

# Congress and History: Enhancing the Methodological Repertoire

Gregory J. Wawro

Associate Professor

Department of Political Science

Columbia University

gjw10@columbia.edu

Ira Katznelson

Ruggles Professor of Political Science and History

Departments of Political Science and History

Columbia University

iik1@columbia.edu

**Extremely Preliminary**

Prepared for presentation Congress and History Conference, Princeton University, May 18–19, 2007. The authors gratefully acknowledge the following individuals who helped in the preparation of this paper: Andrew Gelman, Quinn Mulroy, Grant Porter, and Josh Savitch.

# 1 Introduction

How should the craft, logics, and landscape of history be made a constitutive aspect of political analysis? Recently, a number of leading historians, using just these terms, have sought to critique the absence of a historical imagination in the social sciences or the manner in which history is utilized (Gaddis 2002; Sewell 2005; Tractenberg 2006; Tilly and Goodin 2006). Concurrently, some historical institutionalists, including members of the subfield of American Political Development (APD) have been ruminating about how to place “politics in time,” mounting their own criticism of the mainstream discipline for its neglect or mechanical deployment of the past, and thus for an apparent lack of interest in the most significant large questions about the character of the polity—issues that only can be addressed with a historical sensibility (Pierson 2004; Orren and Skowronek 2004; Pierson and Skocpol 2002).

In the main, these appraisals have failed to notice both the turn to historical materials and cases in parts of political science that previously had been doggedly oriented to the present or very recent past. There has been a growing number of accounts that trace how processes, including the temporal horizons of actors, unfold over time. We can see this development in American politics scholarship that studies public opinion (Page and Shapiro 1992) or the control of regulatory agencies (Moe 1987) over the long term. Such work has become more common among quantitative and formal students of International Relations (Mansfield and Snyder 2005) and Comparative Politics (Kalyvas 1996, 2006). If nothing else, attention to history, the discipline has learned, offers a rich archive of information that can be studied even by the most systematic deductive and quantitative as well as a range of qualitative methods, sometimes serving as means to apply and evaluate models and hypotheses first developed in analyses of more current situations (Bates, Greif, Levi, Rosenthal, and Weingast 1998; King, Keohane, and Verba 1994).

But therein often lies a problem. History frequently is treated simply as data with the implicit assumption of an equivalence among facts and particulars found in various periods and contexts. Historians and historical social scientists often blanch at what they believe to be far too simple, and simplifying, ways of working by mainstream colleagues who set out to work on the past. They tend to have two sets of concerns. Substantively, they think this emergent work misses the chance to enlarge scholarship’s agenda, to ask the most important,

difficult, and challenging historical questions. Methodologically, they find this scholarship to be limited, even narrow, and innocent about the challenges that come with taking history seriously. Sometimes, as in John Lewis Gaddis' *The Landscapes of History* (2002), these critics get rather testy, to the point of being dismissive.

Not without self-interest, we are delighted, by contrast, to see the turn to history in political science. We value movement in this direction not only among political scientists whose first commitments include a devotion to studies of the past, but by others whose work is located at the center of intellectual communities devoted to advanced quantitative scholarship and formal theory. Still, we agree with many of the judgments of the critics who would like the social sciences to be more attentive to inherent methodological challenges regarding temporality, periodicity, specificity, and context in historical studies, and thus better able to probe questions of political development.

This set of issues is arguably most important in the richest area of scholarship on American politics in terms of depth of debate, sophistication of method, theoretical rigor, and empirical innovation in the past quarter-century—the literature on Congress. Here, the promise of the behavioral revolution and of deductive modeling has been achieved most fully. We know vastly more about this branch of American government than any other, thanks to the depth and acuity of these studies.

More and more scholarship on Congress has been taking a historical tack. Though not wholly new as a research program (Brady 1973; Cooper and Brady 1981), studies of Congress and history have grown to constitute a coherent trend. Just this welcome development makes the reservations and imprecations of historians and historically-oriented political scientists (simply referred to in what follows as historians) particularly significant, especially because congressional work on the past paradoxically risks being largely ahistorical unless it reexamines its ways of working. For the powerful ways formalization and quantitative investigations have proceeded may themselves have raised some barriers to good historical scholarship. In the absence of such self-consciousness, the Congress field, even in its historical turn, can seem beside the point to many historians and APD scholars, a result that reinforces their own long-standing neglect of Congress.

Recently, Katznelson and Lapinski (2006) have urged students of APD to more deeply

integrate scholarship on Congress into their research. After all, the marginal place accorded to Congress in APD and by historians belies the centrality of political representation and lawmaking to the country's broadly liberal polity and limits understanding of the distinctive qualities of the American state. It also misses chances to study such key mechanisms as temporal sequencing and policy feedback that have been central to work in APD.

In turn, unless the disquiet of historians concerning methodology is closely attended, historical studies within the Congress subfield risk a limiting amateurism that makes it difficult for this literature to contribute to ongoing vibrant debates about the character of the United States as a particular kind of political regime. Our motivation to make historical work by scholars of Congress more self-consciously and instrumentally historical, and thus make it better in its own terms, follows from this concern.

Just as some who engage in historical research have become more self-conscious about the way they conduct their work, especially in terms of what social science might offer (Gaddis 2002; Sewell 2005; Tractenberg 2006; Pierson 2004; Orren and Skowronek 2004), Congress scholars would do well to engage in similar introspection. As a contribution to such an effort, we suggest that moving forward requires innovations in methodology to supersede what less formal and more qualitative historians and APD researchers can accomplish, while taking seriously the central themes and epistemological concerns of their work. History, they rightly insist, is more than a mere laboratory or depository of additional cases for model building and testing.

There is a big opportunity waiting to be seized. No American institution is better documented than Congress in detail over time. In part as a function of the unique position of the legislature in the separation of powers system and its long-term stability, existing data is incredibly varied and rich. Given the treasure trove of information available on congressional elections, roll call voting behavior, committee and floor debates, lawmaking, and institutional development, opportunities for analysis that link directly to the central issues that concern historians and the APD community seem almost limitless. Scholars of Congress, moreover, have never been hesitant when it comes to innovation.

The paper proceeds as follows. We give a general overview of the critique by historians of the political science approaches history, proceed to a discussion that contrasts more stan-

dard regression approaches with some relatively recent innovations, and briefly illustrate the latter’s potential by offering two pilot demonstrations of the plausibility of our approach. We conclude with a statement about a potential research program.

## 2 Skepticism and Possibility

Many historians pay little or no attention to political science, thinking its practitioners to be primitive or uncurious about the past. On this view, political scientists spend too little time with primary sources in archives or with secondary sources written by historians. But these are relatively superficial matters; after all, there is no reason political scientists cannot exercise more due diligence with historical evidence or broaden their libraries. But some historians who take the social sciences seriously, and who wish, like Richard Hofstadter writing a half-century ago, to develop history “informed by the insights of the social sciences,” and whose “primary purpose will be analytical” (Hofstadter 1956, 363), have posed more fundamental challenges. Such historians know that the two crafts, irrespective of an overlap, have distinctive purposes and qualities, and they understand that political scientists, within the division of scholarly labor, are less focused on period-specific narratives of “what happened” than attuned to theoretical and empirical models fashioned to discern causes and mechanisms.

Their worries run both ways. An example is William Sewell’s *Logics of History* (2005). Just as he worries about social scientists who apply models over broad swaths of history in ways that risk ignoring changes to context, temporality, and historical specificity, he laments how historians often do not attend to theory and methodology, substituting a “narrative overconfidence” in which the story can mask the absence of a systematic approach to causation or interpretation (Sewell 2005, 11).

