σ are very diffuse given the scale of the outcome data, $\{0, 1\}$, and the running variable that only takes values in the interval $[-5, 5]$, a bandwidth of ±5 percentage points around the threshold.

A possible retort to this model setup is a Bayesian approach would be entirely unnecessary if instead we employed a binary outcome model like logit or probit regression. These models are typically used to estimate probabilities underlying binary data in other contexts, so we entertain it here as well. Although this model contradicts the limiting intuition that the regression function is instantaneously linear at the discontinuity (as any function is instantaneously linear for an infinitesimal change in its input), I indulge this possible retort by building a Bayesian logit specification as well. This setup considers the binary election result as a Bernoulli variable with a probability parameter specified by a logit model,

$$y_{dpt} \sim \text{Bernoulli}\left(\pi_{dpt}\right) \tag{3.41}$$

$$\text{logit}\left(\pi_{dpt}\right) = \alpha^{*}_{w[dpt]} + \beta^{*}_{w[dpt]}(\text{Extremist Primary Margin})_{dpt} \tag{3.42}$$

with parameters denoted α^{*}_{w} and β^{*}_{w} to distinguish them from the α_w and β_w parameters in the linear setup.

Although this logit specification constrains all win probability estimates to fall in the appropriate region, specifying priors for logit models is more challenging because regression parameters are defined on the log-odds scale instead of the probability scale. Fortunately for the case of regression discontinuity, the treatment effect is defined at the threshold where the running variable is 0, so our prior for the treatment effect can be constructed in a region of parameter space where the running variable and its coefficients have dropped from the equation.

$$\begin{aligned} \text{logit}\left(\pi_{dpt}\right) &= \alpha^{*}_{w[dpt]}, \text{ at Extremist Primary Margin}_{dpt} = 0 \\ \text{which implies } \pi_{dpt} &= \text{logit}^{-1}\left(\alpha_{w[dpt]}\right) \end{aligned} \tag{3.43}$$

If we want to construct a prior for the treatment effect that is similar to the structural information we encoded in the linear specification, we must specify priors for the extremist and non-extremist win probabilities that are flat over valid probability values at the discontinuity. This requires a prior for α_w^* on the log-odds scale that implies a flat prior for $\text{logit}^{-1}(\alpha_w^*)$ on the probability scale. To solve this problem, we leverage the logit model's connection to the standard Logistic distribution. The logit function maps values in the $(0, 1)$ interval to any real number, and the inverse logit function maps any real number to $(0, 1)$. We accomplish a flat prior for win probabilities at the threshold using a a standard Logistic prior on the log-odds scale,

$$\alpha_w^* \sim \text{Logistic}(0, 1) \tag{3.44}$$

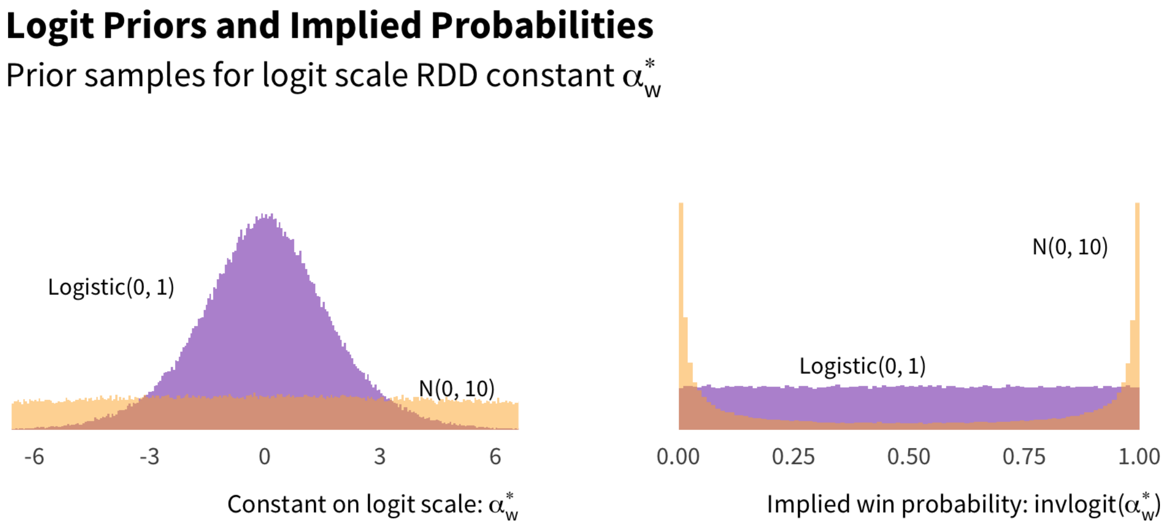
which becomes a flat density for $\text{logit}^{-1}(\alpha_w^*)$. It is startling at first to consider a prior as narrow as $\text{Logistic}(0, 1)$ as an uninformative prior for a key parameter. But at discussed in Section 3.3.3, the connection between prior vagueness and prior flatness is not absolute. Flatness is only a shape. The relationship between flatness and informativeness depends on model parameterization and the scale of the data.

Figure 3.6 visualizes how the Logistic prior for the intercept on the log-odds scale becomes a flat prior on the probability scale at the threshold. The left panel shows a histogram of $\text{Logistic}(0, 1)$ simulations, and the right panel shows a histogram of the same values after they are converted to probabilities using the inverse logit function. For comparison, I also simulate a $\text{Normal}(0, 10)$ prior, which is something a researcher might pick if they wanted to be vague on the log-odds scale. Converting the wide Normal prior to the probability scale, however, shows that greater prior density on logit values far from zero translates to greater prior density over probability values very close to 0 and 1.

The fact that the wide Normal prior has strange behavior on the probability scale does not mean that it shouldn't be used in Bayesian logistic modeling. It could be an appropriate choice for specifying priors for constructs that should be understood directly on the logit scale. For instance, I give this exact prior to the slope parameters in this RDD logit model,

$$\beta_w \sim \text{Normal}\left(0, 10\right) \tag{3.45}$$

because I want the prior to consider a broader distribution slopes *on the logit-scale*. The lesson with these priors, as with any prior, is that prior distributions should be chosen to suit the modeling context. Elements of that context include link functions, model reparameterization, the scaling of outcome data or covariates, regularization concerns, and so on. Choosing "default priors" that always encode flatness on one scale has no guaranteed behavior for implies priors for important functions of parameters.


Logit Priors and Implied Probabilities

Prior samples for logit scale RDD constant α^*_w

Figure 3.6: Scale invariance of logit model priors. Standard logistic prior on logit scale becomes a flat prior on the probability scale. "Diffuse" priors on logit scale imply priors on probability scale that bias toward extreme probabilities.

These prior interventions in both the Bayesian LPM and the Bayesian logit are minor. They merely encode structural information about the outcome scale. Win probabilities for extremists and non-extremists are constrained to take valid values—between 0 and 1—but the prior is otherwise agnostic about which win probabilities are more likely than others before seeing any data. What effect do these minor interventions have? Figure 3.7 plots the results from these three Bayesian models: the problematic original model with improper flat priors, the Bayesian LPM with structural priors to constrain the intercepts, and the logit model that creates the structural prior using the transformed Logistic distribution. The left panel shows a histogram of posterior MCMC samples for the non-extremist win probability at the threshold. The LPM at the top of the panel included no parameter constraints whatsoever. As a result, we see the pathological behavior where the posterior distribution places positive density on win probabilities that we know with certainty to be impossible to obtain. The histograms in the second and third rows show the LPM and logit with the structural prior. Both models concentrate prior density on possible win probabilities only, resulting in posterior distributions that reflect prior information better than the unconstrained model. The posterior distributions are asymmetric and place a lot of posterior density at high win probabilities, but this should not alarm us. The asymmetry in the distribution reflects the signals obtained from the data, rationalized against weak information encoded in the prior. The asymmetry is direct indicator of the way Bayesian priors added value to the analysis.

The right panel of Figure 3.7 shows how these parameter constrains ultimately manifest in our LATE estimates by plotting posterior means and 90 percent compatibility intervals for each model. As with nearly all Bayesian modeling approaches, our priors have the effect of shrinking important effects toward 0 and reducing the variance of the effect. In this particular case, the posterior mean for the local average treatment effect shrinks from -0.53 with flat/unconstrained priors to -0.44 using the LPM with constrained intercepts: a 17% reduction in the magnitude of the effect. The LATE from the Bayesian logit is , which is a

reduction in magnitude. This shrinkage comes from the fact that some of the largest treatment effects in our original posterior distribution were composed of impossible parameters. This manifested earlier in Figure 3.5, which showed that larger treatment effects were more likely to contain pathological parameters than the smaller treatment effects. The standard deviation of the posterior samples is reduced for the models with structural prior constraints, so these prior interventions are also improving the precision of our estimates. This is because a fair amount of posterior uncertainty in the unconstrained model was owed to impossible parameter values.