Sewell’s main contribution is to ask us to think more richly about types of temporality. He distinguishes three kinds. These he calls teleological, in which outcomes are the result of large-scale processes, beyond events and often beyond agency, like modernization, state-building or capitalist development; experimental, in which time is seen as equivalent and the comparative method is used to tease out causal parallels across time; and eventful, sensitive to the context specific intertwining of structure and agency (Sewell 2005, 83). Interestingly

defining “events” not as anything that happens, but “as that relatively rare subclass of happenings that significantly transform structures,” he stresses “a causal dependence of later occurrences on prior occurrences and assumes that social causality is temporally heterogeneous, not temporally uniform” (Sewell 2005, 100–101). All three approaches to time, he says, can be productive, but only if what he calls the eventful approach is integrated with the others. This kind of history must be particularly attentive to how categories of understanding and analysis emerge and change at different moments, as meanings are not static (73ff). From this vantage, negotiation of choices are informed not only by particular contexts and situations but also by particular tools of meaning and understanding that actors have that are embedded in particular historical situations.

Located at the border between respect and dismissal, John Lewis Gaddis’s *Landscapes of History* offers an even more challenging assessment by a historian seeking to engage with the social sciences (Gaddis 2002). Though overstated, his complaints deserve to be taken seriously as condensed versions of reservations by historians. Gaddis sets out from the premise that apprehending history as a whole is impossible. As they seek to connect causes to consequences, he counsels, historians thus depend on approaches and methods that can move back and forth between more literal and more abstract representations of the past, and between generalization and particularization (Gaddis 2002, 12–15). This is exactly the challenge that confronts historically-oriented political science, a challenge he depicts in a metaphor of the tension between maps that depict reality and mapping as an act of choosing how to simplify in order to show what is real. Such acts perform the work of modeling, and they range from more to less simplified and from more to less logical as distinct from more to less empirical. History, from this perspective, depends not only on more or better facts, but on the quality of its methods and the character of its analytical reason.

Gaddis believes that many social scientists (as opposed to non-laboratory natural scientists) fall short in how they conduct such exercises, and it is to these issues that we should attend, despite his annoying tendency to caricature too broadly. His concerns fall into three main categories:

The first relate to the role of particularity and context when dealing with variables that possess a logic of tendency that can be generalized across time and space. To illustrate

his point, he uses the example of mountains. There is not an endless variety of types or shapes; all mountains are constrained by a logic of geometry. Notwithstanding, each actual mountain is particular and specific (Gaddis 2002, 83). History, too, has a limited range and trans-historical features. But, he insists, particularity matters. “Causes always have contexts, and to know the former we must understand the latter” (Gaddis 2002, 97).

From this point of view, context is not a kitchen-sink category, but a feature of reality that can be tied closely to a analyses of causality. Defined as “the dependency of sufficient causes upon necessary causes,” Gaddis treats context as those features of social reality in specific locations and moments that make a given cause a candidate for sufficient status (Gaddis 2002, 97). Taken seriously, this implies that any specific factor or variable only can gain such power in context, not outside it. This is a powerful cautionary lesson. It does not do away with more general claims about the logic of things, like the geometry of mountains, but it insists that this logic alters as parameters change.

Second, Gaddis is unhappy with what he represents, in exaggerated form, as the propensity of social scientists to treat dependent variables in terms of rich variation awaiting explanation by a discovery of the key independent variable. He wishes to invert this balance of parsimony and complexity, strongly preferring work that identified a single, highly-targeted object of analysis coupled to an acknowledgment that causation is often complex and interdependent.

Third, he observes that temporality matters causally. He insists not only that there is a constellation of causes, but that we have to be sensitive to their heterogeneity in time, noting the “distinctions that have to be made ... between the immediate, the intermediate, and distant” (Gaddis 2002, 95). This, in fact, is a point of emphasis rather similar to Sewell’s characterization of the causal effects of time and what Pierson tries to accomplish in *Politics in Time* in focusing on path dependency, heterogeneous causality, and contingency in an effort described as that of joining “structure, conjuncture, and events in a common causal universe” (Pierson 2004, 102, 109).

Taken together, the three concerns expressed by Gaddis invite political scientists to think more systematically about how, in Sewell’s terms, to bring “a serious infusion of historical habits of mind” into theory and empirical inquiries. It invites us, further, to read and

extract lessons sympathetically from how historians, and historically-oriented social scientists contextualize causal accounts, what they do regarding context, particularity, periodization and temporality, and how they treat the fatefulness and importance of sequence, the layering of processes, variations to rates of change, and the configurational contingency of events. So it is to relevant methodological questions that we now turn to suggest how we might develop a response to the call for a “sophistication about temporality” (Sewell 2005, 6).

### 3 Methodological issues

A major problem that historians and APD scholars have with causal empirical modeling is the manner in which a reduction in complexity inherent in such endeavors is carried out. Of course, some historians reject causal modeling out of hand, claiming that their mandate is simply to explain what, not why, things happen. But others, as we have seen, are much more sympathetic and attuned to the powers of social science, yet uncomfortable with how modeling of complex reality, especially quantitative modeling, proceeds.

This, we believe to be wholly explicable. After all, the workhorse quantitative method in political science is regression. By its very nature, the regression model is an attempt to represent complex parts of the world in a highly parsimonious way by taking a number of dimensions (i.e., a set of explanatory variables) and reducing them to one (i.e., the dependent variable). Some historians reject the notion that the world can be expressed in terms of such simple relationships. This criticism applies not just to dimensionality reduction, but also to the direction of causality. It is difficult for historians to accept that there are variables that are truly exogenous to others.

Although there are well-known methods for addressing issues of endogeneity, in practice the assumptions necessary for the methods to perform well are often not met. These methods involve complex systems of equations where certain variables are allowed to explain as well as be explained. While such systems can be easy to write down, data limitations may simply prevent empirical identification of the direction of causal arrows. We think that more progress can be made, however, by augmenting standard models with techniques that have only recently come to the attention of political scientists. These techniques work within the framework of the regression model in ways that satisfy a desire for parsimony while incorpo-



rating features historians see as crucial to representing historical events and development.

A key feature of an alternative approach is to posit more general models than are commonly employed and let the data tell us as much about the model's structure as possible. Historians often feel uncomfortable with the restrictive confines of a regression model. At some level, these kinds of restrictions are unavoidable. But we can incorporate more flexibility by having researchers impose less of an a priori structure on the phenomena under investigation.

The central features of history that simple regression models fail to capture are temporality, periodicity, specificity, and context. Temporality refers to the importance of the sequence of events, periodicity to the clustering of events along the dimension of time, specificity to the uniqueness of events in historical time, and context to larger environments within which events take place. There is some overlap among these concepts. Context may vary according to period. Thus understanding events may require grouping them into particular phases or epochs despite perpetual uncertainty about the nature of the periodization and the multiplicity of plausible ways to divide historical time.

The main reason that standard regression models are poorly equipped to address these concerns is that they typically treat parameters as non-varying over long stretches of history. It is not uncommon for political scientists to estimate a regression model over decades, even centuries where the relationship among variables is treated as constant over the entire period.<sup>1</sup> Coefficients on explanatory variables—which capture how several dimensions reduce to one—do not vary and thus assume away a significant amount of complexity in the development and evolution of historical processes. While such simple models are consistent with the desire to derive and test general theories, they raise an alarm for scholars who doubt the tremendous, indeed excessive, determinism that such models imply. While statistically significant relationships may give us confidence that we have discovered a grand and fundamental pattern of behavior, or an institutional truth, such models tend to impose far too much structure on data, concealing important nuances and providing an unsatisfying and perhaps

---

<sup>1</sup>For example, see Brady, Buckley, and Rivers 1999; Brunell and Grofman 1998; Schickler 2000; Wawro and Schickler 2006. We are, of course, in no way claiming that this is “bad” work. All we are saying is that historians may find the quantitative approaches adopted in these studies as lacking sensitivity to some of their concerns.

even incorrect account. We may draw the wrong inferences about an historical process if the relationship between variables is nonexistent in certain periods, but is exceptionally strong for others.