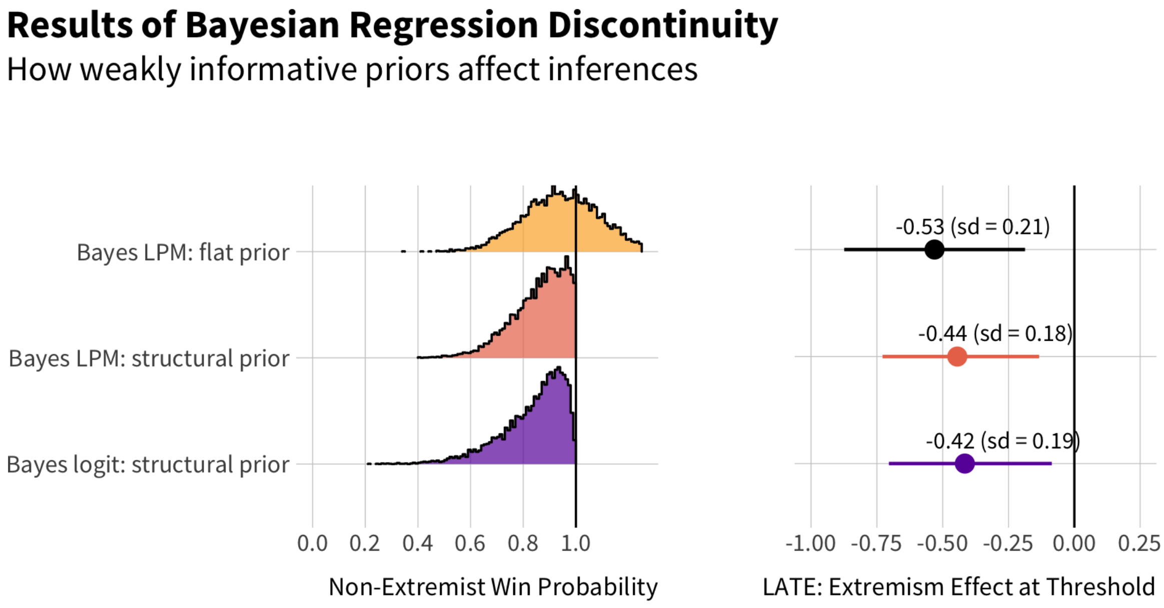


Figure 3.7: Comparison of posterior samples from Bayesian models with improper flat priors and weakly informative priors. Weakly informative models have flat priors over valid win probabilities at the discontinuity. Priors reduce effect magnitudes and variances by ruling out impossible win probabilities.

It bears emphasizing that the prior interventions in this case study were no more controversial than declaring what is already known: probabilities lie between 0 and 1. Since many causal research designs estimate treatment effects on binary variables, and many causal research designs are limited to small numbers of relevant real-world observations or budget-limited experimental samples, simple interventions like this have the potential to substantially improve the precision of research findings in contexts where researchers do not realize how much information they are leaving out of their analyses.

3.4.3 Models for Nonparametric Treatment Effects with Applications to Meta-Analysis

3.5 Other Frontiers of Bayesian Causal Inference

3.5.1 Beyond Estimation: Inferences About Models and Hypotheses

We can think bigger about Bayesian *inference* for a parameter as distinct from Bayesian *estimation* of the in-sample quantity. This lets us use a nonparametric data-driven estimator for the data, but the "inference" or "generalization" still has a prior. For instance a sample mean estimates a population mean without a likelihood model for the data, but inference about the population mean often follows a parametric assumption from the Central Limit Theorem that the sampling distribution from the mean is asymptotically normal (but doesn't have to, c.f. bootstrapping). Even if the point estimator we use for a mean is unbiased, we can assimilate external information during the interpretation of the estimator (biasing the inference without biasing the point estimator). Restated: the posterior distribution is a weighted average of the raw point estimator and external information, rather than biasing the data-driven estimate directly.

Even bigger: Bayesian inference about *models* (Baldi and Shahbaba 2019). This is probably where I have to start my justification for this? The *entire point* of causal inference is to make inferences about counterfactuals given data (Rubin 1978?). Invoking Bayesian inference is really the only way to say what we *want* to say about causal effects: what are the plausible causal effects given the model/data. We do *not* care about the plausibility of data given the null (as a primary QOI). - Probably want to use Harrell-esque language? Draw on intuition from clinical research, or even industry. We want our best answer, not a philosophically indirect weird jumble. - This probably also plays into the Cox/Jeffreys/Jaynes stuff I have open on my computer.

This presumes an m-closed world(?right?), which maybe we do not like (Navarro, "Devil and Deep Blue Sea"). Me debating with myself: how to think about Bayesian model selection vs "doubly robust" estimation ideas…

Inherit material from earlier section (baldi paper)

Quasi-experiment paper (LR/BF of two models)

Conventional:

$$p(\theta \mid \mathbf{y}) = \frac{p(\mathbf{y} \mid \theta)p(\theta)}{p(\mathbf{y})} \tag{3.46}$$

$$p(\theta \mid \mathbf{y}) \propto p(\mathbf{y} \mid \theta)p(\theta) \tag{3.47}$$

Implied:

$$p(\theta \mid \mathbf{y}, \mathcal{H}) = \frac{p(\mathbf{y} \mid \theta, \mathcal{H})p(\theta \mid \mathcal{H})}{p(\mathbf{y} \mid \mathcal{H})} \tag{3.48}$$

3.5.2 Priors are the Basis for all Generalization

No-Free-Lunch theorems

3.5.3 Agnostic Causal Inference

Sidestepping priors

complexity of bayes vs. parsimony of causal inference NOT A RULE

Causal doesn't imply nonparametric, Bayesian doesn't imply complex

At any rate:

- simple case: sensitivity testing for noisy circumstances
- complex case: stabilizing highly parameterized problems
  - dynamic TCSC models, lots of parameters
  - that hierarchical conjoint thing

  - priors in high dimensions are scary: consider parameterizations and do simulations

- For nonparametric estimators, structural priors can be helpful for concentrating probability mass in sensible areas of space, since nonparametric estimators may be lower-powered than estimators that get a power boost from parametric assumptions.
- Parametric models, in term, are obvious areas where Bayesian estimates can go, but they are stacking more assumptions on top of the parametric assumptions.
- Semi-parametric models are an interesting middle ground, where we want to be flexible about the exact nature of the underlying relationships, but we want to impose some stabilization to prevent the model from behaving like crazy.

— 4 —

How District-Party Ideology Affects Primary Candidate Positioning: A Bayesian De-Mediation Model

Do primary elections effectively transmit citizens' policy preferences into government? For this to be true, we should expect that the policy ideology with a partisan constituency to affect the ideological positioning of candidates who run for that party's nomination. This chapter explores the effect of district-party public ideology on the positioning of primary candidates running in that district.

It is important to distinguish the influence of the district-party public from the influence of the district overall. Does a candidate like Senator Susan Collins have a reputation as a moderate Republican because of a close balance between the number of Republican and Democratic voters in Maine? Or is the Republican constituency in Maine relatively moderate compared to Republican constituencies in states that elect more conservative Republicans? Although past research has been interested in the threat of primary challenges as a cause of ideological divergence between partisan legislators (for example Boatright 2013; Hill 2015; Hirano et al. 2010; McGhee et al. 2014), many of these studies lacked the capability to observe the preferences within local partisan groups as a concept distinct from aggregate partisanship or aggregate voting in the entire district. This chapter uses my new measures of district-party ideology to investigate this question in ways that previous research projects could not.

The effect of district-party ideology on candidate positioning is a challenging causal inference problem. We cannot directly compare the "explanatory power" of district-party ideology and district-level voting by measuring whether one is more strongly correlated with candidate ideal points than the other, nor can we simply control for aggregate voting to recover the "partial effect" of district-party ideology. This is because aggregate policy ideology and aggregate voting are causally related: if a district contains a voter base with more conservative policy preferences, these policy preferences should influence aggregate voting behavior in the district as well as the positioning of candidates who try to respond to those policy preferences. Simply controlling for district voting in a regression will likely introduce collider bias by conditioning on a post-treatment variable (Greenland, Pearl, and Robins 1999; Montgomery, Nyhan, and Torres 2018).

This chapter investigates the effect of district-party ideology on primary candidate positions using a sequential-*g* analysis, a multistage modeling approach that estimates the direct effect of district-party ideology while fixing district-level voting, which mediates the effect of local ideology. Substantively, I translate the primary candidate's strategic positioning dilemma into the language of causal graphs, highlighting how aggregate districting voting mediates a relationship between district-party ideology and candidate positioning. Methodologically, I take the sequential-*g* method as it appears in political science (Acharya, Blackwell, and Sen 2016) and embed it in a Bayesian framework. The Bayesian framework estimates all components of the structural model simultaneously, quantifying uncertainty in all model parameters in a single posterior distribution. This includes measurement uncertainty in ideal point estimates from the IRT model in Chapter 2, which is included as a prior distribution over ideal points. The key payoff for the Bayesian structure, therefore, is a unified framework for conducting inference about treatment effects by marginalizing over other sources of uncertainty, be they design parameters or imprecise data.

4.1 Candidate Positioning and Voters' Policy Preferences

How do constituent preferences affect candidate positioning? This project explores the implications of what Brady, Han, and Pope (2007) call the "strategic positioning dilemma" (SPD), striking a balance between moderate position-taking to appease the general election constituency and ideological positioning taking to appease the partisan primary election constituency. Existing research contains plenty of studies that support the general theoretical intuition of the SPD theory, although I review some conflicts and ambiguities in detail in Chapter 1. To briefly review, general election candidates are rewarded at the ballot box for taking more moderate campaign stances (Canes-Wrone, Brady, and Cogan 2002; Hall 2015) and aligning themselves with local public opinion on specific issues (Canes-Wrone, Minozzi, and Reveley 2011; Fenno 1978; though see Fowler and Hall 2016). Nevertheless, no candidate makes it to the general election without first winning a primary nomination, where many scholars theorize that candidates benefit by taking more ideological positions that represent conventional views within the party. This could be a within-party Downsian incentive: the median primary voter is a ideological partisan with off-median policy preferences, so candidates take more extreme positions to appeal to partisan constituency preferences (Aldrich 1983; Burden 2001). This is consistent with evidence from safe congressional districts, where candidates experience less general election threat and can more freely position their campaigns to target the primary electorate (Ansolabehere, Snyder, and Stewart 2001; Burden 2004). Pressures for primary candidates to take non-median stances may come through mechanisms unrelated to bottom-up voter pressures, instead reflecting candidates' need to organize committed staff and volunteers for their campaigns (Aldrich 2011; Layman et al. 2010; McClosky, Hoffmann, and O'Hara 1960), seek campaign funds from policy-seeking contributors (Barber 2016; Barber, Canes-Wrone, and Thrower 2016; La Raja and Schaffner 2015), or garner support from policy-demanding groups that control access to connections and resources to support candidates (Cohen et al. 2009; Koger, Masket, and Noel 2009; Masket 2009).

As I explained in more detail in Chapter 1, the explicit evidence about the SPD from primary elections themselves is surprisingly weak. This is mainly because most studies do not explicitly measure the ideological preferences of primary voters, instead using aggregate measures of voting that neither identify policy preferences nor differentiate between partisan constituencies within a district (Kernell 2009). The IRT measures of district-party ideology that I create in Chapter 2 are, to my knowledge, the first ideal point scores for district-party groups to be applied to the study of congressional primaries.[1]

Research on primary competition and candidate positioning is also held back by the availability of primary candidate data, which until recently was not available. Past studies of primary competition and polarization typically use ideology scores from legislative roll call votes, which include only incumbent members of congress or state legislators (Brady, Han, and Pope 2007; Hirano et al. 2010; McGhee et al. 2014), or they use surveys of general election candidates, which may include non-incumbent candidates for the general election but no candidates who ran in the primary and lost (Ansolabehere, Snyder, and Stewart 2001; Burden 2004). Recent ideal point methods that use political financial contributions data have the capacity to scale a much broader universe of political actors, including candidates, political parties, PACS and interest groups, and donors (Bonica 2013, 2014; Hall and Snyder 2015). Few studies have yet used these contribution-based scores to study candidate positioning in primary elections (Ahler, Citrin, and Lenz 2016; Porter and Treul 2020; Rogowski and Langella 2015; Thomsen 2014, 2020).

[1] For IRT estimates of partisan constituencies at the U.S. state level, see Caughey, Dunham, and Warshaw (2018).

- Thomsen (2014)
- Porter and Treul (2020)
- Thomsen (2020)
  - no (significant) evidence that ideology–victory relationship is different for men and women (but the interaction pt estimates are comparable in size to main effects)
  - Dem women are more liberal than men, slight advantage in primaries
- Rogowski and Langella (2015)
- Ahler, Citrin, and Lenz (2016)

primary positioning (Rogowski and Langella 2015)

Primary systems - redistricting, same district, divergent candidates (Brunell, Grofman, and Merrill 2016; McCarty, Poole, and Rosenthal 2009)

- not everything: miller and stokes
- other things:
  - partisan organization (Layman et al. 2010; McClosky, Hoffmann, and O'Hara 1960)
  - party network
- issue-by-issue, mixed evidence.
- classic models of electoral competition view candidate moderation as an electoral benefit. You can make non-moderate positions make sense if you incorporate campaign volunteers, primaries, etc.

- Researchers find evidence that candidate positioning is related to district-level aggregate voting (e.g. the presidential vote) and to voter preferences (Ansolabehere, Rodden, and Snyder 2008; Ansolabehere, Snyder, and Stewart 2001; Clinton 2006).

- Why do candidates take non-moderate stances in reality? Studies that look into the polarizing effects of primaries find little evidence that primaries matter for candidate positioning, but many of these studies are focused on Congressional incumbents.

- Studies that include non-incumbent candidates show that many of the empirical implications of the "strategic positioning dilemma" don't receive a lot of evidence. For instance, studies of primary "openness" consistently show basically zero or even reversed relationships to candidate ideology as you would expect from theory.

The second obstacle preventing a more complete study of primary representation is the failure to incorporate primary candidates' ideal points into the analysis. Although ideal point estimates derived from roll-call votes such as Nominate are a popular tool for measuring politicians' ideological locations (Poole 2005; Poole and Rosenthal 1997), only incumbents cast roll call votes, so these measures are unavailable for non-incumbent candidates.[2] Further, when non-incumbents enter the picture, researchers tend to focus on the positioning of general election candidates rather than primary candidates (Ansolabehere, Snyder, and Stewart 2001; Burden 2004; Canes-Wrone, Brady, and Cogan 2002). Some studies have argued that primary competition leads incumbent legislators to take non-median positions, but these studies do not observe primary candidate positions directly, instead observing the presence or threat of challengers (Brady, Han, and Pope 2007; Burden 2004). Recent advances in ideal point modeling using campaign contributions are a promising path forward

[2] Studies of candidate positioning that go beyond incumbents sometimes use survey data from challenger candidates (Ansolabehere, Snyder, and Stewart 2001; Burden 2004), but the surveys only interview general election candidates. Furthermore, the rarity of these surveys limits the generalizability of their findings over time.

(Bonica 2013, 2014; Hall and Snyder 2015), but they are not designed for the careful study of primary competition and thus contain many "post-treatment" measurement artifacts.

What to do with the Hopkins/Sniderman theory

- does party's issue emphasis mean parties should collapse, or that variation should track
- Grossman-Hopkins; parties care about issues so they try to position themselves on it
  - read their stuff to see how they quality this
  - do they think districts collapse?
- weighting issues: left groups see fine variation in left members, but republicans are all bunched at zero (Brunell? Linda Fowler?)
- or Sniderman conflicted theory: Democrats are conflicted about appeasing groups, so variation matters. Republicans are conflicted
- THINK about abortion as a toy case
  - if weighting… if not weighting…

4.1.1 Exploratory analysis

- introduce the data first.
  - DIME candidate scores
  - posterior means only
  - we will do a more complete modeling routine later
- what is the relationship
- let's control for the thing
- what does this imply?