Parameter variation offers a potential solution. It permits the effects of explanatory variables to change along dimensions that we deem important. For historical analysis, time is generally the most important dimension. Suppose we want to analyze annual data over a broad swath of history. Time series analysis of the kind typically conducted in political science would likely estimate one coefficient value for a given explanatory variable for the entire series. Flexibility of the kind historians favor can be introduced by letting that coefficient vary over the series. But letting parameters be different for each observational unit in the series (i.e., each year), however, is not desirable either. Such a move makes it impossible for a univariate time series to identify annual coefficients, since it would yield as much, or more, parameter variation as variation in the data. Nor would this degree of parameter variation always be preferable, since it may very well leave us with results that are difficult if not impossible to interpret in a way that is useful for hypothesis testing or as a means to make any sort of general claim.

Yet an attractive middle ground is possible to locate, especially if we have data variation along another dimension besides time. What would be more helpful practically and theoretically is to allow coefficients to vary while finding a way to tie them together that can capture temporality, periodicity, specificity, and context. Multilevel modeling provides just such opportunities (Gelman and Hill 2007). As their name implies, multilevel models permit parameters to vary across different dimensions, whether they be cross-sectional, temporal, or spatial. Temporal variation can be captured with one level, exploiting variation in a different level. For example, suppose we were examining the effects of different factors on the roll call votes of members of Congress on civil rights measures over the course of the 19th and 20th centuries. Parameters on member-specific characteristics could be permitted to vary annually or by congress if we believe that different factors may have different effects over time. Or we may have specific periodization schemes where parameters would be constrained to be similar across specific years. While we could impose these constraints explicitly, the multilevel framework can be employed with Bayesian approaches that would enable us to have the data

determine to a large extent which periodization scheme is most appropriate.

Although Bayesian methods have been used with increasing regularity and success in political science, the lion's share of quantitative work since the behavioral revolution has been in the frequentist tradition. While we generally adopt a utilitarian position when it comes to methods, the philosophical foundation as well as instrumental application of Bayesian approaches may appeal more to historians than frequentist statistics.

The notion of repeated samples forms the basis of inference in the frequentist approach, since the properties of estimators are evaluated in terms of their performance in a large number of repeated samples (Judge, Hill, Griffiths, Lütkepohl, and Lee 1988, 117–118). While the idea of taking repeated samples often does not make sense for studies of contemporary political behavior (e.g., when our data is the universe of cases), it makes even less sense when studying behavior in a specific time period with a unique historical context (cf. Western and Jackman 1994).

Bayesian approaches may make more sense for these kinds of analyses, since they do not rely on notions of repeated sampling for assessing the properties of estimators and for hypothesis testing. History happens once. We are interested in understanding why it happened in a particular way. The standard historical tool that explore counterfactuals by “rerunning history” is different from how frequentists think about repeated sampling. Such sampling is supposed to be performed under identical conditions; by contrast, counterfactuals change one or more important conditions to explore alternative paths that history might have taken. Although some may argue that we are still studying data generating processes with stochastic elements and the historical period under examination is one realization of that process, the frequentist approach to inference can still be highly unsatisfying. A 90 percent confidence interval in the frequentist tradition tells us that the interval covers the true value of a parameter of interest in 90 percent of the repeated realizations. But there is no way to tell whether the interval constructed based on the observed data “is one of the fortunate 90 percent and [there is] no possibility of further replications” (Western and Jackman 1994, 414).

Another advantage of Bayesian methods is that, through the use of priors, they offer a systematic way to incorporate the rich information provided by historians' scholarship. The extant division of labor between the disciplines of history and political science seems to be

one in which historians attempt to tell us what happened by extracting relevant information from vast amounts of primary source material, while political scientists try to account for why events happened the way they did by deriving and testing theoretical conjectures.<sup>2</sup> Quantitative studies of historical data in political science draw heavily on the descriptive work of historians. The use of this kind of prior information through probability statements about parameters of theoretical interest is a fundamental feature of the Bayesian approach. Through priors, Bayesian approaches facilitate an explicit and formal way to incorporate this information, thus helping to bridge the gap between the descriptive work of historians and quantitative analyses by political scientists.

To return to the discussion of parameter variation, specific kinds of Bayesian models offer solutions to the problem of pooling observations over long stretches of history. Complete pooling of the data is most likely the kind of transgression that historians refer to when they complain that political scientists ignore the “texture and complexity” of history (Silbey 2000, 326). Allowing the effects to vary across the time dimension of the data builds in some complexity that complete pooling would ignore. Yet, of course, we cannot let parameter effects vary completely or we risk explaining nothing.

Bayesian multilevel models, also referred to as hierarchical models, can help us strike a balance between incorporating more of the complexity that historians like to see and imposing the kind of structure that is necessary to model behavior quantitatively with the goal of revealing underlying patterns.<sup>3</sup> For example, we could conduct the roll call analysis mentioned above using a hierarchical model where congresses are grouped according to the party systems within which they occurred. It is quite likely that roll call voting behavior will vary under different party systems given how parties line up along issue cleavages. Allowing parameter effects to vary across congresses in this way can help account for periodicity and variation in context. If we believe that particular variables are related to period-specific or contextual variation, we can model parameters as functions of these variables.

The downside to doing this, of course, is that we could get the periodization scheme or

---

<sup>2</sup>This is not to say that political scientists never engage primary resources. But it is fair to say that historians devote a substantially greater amount of their time and effort to mining archival sources.

<sup>3</sup>While multilevel models do not necessarily require Bayesian estimation, it will become clear shortly why we emphasize this approach over the frequentist approach.

contextualization wrong—whether by specifying it in terms of measurable variables or not—which could result in misspecification bias. Undoubtedly, there will be disagreement over any explicitly imposed structure on parameter variation. In cases where such disagreement is particularly contentious, we may choose to be less structural and let periodization or contextualization schemes be substantially data-dependent. One way to do this is to employ intrinsic autoregressive (IAR) priors, which we could use to smooth parameters such that the effects of variables will be more similar for adjacent time periods.<sup>4</sup> IAR priors are a type of Markov random field (MRF) prior, which set up a general functional relationship for the parameters for different observational units that captures the similarity or proximity of the units.<sup>5</sup>

For example, in a roll call analysis, immediately adjacent congresses could be designated as similar without specifying exactly the nature of the similarity apart from proximity. Let  $C_t$  denote a congress at time  $t$ .  $C_{t+1}$  and  $C_{t-1}$  would be designated most similar to  $C_t$ . The similarity would decay as we move away from  $t$ , so that we would posit that  $C_{t\pm 2}$  would be less like  $C_t$  than  $C_{t\pm 1}$ , and so on. In this way, parameters for a given congress would be estimated by “borrowing strength” from proximate congresses without imposing an explicit periodization scheme. Borrowing strength in this manner enables parameter variation without giving up too much in terms of the precision of estimates. As the parameters are smoothed across time periods, periodization/contextualization schemes would emerge dynamically based on what is occurring in the data. It is important to keep in mind that these are priors. They do not force parameters to be similar; they are merely “suggestions” that the estimation technique uses to find parameter estimates. We could examine the hyperparameters that perform the smoothing to see what patterns emerge in terms of which observational units are more similar than others.