With direct measures of district-party policy ideology, do we get a different topline picture of within-party representation in primaries? Figure **??** shows a descriptive picture of

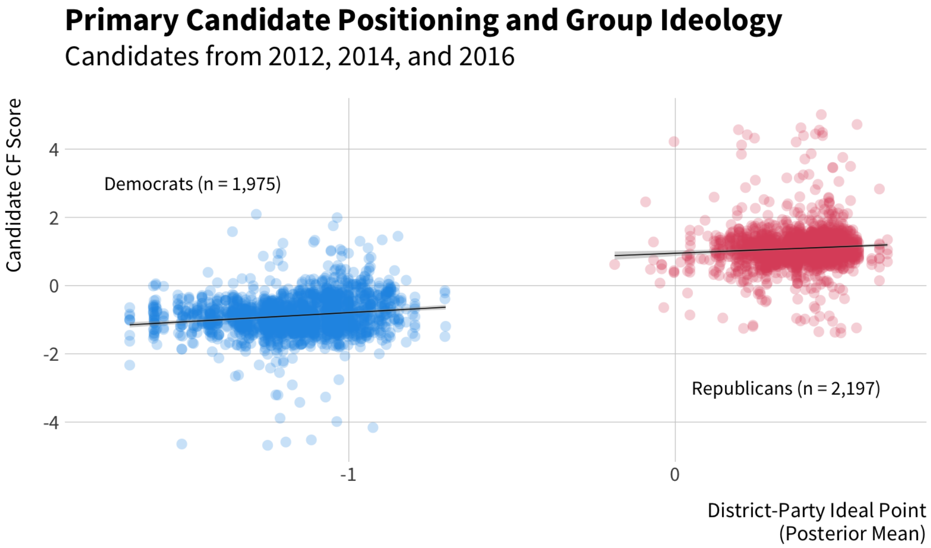


Figure 4.1: Topline relationship between district-party ideology and candidate positions. The horizontal axis plots the posterior mean for a group ideology, and the vertical axis is the dynamic CF score for primary candidates included in the DIME congressional candidate database.

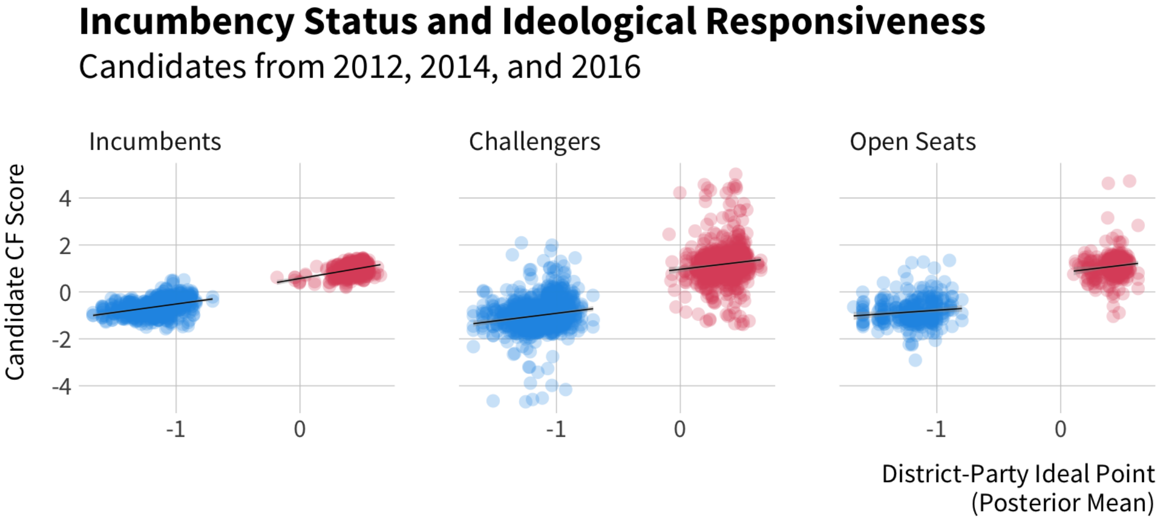


Figure 4.2: District-party ideology and candidate positioning across candidate incumbency status.

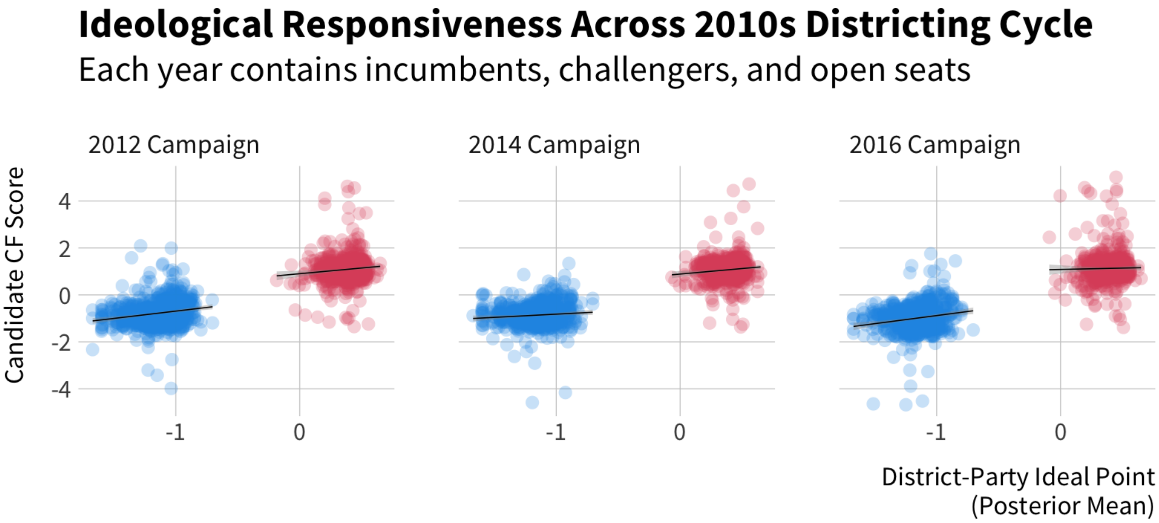


Figure 4.3: District-party ideology and candidate positioning across election cycles within the 2010s districting cycle.

the relationship between district-party ideology and candidate ideology for House primary candidates, the latter measured with dynamic ideal point scores derived from campaign contributions (Bonica 2014). Across incumbents, challengers, and open-seat candidates, in both parties, and across several election cycles, we observe a generally positive relationship between these two measures. As partisan citizens in a district become more conservative, so too do the candidates who run in those districts.

The strength of these relationships vary most dramatically by candidate incumbency status, which could be owed to competitive positioning incentives when, for example, challengers attempt to differentiate themselves ideologically from incumbents in ways that aren't entirely explained by bottom-up ideological pressure from voters (c.f. Ansolabehere, Snyder, and Stewart 2001; Burden 2004). Incumbent candidates' ideal points are most strongly related to district-party ideology, consistent with a notion that ideological consistency creates a selection effect into incumbency in previous election.

This descriptive picture informs a few modeling choices during the sequential-*g* routine. Because incumbency status appears to be a substantial modifier of the relationship between citizen and candidate ideology, I estimate the direct effect of district-party ideology separately for incumbents, challengers, and open seat candidates. By comparison, estimates are quite similar across election cycles, so I pool election cycles into one model with cycle fixed effects rather than estimating entirely separate models for different cycles. And although the descriptive relationships vary modestly across parties, the variables that could confound the relationship between citizen and candidate ideology could differ dramatically across parties. I therefore estimate models for Democrats and Republicans separately. This results in six groups of sequential-g estimates: three incumbency categories × two major parties.

Although this descriptive picture is suggestive about the relationship between district-party ideology and candidate ideology, we need more rigorous methods to interrogate a causal relationship. In particular, we should be concerned that district features that promote conservative voters also promote conservative candidates, so background features of districts are important to control. Furthermore, if it is worthwhile for researchers to consider the unique effect of district-party ideology, it is important to demonstrate that it affects candidate positioning above and beyond its intermediate effect on district voting. The sequential-g approach confronts both of these threats to inference.

4.1.2 The direct effect of district-party ideology

Make the argument for the direct effect setup

- appeal to SPD theory
- connect to spatial theory: we don't get different voting *but for* different voter distributions
- this may be unrealistic, but that is perhaps the point, since this is theory testing.

This section describes the causal estimand and estimation routine that follows. Sequential-g estimates a quantity called the *average controlled direct effect*, the average effect of a treatment on an outcome, holding fixed a mediator variable for all units under consideration.