For data where we do not have adequate variation to estimate parameters using the multilevel approach, alternative methods that are similar in spirit are available to uncover periodization. Structural breaks are a key concern for APD scholarship; (usually exogenous)

---

<sup>4</sup>For other examples of parameter smoothing priors in political science, see Bartels 1996.

<sup>5</sup>Initial work on MRF priors was undertaken by Besag 1974 (see also Besag 1975; Besag and Kooperberg 1995; Besag, Green, Higdon, and Mengersen 1995). For an accessible discussion on IAR priors, see Girosi and King N.d.

significant events occur and things are just different subsequently. Determining the existence of structural breaks and locating when they occur can be tricky with conventional methods. The textbook method for testing for a break in a time series is the Chow test. This test assumes that it is valid to break a series into two parts—before and after some posited significant event. Yet it could be the case that the series should be broken into more than two, and that the most important breaks occur at different points in time from those previously assumed. If there are more structural breaks in the data than specified, we could reach incorrect inferences. Standard tests of structural breaks require a specification of where they occur. It is better to be agnostic, and let the data tell us how many breaks there are and where they occur.

The method developed by Bai and Perron (1998, 2003) enables us to allow for multiple break points without specifying their locations. It identifies potential break points and provides tests to determine how many breaks, if any, occur in the series. Additionally, it gives measures of uncertainty around break point estimates through confidence intervals. Bai and Perron develop an “algorithm based on the principle of dynamic programming that allows the computation of estimates of the break points as global minimizers of the sum of squared residuals.” An advantage of this method is that it requires fewer least squares operations than a standard grid search would when investigating multiple breaks. The sequential procedure involves breaking the series into smaller and smaller possible partitions and checking to see which of these give the optimal fit to the data. The method can be used to assess whether there are breaks in the series itself or in terms of the effects of coefficients. For the former, the approach assesses whether there is a break in the mean of the series, by regressing the time series variable on a constant and then checking whether the intercept varies over the series.

## 4 Demonstrations

In this section, we demonstrate the feasibility of our methodological recommendations, including in our applications examples from our own work. Farhang and Katznelson (2005) investigate sectional influences on the construction of labor policy in the New Deal and Fair Deal eras. They contend that important changes in Democratic support for labor-friendly

policies took place over these periods. In the early years of the New Deal, southern Democrats behaved more like their northern colleagues because labor policy was explicitly designed so that it would not interfere with the southern system of racial apartheid. Specifically, domestic and agricultural sectors—occupations in which the majority of African-Americans were employed—were largely exempted from New Deal labor protections, continuing the long-standing tradition of Faustian bargains where liberals outside the south allowed a distinctly illiberal social and political order to perpetuate for the sake of maintaining majority coalitions. However, as labor unions began to make inroads in the south, senators and representatives from that section became less willing partners in the New Deal coalition. Labor mobilization efforts in the south threatened to undercut Jim Crow through the integration of unions. In response, southerners became less likely to vote with their northern counterparts and the Conservative Coalition emerged as a more decisive force in the Congress.

Farhang and Katznelson analyze roll call voting data from the 73rd through the 80th Congress (1933–1948), reporting “likeness scores” that clearly demonstrate the changing coalitional patterns. The case for the role that unionization plays, however, is made through narratives that discuss in detail particular cases of legislation considered during this period. The methods that we have proposed in this paper offer an alternative approach for testing the unionization hypotheses. The story that Farhang and Katznelson are telling can be conceived as one of changing parameters. It is not simply just that—as unionization in their region increased—southerners became less likely to support pro-labor policies. The southern reaction to union efforts became more virulent over time as they perceived a more severe threat to Jim Crow. Different kinds of parameter variation would capture more precisely how southern attitudes changed. The following multilevel voting model could be used to test the unionization hypotheses:

$$\Pr(y_{ij} = 1) = \text{logit}^{-1}(\alpha_{j[i],k[i]} + \beta_{j[i],k[i]}u_{ij} + \delta'\mathbf{x}_{ij}) \quad (1)$$

where

$$\begin{pmatrix} \alpha_{j,k} \\ \beta_{j,k} \end{pmatrix} = \begin{pmatrix} \mu_0 \\ \mu_1 \end{pmatrix} + \begin{pmatrix} \gamma_{0j}^{\text{period}} \\ \gamma_{1j}^{\text{period}} \end{pmatrix} + \begin{pmatrix} \gamma_{0k}^{\text{region}} \\ \gamma_{1k}^{\text{region}} \end{pmatrix} + \begin{pmatrix} \gamma_{0jk}^{\text{period} \times \text{region}} \\ \gamma_{1jk}^{\text{period} \times \text{region}} \end{pmatrix}$$

and

$$\begin{aligned} \begin{pmatrix} \gamma_{0j}^{\text{period}} \\ \gamma_{1j}^{\text{period}} \end{pmatrix} &\sim N \left( \begin{pmatrix} 0 \\ 0 \end{pmatrix}, \Sigma^{\text{period}} \right), \text{ for } j = 1, \dots, J \\ \begin{pmatrix} \gamma_{0k}^{\text{region}} \\ \gamma_{1k}^{\text{region}} \end{pmatrix} &\sim N \left( \begin{pmatrix} 0 \\ 0 \end{pmatrix}, \Sigma^{\text{region}} \right), \text{ for } k = 1, \dots, K \\ \begin{pmatrix} \gamma_{0jk}^{\text{period} \times \text{region}} \\ \gamma_{1jk}^{\text{period} \times \text{region}} \end{pmatrix} &\sim N \left( \begin{pmatrix} 0 \\ 0 \end{pmatrix}, \Sigma^{\text{period} \times \text{region}} \right), \text{ for } j = 1, \dots, J; \text{ for } k = 1, \dots, K \end{aligned}$$

$\Pr(y_i = 1)$  is the probability that senator  $i$  from region  $k$  in Congress  $j$  votes the pro-labor position. The subscripts indicate the various levels where we think there is potentially interesting variation. The variable  $u_i$  is a measure of unionization and  $\mathbf{x}_{ij}$  represents other measurable factors that affect the vote choice, but do so in a way that is constant across regions and periods.

The parameters  $\alpha$  and  $\beta$  are allowed to vary over region and time period. This captures an important nuance in Farhang and Katznelson’s argument. First, there is a temporal component: it could be southerners not only responded negatively to unionization, but that this response became more intense over time as it was perceived as a greater threat. Thus, there is a periodicity to the marginal effect of unionization. Second, the circumstances surrounding a particular vote or set of votes might produce contextual effects. Finding differences in parameter estimates across levels  $j$  and  $k$  indicates that context and periodicity were an important part of the unionization hypothesis, and that these innovations to the model are worthwhile.<sup>6</sup>

To measure unionization, we use Troy and Sheffin’s (1985) state-level data on the per-

---

<sup>6</sup>Another approach—one that perhaps would be more familiar—would be to estimate a model with interaction terms for region and period. We did attempt to estimate such a model, but experienced difficulties with model convergence and parameter stability. We have relatively coarse data and standard logit regression had a difficult time producing model estimates. While the gains that we get with multilevel modeling over pooling may not be that great since we have few groups in our levels, computationally we found significant advantages with the Bayesian approach. While we are not entirely comfortable with some of the independence assumptions that the multilevel modeling approach makes with respect to random effects and explanatory variables, we are for the moment willing to suspend disbelief to see what improvements this approach can offer.



centage of the non-agricultural workforce organized in unions.<sup>7</sup> Since we expect important differences across parties, we include a Democratic party indicator in  $\mathbf{x}_{ij}$ . The availability of the unionization measure led us to restrict our replication to the 75th, 78th, 79th, and 80th Congresses. This is somewhat problematic since it might attenuate the variation that we expect to see across the New Deal and Fair Deal periods. Farhang and Katznelson’s aggregate analysis suggests that unity that existed between southern and non-southern Democrats in the early New Deal had already begun to break down in the 75th Congress. Thus, we may see less dramatic changes in the parameters than we would if we included earlier congresses.

We estimated model the model in Eq. 1 using Markov Chain Monte Carlo methods.<sup>8</sup> The results are reported in Table 1 and Figures 1–2. The  $\alpha$  parameters are all bounded away from zero, capturing important variation across Congresses and region. They are all negative, indicating a general propensity to vote in the anti-labor direction, although there appears to be separation in the effects across regions. In the 75th Congress, the  $\alpha$  point estimates indicate that senators from the deep south and border south are generally less likely to vote pro-labor than non-southern Democrats, but there is overlap in their confidence intervals suggesting there is not clear separation in their voting patterns. However, in the 79th and 80th Congresses, there is no overlap in the confidence intervals between southern and non-southern Democrats, which is consistent with Farhang and Katznelson’s aggregate level analysis of coalition patterns. The coefficient on the Democrat dummy indicates, not

---

<sup>7</sup>State level data is available only for 1939 and 1953. We filled in the intervening years by doing linear interpolation. As an alternative measure of union activity, we collected data on work stoppages published by the Bureau of Labor Statistics (BLS) for this period. This is annual data and would not require interpolation. However, we would need to weight this variable with something like state population (which would require interpolation) or estimates of the size of the non-agricultural workforce in a state. Although we have collected the latter data, we are concerned that the BLS changed the way it computed these statistics over the period of interest. We will explore the use of this variable in future drafts.

<sup>8</sup>Although in theory the model could be estimated using canned routines for mixed effects models, such as `xtmixed` in `Stata`, `proc mixed` in `SAS`, and `lmer` in `R`, we found these options to be unsatisfactory. These routines either produced non-sensical results or no results at all. For example, `lmer` consistently returned negative estimates of variance parameters. We had much better luck using `WinBugs`, where we ran three Markov Chains in parallel and used redundant parameterization to help with convergence. The model converged after five thousand iterations. Details on convergence diagnostics are available from the authors upon request.

surprisingly, that Democrats are more likely to vote pro-labor than are Republicans, all else equal. Interestingly, the  $\beta$  coefficients capturing the variable effects of unionization have statistically significant and positive coefficients for border and deep south states in the 75th, 79th, and 80th Congress. Despite a general southern propensity to defect from the median party position on labor, unionization nonetheless in the south is associated with more pro-labor voting.

In order to gauge the magnitude of the effects, we present simulated probabilities for different values of the variables of interest in Figure 3. We computed point estimates and confidence bounds for the simulated probabilities for the different periods and regions, setting the value of the percentage of the workforce unionized variable equal to the relevant medians and then increasing it by one standard deviation. At median values for the unionization variable, deep south senators are generally the least likely to vote pro-labor, although the confidence bounds for the simulated probabilities overlap with border south senators in all congresses. Non-southerners' probabilities overlap with border state senators in the 75th and 78th Congresses, but not in the 79th and 80th Congresses. This separation is what we would expect to see given aggregate patterns analyzed by Farhang and Katznelson.

In the 75th, 79th, and 80th Congresses, a senator from the deep south became 10 percentage points more likely to vote pro-labor if his state experienced a one standard deviation increase in unionization. The same change in unionization in a border state is associated with a 20 percentage point increase in the propensity to vote the labor position by the state's senator during those Congresses. Non-southern Democrats experienced the smallest increase in the likelihood of voting pro-labor, but they generally were much more likely to vote that way in the first place. With the exception of border state senators in the 80th Congress, all of the 95 percent confidence intervals for the simulated probabilities for the initial cases and with the one standard deviation increase overlap. This means that we should perhaps be cautious in concluding that there are actual increases in the propensity to vote pro-labor given increases in unionization.<sup>9</sup>

One possible explanation for the results is that southerners felt cross-pressured on labor votes. While part of their constituencies did not like the threat to desegregation that

---

<sup>9</sup>The estimates of the  $\gamma$  parameters, which we do not report, indicate that there is more interesting variation across period than across region in the effects.

unionization presented, other constituents were actually being organized into unions, which perhaps meant that they could have been more easily mobilized to oppose a senator who was not sympathetic enough to labor. Southerners may have hedged their bets in places where unions were experiencing the most success at organizing. Border state senators most likely felt more cross-pressure than those from the deep south, given the stronger preferences for segregation in those states.

This model is merely a first step toward achieving the kind of complexity that we are advocating and does not go as far as we would like to in demonstrating the usefulness of this approach. We plan to do more in terms of freeing up the parameters of interest. Rather than pooling roll calls by Congress, we would like to estimate separate parameters for individual roll calls, which would allow us to distinguish among types of roll calls. With this level of analysis, IAR priors would be more appropriate, since we will need to borrow strength from temporally adjacent roll calls in order to get decent parameter estimates for a given roll call. Separating out roll calls in this manner would enable us to investigate in more detail the degree of cross-pressure that southern senators felt. It may be the case that southerners could have voted differently depending on the degree of visibility of votes in order to please the different segments of their constituencies. The hyperparameters that we would estimate would give more clues about the periodization in the data and possible variance over roll call votes within a given Congress. We plan to explore such possibilities in future drafts.

#### **4.1 Where's the break?**

Wawro and Schickler's (2006) analysis of coalition sizes is another study where the kinds of methods we are advocating could prove useful. In their analysis, they focus on the impact of the adoption of a supermajority cloture rule in the Senate in 1917. They find that coalition sizes on significant legislation generally increased after the adoption of cloture and conduct a number of tests to explain why this is the case. Their basic story is that norms and other constraints against parliamentary obstruction began to break down around the turn of the century, which led senators to seek a formal rule to curtail the use of filibusters. Contrary to what others have claimed, they contend that cloture was a meaningful reform that helps to explain the increase in coalition sizes by providing an institutional mechanism that senators

could invoke to reduce the uncertainty surrounding the passage of legislation by building larger (i.e., supermajority) coalitions.

Throughout most of their analysis, they assume that it is valid to break their time series on coalition sizes on significant legislation (which covers 1881 to 1946) into two parts—before and after the adoption of cloture in the 65th Congress. Yet it could be the case that the periodization of the series is more complicated, involving more structural breaks located at different time points. If there are more structural breaks in the data and if they are located far away from when the reform took place, this would call into question their conclusions about coalition sizes and their relation to cloture reform.

This is a tailor-made application for the Bai and Perron method discussed above. To implement this test, we use the average coalition size in each congress covered by the data as the dependent variable and allowed for up to five breaks in the series.<sup>10</sup> The sequential procedure for selecting breaks indicates that indeed only one break exists in the data, and that occurs at the 61st Congress. The 95 percent confidence interval for this break indicates that it may also have happened as late as the 62nd Congress. This is consistent with Schickler and Wawro’s argument that the dynamics of obstruction had changed during this period, but suggests a more nuanced story. Members of the Senate appear to have begun to build larger coalitions prior to the adoption of cloture, possibly attempting to thwart obstructionists by denying them the resources that a larger opposition coalition would possess. As senators saw the success that larger coalitions brought, it would make formal institutional reinforcement of this strategy all the more attractive. No additional breaks occur through the end of the time series, indicating that the trend in larger coalitions did not reverse or change in significant ways. The method employed allowed for more complex periodization in the data, but none emerged.