Consider a potential outcome where candidate positioning C is affected by district voting V and district-party ideology T, or $C(T, V)$. The controlled direct effect imagines that we can intervene on both T and V, varying the value of T between t and t' while fixing $V = v$. The controlled direct effect is defined for a single unit i as,

$$CDE_i(t, t', v) = C_i(t, v) - C_i(t', v), \tag{4.1}$$

or, how would C_i change if we could vary t without influencing v in the process? The dependence of district voting V on district-party ideology T is shown by the causal graph in Figure 4.4. A model that estimates the *total* effect of district-party ideology will fail to differentiate the fraction of the effect flowing through path $T \rightarrow C$ from the fraction of the effect through path $T \rightarrow V \rightarrow C$. However, simply controlling for V will not isolate the direct effect, since it can open back-door paths from T to C through confounders represented by U (Montgomery, Nyhan, and Torres 2018). If there are variables that affect aggregate district voting that are independent of district-party ideology, such as valence features from unrelated prior candidates, post-treatment conditioning on the district vote can create confounders unintentionally. Sequential-g is a special case of a broader class of models (structural nested mean models) that measure direct effects by subtracting intermediary effects without creating collider bias (Acharya, Blackwell, and Sen 2016; Vansteelandt 2009).

In order to implement a sequential-g routine, we need to specify valid models that separately identify the mediator-outcome relationship (the total effect of district voting on candidate positioning) and the treatment-outcome relationship (the total effect of district-party ideology on candidate positioning). This twin identification is formalized using an assumption of *sequential ignorability*, or sequential unconfoundedness (Robins and Greenland 1994). This means that unit potential outcomes $C_i(t, v)$ are independent of treatment,

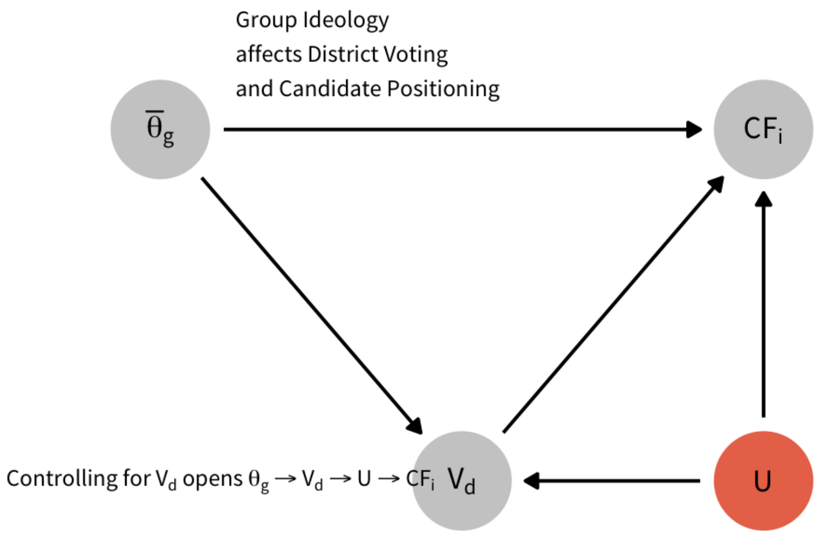

Group Ideology
affects District Voting
and Candidate Positioning
$\bar{\theta}_g$
CF_i
V_d
U
Controlling for V_d opens $\theta_g \rightarrow V_d \rightarrow U \rightarrow CF_i$

Figure 4.4: A DAG that presents district partisanship as a collider along the path from district-party preferences to candidate positioning. Controlling for district partisanship can bias the causal estimate of district-party preferences in several ways. If there is a path $\theta \rightarrow P \rightarrow Y$, then the effect of θ does not represent the total effect. In the presence of intermediate confounder U, controlling for district partisanship induces collider bias by unblocking the path $\theta \leftarrow U \rightarrow Y$.

conditional on pre-treatment covariates X_i,

$$C_i(t, v) \perp\!\!\!\perp T \mid X_i = x \tag{4.2}$$

and secondly that potential outcomes are independent of the mediator, conditional on treatment, pre-treatment covariates, and *intermediate covariates* Z_i that may affect the mediator separately from T and X.

$$C_i(t, v) \perp\!\!\!\perp M \mid T_i = t, X_i = x, Z_i = z \tag{4.3}$$

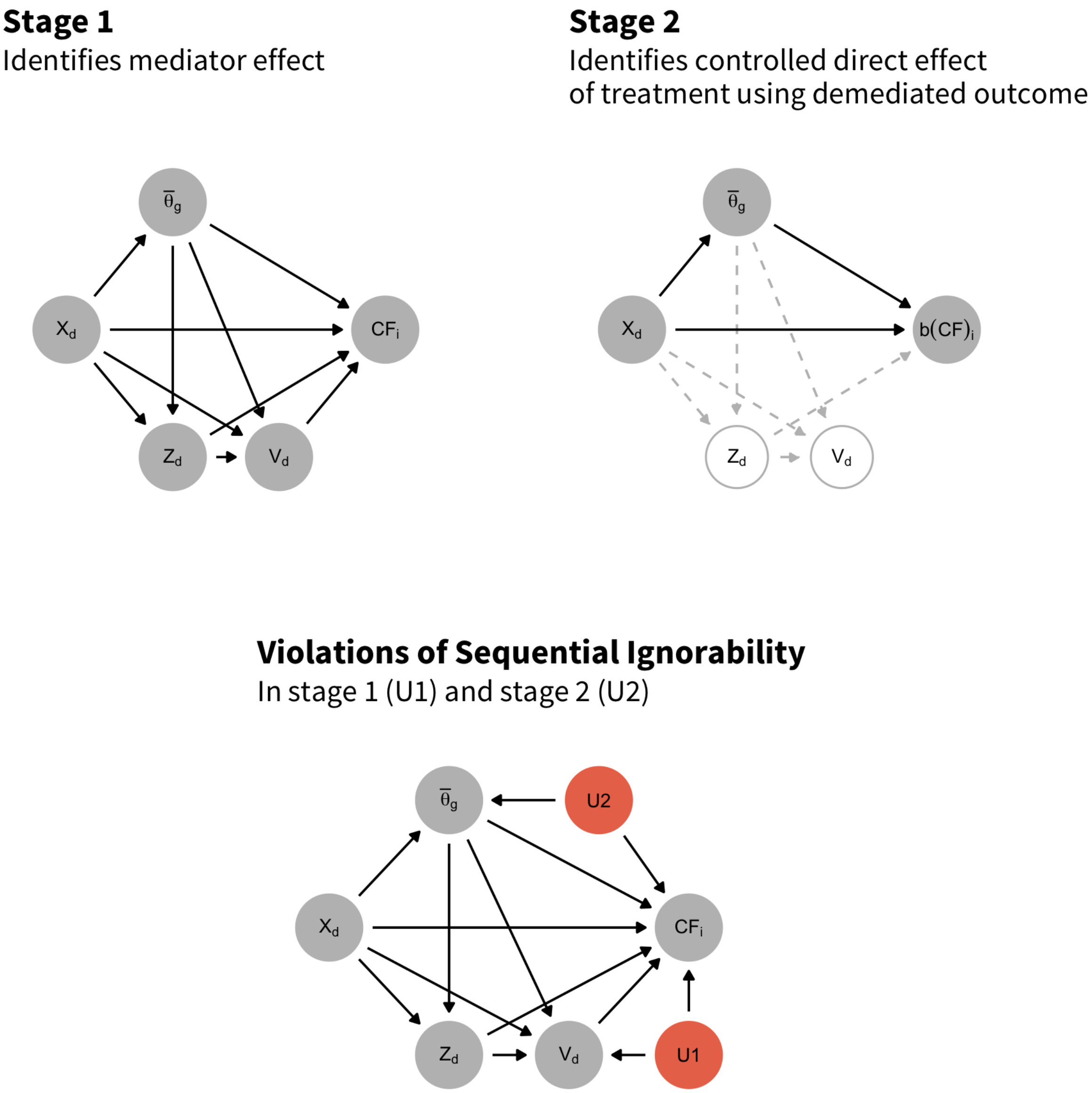
Figure 4.5 visualizes the modeling assumptions for sequential-g estimation using causal graphs, which helps explain how to implement the routine. The left panel shows the stage-one model, which estimates the effect of past voting V (the mediator) on candidate positioning C (the outcome). This first stage conditions on district-party preferences T, pre-treatment confounders X, and intermediate confounders Z, all of which are necessary to identify the causal effect of the mediator.