We also replicated part of Cox and McCubbins’s (2005) analysis examining the ability of the majority party to use agenda setting to move policy away from the median of the legislature as a whole, toward the median of the majority party. They focus on the importance of the Reed Rules, which were initially adopted in 1890 to crack down on parliamentary obstruction in the U.S. House of Representatives. In Chapter 4 of *Setting The Agenda*, they

---

<sup>10</sup>We cannot use individual bills since most congresses have more than one significant bill passed during them.

investigate the impact of the Reed Rules in moving policy outcomes toward the majority party's position and find a major impact for a dummy variable indicating whether or not a Congress occurs in the post-Reed era or not. We applied to the Bai-Perron method to the dependent variable that Cox and McCubbins use in their analysis—the proportion of final passage bills that move policy toward the majority. The sequential selection procedure indicates a break at the 53rd Congress. Although the Reed Rules were initially adopted in the 50th Congress, they were repealed when the Democrats assumed control in the 52nd and not fully reinstated until the 54th. Thus, this estimated break point makes sense. Interestingly, the 95 percent confidence interval for this break is quite wide, indicating that it could have occurred as early as the 49th Congress or as late as the 60th. The width of this intervals may be due to the fitful adoption of the rules.

## 5 Discussion

Given the preliminary and sketchy character of these reflections, suggestions, and tentative empirical forays, none of which is yet ready for prime time, this paper aims more at suggestion than definitive demonstration. We write with the concern that as the project of linking congressional to historical studies proceeds, forms of mutual incomprehension, semi-accurate caricature, and concern about the proper range of assumptions and research methods have erected barriers separating scholars with a rich and nuanced approach to history from colleagues who have fashioned systematic means to probe lawmaking. Even with the deepening of the historical project by systematic students of Congress and with the much more tentative but not absent turn to Congress by political historians and APD political scientists who, previously, had mainly worked on other sites, there exists a great deal of mutual skepticism. Yet unless such hesitation and doubt is overcome, both worlds of scholarship will be less robust than they might be.

Within the framework of identifying the main legitimate reservations about mainstream congressional scholarship that is historical by those who practice historical research as their primary craft, our main goal is that of suggesting how a thoughtful utilization of models that allow parameter variation can serve as a promising means, though not the only one, to bridge these research communities by systematically capturing just those features about parameters

and their alteration that most interest the historical community. From our perspective, not only is it imperative to take seriously the worry among historians and historical social scientists that mainstream studies of Congress fail sufficiently to attend to context, historical specificity, temporality and periodicity, but to acknowledge that unless such matters can be made constitutive features of inquiry, their skepticism will remain justified. Good research on past times cannot proceed by a flattening universalism, and certainly not by any such *a priori* assumption. Rather than expect models to predictably port across time, we should be building models that seek to internalize and reflect central historical features and processes by integrating parameter change inside their very construction.

In turn, though, such efforts, to the extent they succeed by illuminating the historical process, put pressure—a welcome kind of pressure—on political historians and the APD collegium to deepen and broaden their research repertoire. It simply is not good enough to express skepticism about, say, mainstream approaches to multivariate analysis and, with varying degrees of smugness and willful unawareness, continue with business as usual that either largely ignores Congress or treats specific historical episodes one at a time in a largely descriptive rather than causal manner.

The list of fundamental questions is long. It includes how to think about party in relation to preferences; issues concerning the number and substantive meaning of ideological and policy dimensions; questions about the structuration of lawmaking and the impact of organizational features on outcomes; the role of information; the ways patterns of representation orient constituency ties; the meaning of roll call votes and the balance between those which are partisan and those which are not; and the barriers to legislative success both within and outside Congress. These, among others, are the matters that concern, even obsess, political scientists who work on Congress, including colleagues who have turned to just these themes in past times. But these inquiries tend to proceed not only without attending to the major methodological concerns of those who more routinely conduct historical research, but with assumptions and methods that treat data with uniformity across historical time while discounting variations in time as they might apply to a wide array of relevant causal features.

Moreover, this rich list of questions hardly exhausts the relevant subjects of inquiry. For a serious project that deeply connects history and Congress must also attend to the agenda

of questions that concern the historical community, including the big questions about the complex character of the American polity as a liberal state, a state for which the legislature arguably is the central and most distinctive institution. If we wish to probe these issues, irrespective of our disciplinary orientations and methodological priors, we will need to find ways of working that, at once, are deeply substantive and systematic. Here, we have suggested, tentatively and illustratively, that approaches that privilege parametric variation might serve as a means to this end. To the extent that this, among other possibilities, is persuasive, it also implies obligations about the range of issues, literatures, and methods that quite disparate research communities might learn to share, and entails commitments that will be difficult, sometimes painful, to achieve. But the payoff might well be considerable.

## References

- Bai, Jushan and Pierre Perron. 1998. “Estimating and Testing Linear Models with Multiple Structural Changes.” *Econometrica* 66 (1): 47–78.
- Bai, Jushan and Pierre Perron. 2003. “Computation and Analysis of Multiple Structural Change Models.” *Journal of Applied Econometrics* 18: 1–22.
- Bartels, Larry. 1996. “Pooling Disparate Observations.” *American Journal of Political Science* 40: 905–42.
- Bates, Robert, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast. 1998. *Analytic Narratives*. Princeton: Princeton University Press.
- Besag, Julian. 1974. “Spatial interaction and the statistical analysis of lattice systems.” *Journal of the Royal Statistical Society, Series B* 36: 192–236.
- Besag, Julian. 1975. “Statistical Analysis of Non-Lattice Data.” *The Statistician* 24: 179–195.
- Besag, Julian, Peter Green, David Higdon, and Kerrie Mengersen. 1995. “Bayesian Computation and Stochastic Systems (with Discussion).” *Statistical Science* 10: 3–66.
- Besag, Julian and Charles Kooperberg. 1995. “On Conditional and Intrinsic Autoregressions.” *Biometrika* 82 (4): 733–746.

- Brady, David, Kara Buckley, and Douglas Rivers. 1999. "The Roots of Careerism in the U.S. House of Representatives." *Legislative Studies Quarterly* XXIV: 489–510.
- Brady, David W. 1973. *Congressional Voting in a Partisan Era: A Study of the McKinley Houses and a Comparison to the Modern House of Representatives*. Lawrence: University of Kansas Press.
- Brunell, Thomas L. and Bernard Grofman. 1998. "Explaining Divided U.S. Senate Delegations, 1788–1996: A Realignment Approach." *American Political Science Review* 92: 391–400.
- Cooper, Joseph and David W. Brady. 1981. "Toward a Diachronic Analysis of Congress." *American Political Science Review* 75 (December): 988–1006.
- Cox, Gary W. and Mathew D. McCubbins. 2005. *Setting The Agenda: Responsible Party Government in the U.S. House of Representatives*. Cambridge: Cambridge University Press.
- Farhang, Sean and Ira Katznelson. 2005. "The Southern Imposition: Congress and Labor in the New Deal and Fair Deal." *Studies in American Political Development* 19: 1–30.
- Gaddis, John Lewis. 2002. *Landscapes of History*. Oxford: Oxford University Press.
- Gelman, Andrew and Jennifer Hill. 2007. *Data Analysis Using Regression and Multi-level/Hierarchical Models*. Cambridge: Cambridge University Press.
- Girosi, Federico and Gary King. N.d. "Demographic Forecasting." Princeton University Press, forthcoming.
- Hofstadter, Richard. 1956. "History and the Social Sciences." In Fritz Stern, editor, *The Varieties of History: From Voltaire to the Present*, New York: Noonday Press.
- Judge, George G., R. Carter Hill, William E. Griffiths, Helmut Lütkepohl, and Tsoung-Chao Lee. 1988. *Introduction to the Theory and Practice of Econometrics*. New York: John Wiley & Sons.