After estimating the mediator's effect on the outcome, the outcome variable is *demediated* by subtracting the mediator effect from the outcome variable. The center panel represents this demediation step by rewriting the outcome variable as $b(C)$, the demediated value of C.[3] The stage-two model then estimates the effect of district-party preferences T on the demediated candidate positions, controlling for pre-treatment covariates X. Demediating the outcome suppresses the path from V to $b(C)$ because (by sequential unconfoundedness) there is no longer any systematic variation between the mediator and the outcome. As such, there is no need to adjust for V, since it has no systematic effect on $b(C)$ after demediation. Furthermore, although there remains a causal effect from the intermediate confounders Z to candidate positions, the stage-two model does not adjust for these confounders to avoid post-treatment bias in the estimate of the CDE. This stage-two model recovers the controlled direct effect of T on C.

It is worth noting here that if we estimate the stage-two model using the natural value of C rather than the demediated $b(C)$, we would obtain the total effect of T (the conditional average treatment effect) rather than the controlled direct effect. This can be valuable, since the difference between the total effect and the controlled direct effect are an indirect indicator of how much of the total effect flows through a mediator variable.

The final panel shows where unmeasured confounding can violate the sequential unconfoundedness assumption. The stage-one model identifies the causal effect of the mediator, so if an unmeasured variable (represented in the future by $U1$) affects both the mediator and the outcome, the mediator's effect is not identified. Similarly, the stage-two model does not identify the effect of T on $b(C)$ if they share an unmeasured confounder $U2$. Unmeasured variables in other locations of the graph certainly exist, but they do not violate sequential

[3] The exact demediation operation is shown below in Equation (4.5).



Stage 1
Identifies mediator effect

Stage 2
Identifies controlled direct effect
of treatment using demediated outcome

Violations of Sequential Ignorability
In stage 1 (U1) and stage 2 (U2)

Figure 4.5: Causal graphs describing the modeling problem and sequential-g estimation. The stage 1 graph identifies the effect of the past district voting (V_d) on candidate positioning (CF_i). The stage 2 treatment-outcome model subtracts the district vote effect from candidate positions and identifies the effect of district-party ideology $\bar{\theta}_g$ on the demediated CF score $b(CF)_i$, which is equivalent to the controlled direct effect on the raw CF score. The final graph shows where unadjusted confounders violate the nonparametric causal identification assumptions in stage 1 ($U1$) and in stage 2 ($U2$).

unconfoundedness unless they can be represented by an open back-door path through $U1$ or $U2$.[4] There are functional form considerations however—biases may remain if the functional forms for confounders X and Z are inappropriate.[5]

4.2 Bayesian Sequential-g Analysis

what is ACDE

Cite ABS

Keep the notation general enough (expectations) so that there are no explicit linear regression functional forms introduced by accident?

Identification and estimand are above?

Contribution: $\bar{\theta}_g$ is drawn from a prior and can be updated in the model.

ACDE is posterior distribution of the causal quantity…

I write a general model that applies regardless of which subset of data is being used. Because the estimation in every data subset proceeds in two stages, I use subscript some parameters 1 and 2 to indicate that they are not fixed across model stages.

The first stage is a mediator-outcome model, predicting the CF score of candidate i within group g, where a group is a combination of district d and party. Because I estimate separate models for each party, each model sees data from only one group per district. Nonetheless, I use separate g and d subscripts to denote which variables have the potential to vary by groups within districts. We set up the sequential-g method to control for the previous Republican presidential vote share in district d, denoted $pvote_g$. This is done with the following multilevel

[4] For instance, a variable W may be a common cause of X and C, thus creating the path $T \to X \to W \to C$, but it does not confound the effect of T because conditioning on X blocks the path. Additionally, although intermediate confounders Z are presented as descendants of group policy ideology T, intermediate confounders do not necessarily have to be affected by T (though they can be). They only need to be confounders of the mediator-outcome relationship.

[5] I plan to investigate more flexible functional forms in the near future.

regression model.

$$\begin{aligned} \mathrm{CF}_i &= a_0 + \eta\bar{\theta}_{g[i]} + \mu \mathrm{pvote}_{d[i]} + \mathbf{x}_{d[i]}^{\top}\beta + \mathbf{z}_{d[i]}^{\top}\gamma + \alpha_{d[i]} + \varepsilon_i \\ \alpha_d &\sim \mathrm{Normal}\left(0, \sigma_\alpha\right) \\ \varepsilon_i &\sim \mathrm{Normal}\left(0, \sigma_\varepsilon\right) \end{aligned} \tag{4.4}$$

This model identifies the effect of the presidential vote, μ, under the identification assumption of sequential ignorability and the statistical assumptions of linearity and no interactions. The group ideal point $\bar{\theta}_g$ is included in the regression as a control, so the coefficient η is estimated as a nuisance parameter, as are the coefficients β for district-level pre-treatment confounders $\mathbf{x}_d$, the coefficients γ for district-level intermediate confounders $\mathbf{z}_d$, and constant a_0. Because the mediator is measured at the district level, I include a district error term α_d in addition to the candidate-level error term ε_i. The multilevel model accounts correlated error among candidates in the same district, similar to clustering standard errors when a treatment is dosed at the cluster level.

Sequential-g estimates the controlled direct effect of treatment by purging the DV of the mediator effect. This is done by predicting the outcome variable as if the mediator were fixed for all units, which purges the outcome of all systematic variation caused by the mediator variable. This can be done by predicting the outcome under a fixed mediator value directly or by demediating the outcome directly using an estimate of the demediation function. Because the first stage is a linear model, the demediation function has a straightforward parametric definition,

$$\delta_d = \mu\left(\mathrm{pvote}_d - \mathrm{pvote}'\right) \tag{4.5}$$

where *pvote′* is the reference value for the mediator where all units are fixed, and μ is the mediator's effect from Equation (4.4). The demediation function represents the effect setting district d's presidential vote to its observed value compared to the reference value *pvote′*.

Because the previous presidential vote varies across districts, so too does the demediation function.[6] We choose a reference value of 0.5, an even vote split between the Republican and Democratic presidential vote shares in the previous election. The demediation function can be more complex if the mediator's effect on the outcome is modeled with a more complex model containing interactions or nonlinearities. We calculate the demediated outcome $b(CF)_i$ by subtracting each district's demediation function from its observed outcome value,

$$b\left(\mathrm{CF}\right)_i = \mathrm{CF}_i - \delta_d \tag{4.6}$$

Because the mediator is fixed for a given district, all candidates in a district have the same demediation function, regardless of their original CF score. The demediated outcome value is equivalent to the outcome under a fixed mediator value,

$$\begin{aligned}
b\left(\mathrm{CF}\right)_i &= \mathrm{CF}_i - \delta_d \\
b\left(\mathrm{CF}\right)_i &= a_0 + \eta\bar{\theta}_{g[i]} + \mu\mathrm{pvote}_{d[i]} + \mathbf{x}_{d[i]}^{\top}\beta + \mathbf{z}_{d[i]}^{\top}\gamma + \alpha_{d[i]} + \varepsilon_i - \mu\left(\mathrm{pvote}_{d[i]} - \mathrm{pvote}'\right) \\
&= a_0 + \eta\bar{\theta}_{g[i]} + \mu\left(\mathrm{pvote}_{d[i]} - \mathrm{pvote}_{d[i]} + \mathrm{pvote}'\right) + \mathbf{x}_{d[i]}^{\top}\beta + \mathbf{z}_{d[i]}^{\top}\gamma + \alpha_{d[i]} + \varepsilon_i \\
&= a_0 + \eta\bar{\theta}_{g[i]} + \mu\mathrm{pvote}' + \mathbf{x}_{d[i]}^{\top}\beta + \mathbf{z}_{d[i]}^{\top}\gamma + \alpha_{d[i]} + \varepsilon_i \\
&= a' + \eta\bar{\theta}_{g[i]} + \mathbf{x}_{d[i]}^{\top}\beta + \mathbf{z}_{d[i]}^{\top}\gamma + \alpha_{d[i]} + \varepsilon_i
\end{aligned} \tag{4.7}$$

which makes it clearer to see how the demediation step subtracts the mediator variation from the outcome, absorbing the non-varying mediator into the new constant a'.

The demediated outcome is then used in the second stage estimation of the controlled direct effect. The second stage is different from the first stage in two ways. First, because we fixed the mediator value in calculating the transformed outcome variable, there is no more variation across observations that can be attributed to the mediator. As such, it is

[6] In any case where the reference value for the mediator is zero, this expression reduces to $\mu pvote_d$, and an observed value of zero implied a demediation function of zero.

omitted from the equation. Second, intermediate confounders are omitted because they are unnecessary to identify the effect of district-party ideology, and they could induce collider bias if included. This new equation is,

$$b\left(\mathrm{CF}\right)_i = a_1 + \tau\bar{\theta}_g + \mathbf{x}_{g[i]}^{\top}\omega + v_{d[i]} + u_i \tag{4.8}$$

$$\tag{4.9}$$

where a_1 is a constant, τ is the coefficient for district-party ideology, ω are coefficients for district-level pre-treatment confounders $\mathbf{x}_d$, v_d is a district error term, and u_i is a candidate error term. Because the presidential vote effect has been removed from the dependent variable, any treatment effect mediated by the presidential vote is not recoverable by the second stage equation. As such, τ measures the average controlled direct effect of a one-unit increase in district-party ideology. More generally, the ACDE of setting district-party ideology from $\bar{\theta}'$ to $\bar{\theta}$ is as follows.

$$ACDE(\tau, \bar{\theta}, \bar{\theta}') = \tau\left(\bar{\theta} - \bar{\theta}'\right) \tag{4.10}$$

Again, the formula for the ACDE depends on the model specification. The form in (4.10) applies to a linear model with no interactions, but a more complex model would entail a more complex formula.

4.2.1 Bayesian causal inference

The key methodological innovation in this chapter is embedding the sequential-g method in a Bayesian framework. Chapter 3 describes a number of special advantages for Bayesian causal modeling, stemming from the fact that Bayesian inference allows the researcher to describe their information about causal effects using the intuitive of language of probabilistic inference: the treatment effect is *probably* greater than x, where "probably" has a direct translation to approximate cumulative probabilities from posterior samples.

The most important feature of Bayesian causal inference for this chapter is the fact that one posterior distribution quantifies uncertainty in all parameters from the multi-stage sequential-g method. This is valuable because uncertainty in stages 1 and 2 are directly related to one another. The average controlled direct effect of district-party ideology in stage 2 is estimated using demediated CF score, which is calculated using the demediation function, which is itself a function of stage 1 model coefficients. Substantively, the ACDE should have a larger magnitude whenever the indirect effect through the mediator has a smaller magnitude, holding the total effect magnitude constant. Whereas Acharya, Blackwell, and Sen (2016) derive an analytical variance estimator for the second stage coefficients or advocate for bootstrapped estimation, the posterior distribution from the Bayesian model captures all variances and covariances among model parameters by its nature. Inferences about the ACDE can be expressed by marginalizing the posterior distribution with respect to these auxiliary model parameters, isolating the remaining dimension of the posterior corresponding to the ACDE. Letting $\boldsymbol{\pi}$ represent all auxiliary model parameters, and using the definition of the ACDE in Equation (4.10), the posterior distribution for the ACDE is given by

$$p\left(\tau\left(\bar{\theta} - \bar{\theta}'\right) \mid \mathbf{y}\right) = \int p\left(\tau\left(\bar{\theta} - \bar{\theta}'\right), \boldsymbol{\pi} \mid \mathbf{y}\right) \mathrm{d}\boldsymbol{\pi}, \tag{4.11}$$

which is a the distribution of ACDE values that condition on the observed data and marginalize over the associated auxiliary parameter values. In practice, we use MCMC samples to approximate this posterior distribution by picking two fixed values of $\bar{\theta}$ and $\bar{\theta}'$, extracting posterior samples for τ, and calculating $\tau\left(\bar{\theta} - \bar{\theta}'\right)$ for each MCMC sample iteration. In cases where $\bar{\theta} - \bar{\theta}' = 1$, the ACDE is equal to τ, so posterior samples for τ are sufficient to characterize that posterior. Posterior expectations for the ACDE can be calculated, up to Monte Carlo error, by averaging the ACDE values from a draw of MCMC samples.

The joint posterior distribution is also essential for incorporating uncertainty in district-party ideal points themselves. The IRT model in Chapter 2 does not estimate district-party ideal points exactly. Instead, ideal points are estimated only up to a posterior distribution, with uncertainty that reflects both prior ignorance and a finite sample of polling data. To estimate the effect of district-party ideology on candidate positioning, the causal analysis incorporates measurement error in ideal points "the Bayesian way," where posterior uncertainty from one analysis becomes prior uncertainty in later analyses. One way to do this would be to build one joint model for the measurment of Rather than jointly estimate the ideal point model and all subsequent causal analyses, which would be an overwhelming feat of model-building and estimation, This prior is constructed by defining the group ideal point $\bar{\theta}_g$ as an element of Θ, the vector of all group ideal points. I then specify a multivariate Normal prior on the vector of ideal points.

$$\Theta \sim \text{MultiNormal}\left(\hat{\Theta}, \hat{\Sigma}_{\Theta}\right) \tag{4.12}$$

The parameters in the prior are estimated from MCMC samples for the ideal points. The mean vector $\hat{\Theta}$ contains MCMC mean for each ideal point, and the matrix $\hat{\Sigma}_{\Theta}$ contains variances for each ideal point and covariances between any two ideal points. Because the IRT model partially pools ideal points using a hierarchical Normal regression, the multivariate Normal prior is a reasonable stand-in for representing prior ideal point uncertainty in causal analyses.

Including ideal point uncertainty as a prior distribution effectively adds a measurement error model overtop to the sequential-*g* analysis. Although Acharya, Blackwell, and Sen (2016) describe a variance estimator and a bootstrap method for dealing with multi-stage modeling uncertainty, neither of these methods is naturally suited to a measurement error context where an additional layer of ideal point uncertainty is represented in MCMC samples. Past research has explored the use of inverse-variance weighting to downweight observations with greater ideal point uncertainty, but that method accomplishes essentially the same goal as a prior distribution, except it throws away all information about the covariance covariance between any two ideal points. More recently, researchers have employed methods of "uncertainty propagation" methods, where the researcher estimates an uncertainty quantity, simulates values from the quantity's posterior distribution, and pushes those simulated values through a downstream analysis. Quantities of interest are then averaged across the posterior simulations (see Kastellec et al. 2015b, 791; Caughey and Warshaw 2018, 6, 2019, 360). This is similar in spirit to "multiple overimputation" (Blackwell, Honaker, and King 2017), which creates "imputation datasets" by iteratively replacing mismeasured observations with draws from their posterior distribution. This project regards these propagation methods as insufficiently Bayesian because they "cut" the flow of information between models: model 1 informs model 2, but model 2 can never inform model 1 (Plummer 2015). This can be an undesirable modeling property if the causal model can inform the measurement model (Treier and Jackman 2008). In this project, if ideal points are related to candidate positioning, and ideal points are measured with error, then candidate positions can be informative about ideal points. Even if the causal analysis does not narrow the marginal distributions for ideal points, the posterior distribution will reflect the correlation between ideal points and other parameters. By specifying the prior directly and updating the parameters, there is no need for any additional imputation steps or post-estimation model averaging (Gelman and Hill 2006, 542).

The Bayesian sequential-g approach, of course, requires priors for other model parameters. Coefficients for linear predictors in stages 1 and 2 are given Normal priors that are proper but overly diffuse, to avoid biasing treatment effects by over-regularizing covariates (Hahn et al. 2018).

$$a_0, \eta, \mu, \beta, \gamma \sim \text{Normal}\left(0, 10\right) \tag{4.13}$$

$$a_1, \tau, \omega \sim \text{Normal}\left(0, 10\right) \tag{4.14}$$

Both modeling stages contain Normal district-level error terms, with estimated variances that facilitate partial pooling. These variances are themselves given half-Normal priors with weakly informative scale parameter values that are about half the range of the raw outcome data within each party.

$$\alpha_d \sim \text{Normal}\left(0, \sigma_\alpha\right) \tag{4.15}$$

$$\sigma_\alpha \sim \text{Half-Normal}\left(0, 2\right) \tag{4.16}$$

$$\nu_d \sim \text{Normal}\left(0, \sigma_\nu\right) \tag{4.17}$$

$$\sigma_\nu \sim \text{Normal}\left(0, 2\right) \tag{4.18}$$

And finally, each stage of the model has a Normal error term for candidates within districts. The variances for these errors are given half-Normal priors with wider scale values that the districts errors, since residual variation between any two candidates is likely larger than the variation between average candidates in any two districts.

$$\varepsilon_i \sim \text{Normal}\left(0, \sigma_\varepsilon\right) \tag{4.19}$$

$$\sigma_\varepsilon \sim \text{Half-Normal}\left(0, 5\right) \tag{4.20}$$

$$u_i \sim \text{Normal}\left(0, \sigma_u\right) \tag{4.21}$$

$$\sigma_u \sim \text{Normal}\left(0, 5\right) \tag{4.22}$$

4.2.2 Multilevel data and causal inference

The research question in this chapter presents us with multilevel data: how is the ideological positioning of primary candidates affected by the policy ideology of partisans in their district, when there are potentially multiple candidates per district? In this scenario, the outcome is a variable specific to an individual candidate i, but the treatment is fixed for an entire district-partisan group g. This introduces a few issues for statistical assumptions and causal assumptions.