- Kalyvas, Stathis. 1996. *The Rise of Christian Democracy in Europe*. Ithaca and London: Cornell University Press.
- Kalyvas, Stathis. 2006. *The Logic of Violence in Civil War*. New York: Cambridge University Press.
- Katznelson, Ira and John S. Lapinski. 2006. "At the Crossroads: Congress and American Political Development." *Perspectives on Politics* 4 (June): 243–260.
- King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry*. Princeton: Princeton University Press.
- Mansfield, Edward and Jack Snyder. 2005. *Electing to Fight: Why Emerging Democracies go to War*. Cambridge: MIT Press.
- Moe, Terry M. 1987. "Interests, Institutions, and Positive Theory." *Studies in American Political Development* 2: 236–299.
- Orren, Karen and Stephen Skowronek. 2004. *The Search for American Political Development*. Cambridge: Cambridge University Press.
- Page, Benjamin and Robert Shapiro. 1992. *The Rational Public*. Chicago: University of Chicago Press.
- Pierson, Paul. 2004. *Politics in Time: History, Institutions, and Social Analysis*. Princeton: Princeton University Press.
- Pierson, Paul and Theda Skocpol. 2002. "Historical Institutionalism in Contemporary Political." In Ira Katznelson and Helen V. Milner, editors, *Political Science: State of the Discipline*, New York: W.W. Norton. Pages 693–721.
- Schickler, Eric. 2000. "Institutional Change in the House of Representatives, 1867–1998: A Test of Partisan and Ideological Power Balance Model." *American Political Science Review* 94: 269–287.
- Sewell, William H. 2005. *Logics of History*. Chicago: Chicago University Press.

- Silbey, Joel H. 2000. "Current Historiographic Trends in the Study of the Twentieth-century Congress." *Social Science History* 24: 317–331.
- Tilly, Charles and Robert E. Goodin. 2006. "It Depends." In Robert E. Goodin and Charles Tilly, editors, *The Oxford Handbook of Contextual Political Analysis*, New York: Oxford University Press.
- Trachtenberg, Marc. 2006. *The Craft of International History: A Guide to Method*. Princeton: Princeton University Press.
- Troy, Leo and Neil Shefflin. 1985. *U.S. Union Sourcebook*. West Orange, NJ: Industrial Relations Data and Information Services.
- Wawro, Gregory and Eric Schickler. 2006. *Filibuster: Obstruction and Lawmaking in the United States Senate*. Princeton: Princeton University Press.
- Western, Bruce and Simon Jackman. 1994. "Bayesian Inference in Comparative Research." *American Political Science Review* 88: 412–423.

Table 1: Results for  $\alpha$  and  $\beta$  parameters from the Analysis of Labor Roll Call Votes in the Senate (75th, 78th, 79th and 80th Congresses)

	Parameter estimate	Std. Dev.	95% CI
$\alpha_{75th,Deep\ South}$	-4.7	0.6	[-6.1, -3.6]
$\alpha_{75th,Border\ South}$	-3.9	0.5	[-4.8, -3.0]
$\alpha_{75th,Non-South}$	-2.7	0.4	[-3.4, -1.9]
$\alpha_{78th,Deep\ South}$	-5.6	1.1	[-7.7, -3.6]
$\alpha_{78th,Border\ South}$	-3.8	0.8	[-5.4, -2.3]
$\alpha_{78th,Non-South}$	-1.6	0.7	[-2.9, -0.2]
$\alpha_{79th,Deep\ South}$	-4.2	0.7	[-5.7, -2.8]
$\alpha_{79th,Border\ South}$	-4.1	0.6	[-5.3, -3.1]
$\alpha_{79th,Non-South}$	-1.2	0.6	[-2.4, -0.2]
$\alpha_{80th,Deep\ South}$	-4.0	0.5	[-5.0, -3.0]
$\alpha_{80th,Border\ South}$	-4.5	0.3	[-5.2, -3.8]
$\alpha_{80th,Non-South}$	-1.0	0.2	[-1.5, -0.6]
$\beta_{75th,Deep\ South}$	12.8	5.5	[ 4.2, 25.6]
$\beta_{75th,Border\ South}$	8.1	2.4	[ 3.0, 13.0]
$\beta_{75th,Non-South}$	1.8	1.5	[-1.3, 4.9]
$\beta_{78th,Deep\ South}$	6.6	5.6	[-6.2, 16.3]
$\beta_{78th,Border\ South}$	6.7	4.0	[-0.4, 14.5]
$\beta_{78th,Non-South}$	-2.8	2.9	[-8.2, 2.2]
$\beta_{79th,Deep\ South}$	11.5	4.7	[ 1.9, 21.8]
$\beta_{79th,Border\ South}$	10.0	2.4	[ 5.0, 14.3]
$\beta_{79th,Non-South}$	3.2	2.3	[-0.6, 7.9]
$\beta_{80th,Deep\ South}$	8.9	3.2	[ 2.5, 15.0]
$\beta_{80th,Border\ South}$	8.0	1.3	[ 5.5, 10.6]
$\beta_{80th,Non-South}$	0.0	0.8	[-1.5, 1.5]
$\beta_{Dem}$	2.6	0.1	[2.3, 2.9]

*Notes:* Fit using WinBugs using 3 chains, each with 5000 iterations (first 2500 discarded), 1074 iterations saved.

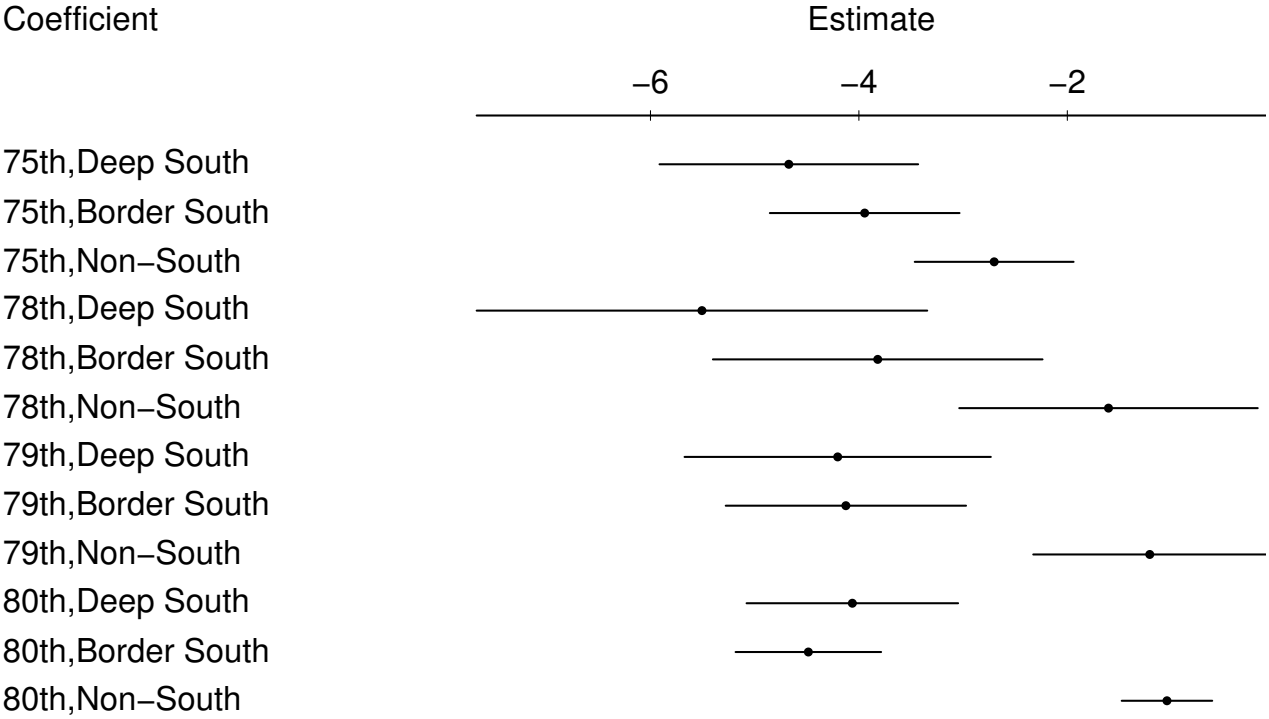


Figure 1: Estimated coefficients and 95% confidence intervals for  $\alpha$  parameters

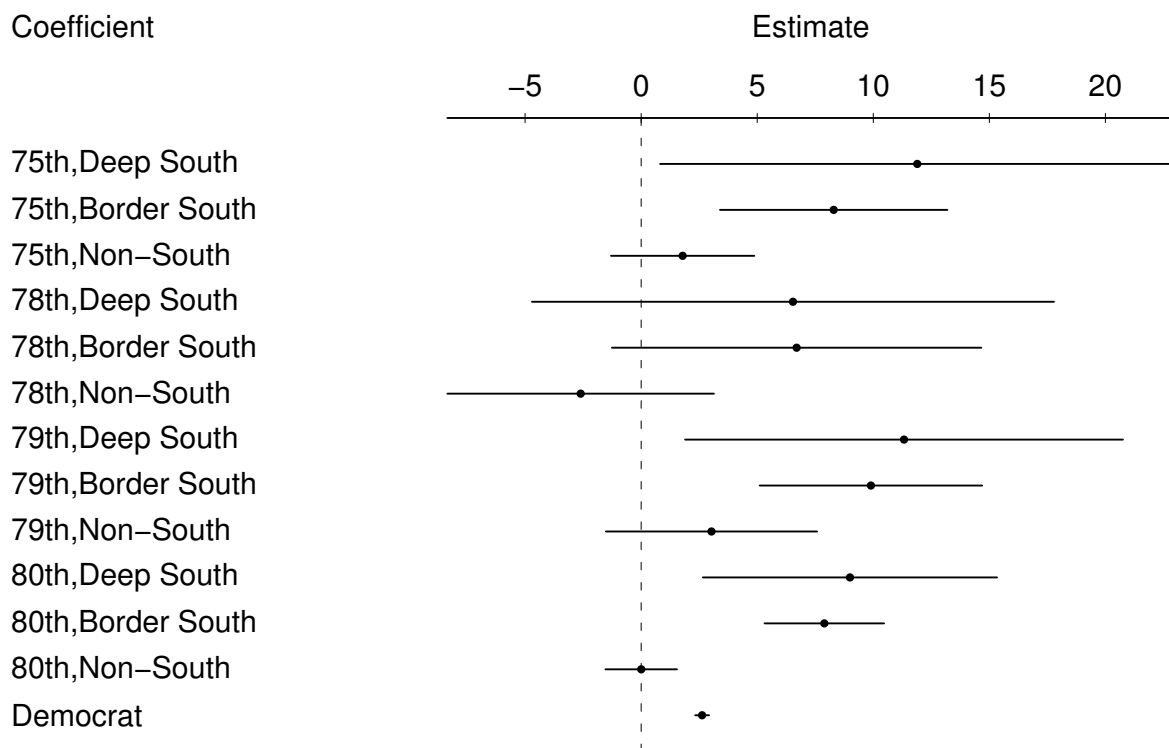
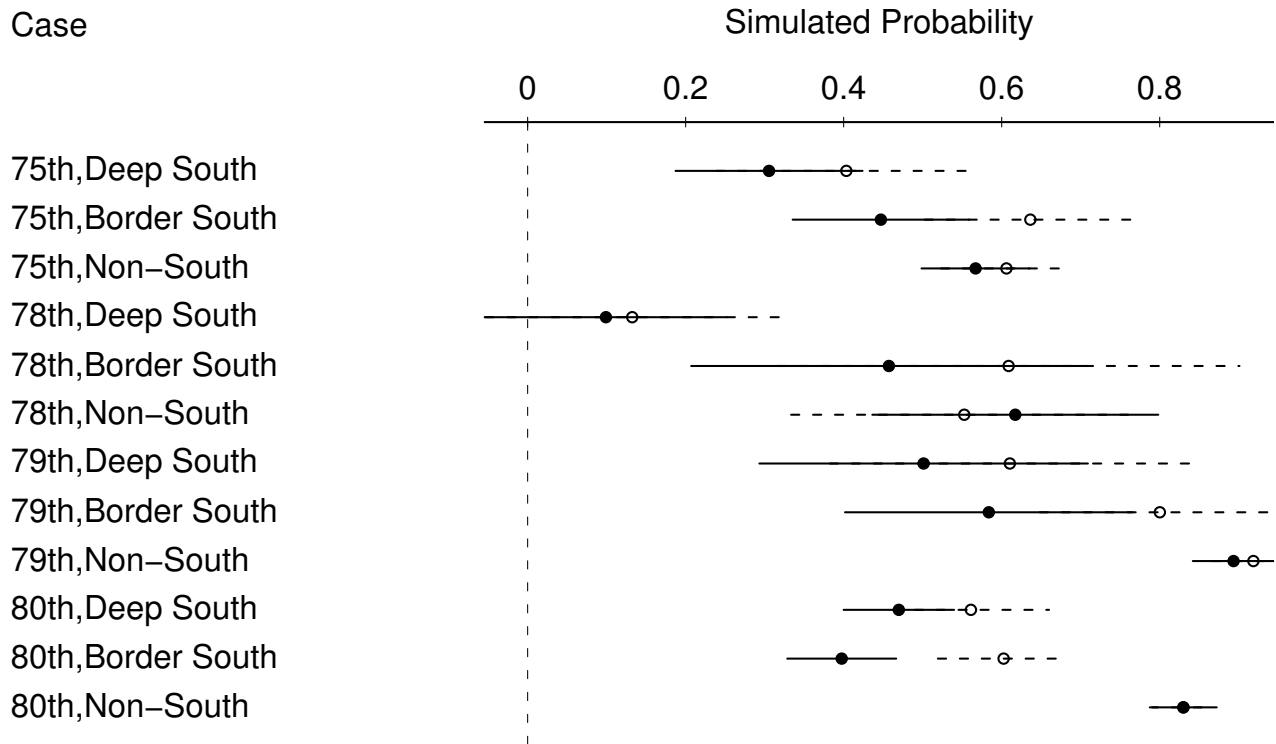


Figure 2: Estimated coefficients and 95% confidence intervals for  $\beta$  parameters

Figure 3: Simulated probabilities (with 95% confidence intervals) for Democratic senators



*Note:* The solid circles and lines represent the simulated probabilities and confidence intervals with percent of the workforce unionized set at its median value for the region and period. The open circles and dashed lines represent the simulated probabilities and confidence intervals with a one standard deviation increase in percent unionized.