On the statistical front, multilevel models bias coefficient estimates when the aggregate errors are not exchangeable. Mechanically, this is similar to "omitted variable bias" in a single-level regression. Although this concern is well founded for many multilevel models, for these models we can be less concerned. Because all predictors in these regressions are measured at the district level, the district error term is analogous to an error term that we would obtain by averaging every candidate's CF score within a district-party group and running a single-level regression on those averages. Both of these model specifications require an exchangeable errors assumption at the district level. The only difference for the models in this analysis is the additional candidate-level errors, but this too is a non-issue. Averaging candidate data within each district would invoke a similar assumption about the exchangeability of candidates given the district, otherwise it would be inappropriate to average data within a district.

Even though the multilevel model has similar assumptions as a regression on averages, it has certain benefits that are convenient for these data. Because the number of candidates in a district isn't fixed across all districts, we would expect heteroskedasticity in a regression-on-averages model, since some districts would have higher variances due to fewer candidates. In the extreme case, if a district contained only one unopposed primary candidate, a naïve estimator would be unable to distinguish district-level variance from candidate-level variance. The multilevel model addresses this by estimating the distributions of district errors and candidate errors simultaneously, enhancing the model's ability to recognize when larger district errors are caused by signal versus noise. Errors from smaller districts borrow more information from the overall distribution of districts, downweighting the contributions of smaller districts by shrinking their errors toward a mean of zero. This has a similar intuition as a weighted least squares regression on the district-averaged data, where districts with more candidates are more informative and receive greater weight. Because the multilevel model is

Bayesian, however, the model can handle single-candidate districts by drawing district and candidate error terms from their respective prior distributions. This is yet another example where priors stabilize pathological model behavior, and underscores how the flexibility afforded by Bayesian model-building allows us to confront the idiosyncrasies of a dataset with tactics that are both intuitive and feasible.

The multilevel data structure also raises causal inference issues that are worth clarifying. As with many causal models where treatments are assigned to clusters of observations, it makes sense to consider SUTVA as violated within a cluster: there is no way for one candidate in a district to be treated by a different district-party ideology than other candidates.[7] The positioning of one candidate may also affect the positioning of another, which could violate the "no interference" component of SUTVA. Under this violation, the treatment effect at the individual level is not identified. If SUTVA holds *between* groups, however, it is possible to identify a treatment effect by considering average effects across groups (Hill n.d.). In potential outcomes notation, even if we can define potential outcomes at the individual level ($CF_i(\bar{\theta}_{g[i]})$), the lowest level where we could credibly *identify* treatment effects would be the group level, where the potential outcome for a group is the average outcome within the group ($\overline{CF}_g\left(\bar{\theta}_g\right)$). This is consistent with the multilevel model setup that we have so far, where the ACDE is a function of aggregate data and aggregate parameters only (see Equation (4.10)).

There are a few additional considerations for causal inference with hierarchical data that, although I do not pursue these threads in this project, could be relevant for future work with similar data. A correlation between treatment effects and group size may arise if a crowded primary field causes larger treatment effects because stiffer competition leads candidates to be more responsive to district-party ideology. On the other hand, more crowded fields would lead to smaller treatment effects if candidates take heterogeneous ideological positions to

[7] Candidates may vary in their ability to perceive district-party ideology, but that might also be described as an issue of treatment compliance or treatment effect heterogeneity.

differentiate themselves. If treatment effects are correlated with group size, then the average causal effect for a candidate is not equivalent to average difference among groups. Instead, the average effect for candidates must be a size-weighted average of group effects (Hill n.d.). I do not pursue this possibility in this project because these dynamics are not identifiable with data on primary candidates only, since incumbents may take ideological positions to deter challengers even if no challengers actually emerge. As such, the observed number of candidates in a district may not capture the true degree of primary threat (Hirano et al. 2010; Maisel and Stone 1997; Stone and Maisel 2003).

One additional consideration for group-level effects is the possibility that group size affects treatment assignment. This may be true if the long-run dynamics of primary competition within a district-party have feedback effects on local ideology, for instance if partisan constituents become more ideologically aware by experiencing stronger intra-party competition in their district, or less ideological after a long period of representation by a single incumbent with little primary competition. There is evidence that primaries contain more ideological campaign content in certain periods of heightened partisan mobilization (Boatright 2013), which could increase voters' ideological awareness as well. Whether voters are responding to primary competition in the district *per se* or to a national state of partisan agitation is an interesting but thoroughly challenging question for future researchers to explore, were they to extend the data and methods in this project to a greater number of election cycles and dynamic causal modeling approaches (e.g. Blackwell and Glynn 2018; Imai and Kim 2019).

4.2.3 Data

Pre-treatment confounders consist of district-level demographic indicators for the racial composition, college graduation rate, median income, inequality (Gini), unemployment rate, foreign-born population, and evangelical population, and $\mathbf{z}_{ig}$ currently contains an order-3 polynomial function of candidate *i*'s total campaign receipts.[8]

CF scoreswork by assuming that a donor i contributes to recipient j by maximizing their donation utility over all potential recipients. The donor chooses a dollar amount y_{ij} subject to contribution limits that maximizes

$$u_i(\mathbf{y}_i) = \sum_j \left[b_i \left(y_{ij} \right) - y_{ij} \left(\theta_i - \delta_j \right)^2 \right] \tag{4.23}$$

where $b_i(y_{ij})$ is i's net benefit for contribution y_{ij} (instrumental and/or expressive benefits minus costs), θ_i is the donor's ideal point, and δ_j is the recipient's ideal point. The term $y_{ij} \left(\theta_i - \delta_j \right)^2$ is a dollar-weighted function of the ideological distance between donor and candidate ideologies. The term grows when the ideological distance is greater, meaning that the donor loses more utility by giving to candidates that don't reflect the donor's ideological preferences, and it also grows when the donation amount y_{ij} is larger. The b_i function acts like a donor-level fixed effect, capturing a donor's overall propensity to donate regardless of their ideological proximity to potential recipients. Bonica (2013) derives a fully parameterized item response theory (IRT) model from this utility framework, estimating latent ideological locations for all donors and all recipients from their contribution patterns. The IRT scores do not cover as wide of a universe as CF the scores created from correspondence analysis (Bonica 2014).

General comment: very difficult to code these.

- law vs. party rules
- for some organizations "open" means there's ANY openness, which can include the McGhee "semi-closed" definition
- most states have the same system for both parties, but some state laws leave it open to parties to modify their rules in the months leading up to the election.

[8] Note: this isn't a good choice for intermediate confounding, and it should change. Ask me why!

I take data from Boatright on primaries.

- 2016: combination of NCSL, Ballotpedia, and OpenPrimaries.org, as of 2020-05-27.

— 5 —

How District-Party Ideology Affects Primary Election Outcomes:

Combining Subposteriors for Honest Bayesian Causal Inference

Group IRT Model

Colophon

This project is open source and managed with Git. A remote copy of the repository is available at https://github.com/mikedecr/dissertation. Currently, the repository is at the following commit:

```
## Commit:  c0d196e935dc6402c04c8ca87572572a757b3a88
## Author:  Michael DeCrescenzo <mgdecrescenzo@gmail.com>
## When:    2020-08-21 20:46:43 GMT
##
##      lit and theory abt primary positioning
##
## 1 file changed, 57 insertions, 4 deletions
## 40_positioning.Rmd | -4 +57  in 4 hunks
```

This version of the document was generated on 2020-08-21 16:32:15.

All Bayesian models were estimated using the probabilistic programming language Stan. Front-end interface to Stan and other data management was performed with R. The document was managed with the bookdown package for R, built to PDF using LaTeX.

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