

# Legislatures, Courts, and Statutory Control of the Bureaucracy across the U.S. States\*

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## **Abstract**

How do state legislatures use statutory language to control policy implementation by state agencies? In this paper, I consider—in a specific policy area and time period—the extent to which this decision is affected by legislative anticipation of the likely actions of state courts. Previous literature has argued that the legislative use of statutory language to control bureaucrats varies with the availability of nonstatutory methods of control, but it does not explicitly consider the potential role of courts. My expectations are derived from a simple formal model of executive-legislative relations and are supported when I test them using data on the number of words added to a state’s Medicaid laws from 1995-1996. In particular, I find that state legislatures write longer, more constraining, statutes when the likelihood that state courts intervene on their behalf is neither very high nor very low.

Literature in public administration and political science has long recognized that unelected bureaucratic agencies can significantly affect federal policymaking. For example, by cultivating reputations for neutral expertise, federal agencies such as the USDA, FDA, and OSHA (Carpenter, 2001, 2010; Huber, 2007) can autonomously determine the contours of federal policy within jurisdictional limits. Even when agencies cannot directly alter the legislative content of policy in this way, much research (e.g., Epstein and O'Halloran, 1999; Huber and Shipan, 2002) has confirmed that political principals often have an incentive to delegate policymaking authority to the bureaucracy. Students of state politics are beginning to take notice of bureaucratic policymaking and to assess the extent to which these characteristics of legislative-executive relations hold at the subnational level. As the roles of state bureaucracies have become more important, the field has paid closer attention to the conditions under which they can affect policy (Potoski, 1999; Poggione and Reenock, 2009).

In a forthcoming chapter on policy delegation across the states, Krause and Woods (2013) review recent literature on the subject and conclude that in order to better understand bureaucratic politics at the subnational level, scholars should begin with truly comparative state-level theories, rather than simply applying those theories generated at the national level. The key weakness in exporting national theories is that they do not adequately account for variations in relevant capacities. In particular, Krause and Woods (2013) seek to build a framework centered on the relative institutional capacities of state legislatures, governors, and bureaucracies. At a fundamental level, the current paper joins in their attempt to provide an institutional explanation for the diversity of executive-legislative relationships that we observe across the U.S. states. In particular, I assess the institutional determinants of the amount of statutory discretion that state legislatures delegate to state agencies. I examine the extent to which legislative capacity might condition legislatures' strategic delegations of policymaking authority, but the real thrust of the theoretical approach holds that the likelihood of exogenous ex post monitoring by the courts should most strongly determine this strategic calculus. In so doing, I add state judicial branches to Krause and Woods's matrix of state-level institutional variables to consider in studying subnational policy

delegation.

The key contribution of this paper is a demonstration that state legislatures strategically consider characteristics of their state’s supreme court when they impose statutory language meant to limit agency discretion. In particular, I argue that when legislative majorities believe state courts to be completely inimical to their interests *or* strong and vigorous allies, they will have little incentive to write detailed implementation instructions to agencies. Conversely, when legislators are most unsure of the potential level of assistance from the courts, they should make an effort to limit agency discretion *ex ante*. Rather than paying the costs of oversight, this allows legislatures to rely on the agencies themselves to moderate their implementation strategies in the shadow of potential punishment. To assess these expectations, I reanalyze Huber, Shipan and Pfahler’s (2001) and Huber and Shipan’s (2002) data on statutory discretion in Medicaid policy across 48 states in 1995-1996. In support of my argument, I find clear evidence that legislatures anticipate the actions of state courts when they craft their policymaking strategies. This is an important contribution because it adds an additional “separation of powers” nuance (de Figueiredo Jr., Jacobi and Weingast, 2008) to the extant literature on substitution effects between *ex ante* and *ex post* strategies<sup>1</sup> (Bawn, 1997; Huber, Shipan and Pfahler, 2001; Huber and Shipan, 2002; Gailmard, 2002). Although I focus empirically on the role of state courts, the theoretical arguments are general and may apply to any exertion of *ex post* veto power by extra-legislative actors. In a cross-national comparative context, for example, scholars may assess the roles of supranational governments (e.g., the European Union) or powerful internal actors (e.g., the military in many Latin American, African, and Asian countries) in affecting the policymaking of legislatures by using the same logical framework developed here.

In the next section, I briefly review the literature on the institutional design of bureaucratic agencies, focusing specifically on the conditions under which legislatures delegate—or do not

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<sup>1</sup>I further address the distinction between *ex ante* and *ex post* mechanisms of control later in the paper, but by way of introduction, *ex post* (or “after the fact”) controls focus on correcting undesirable bureaucratic behavior after it has occurred (e.g., oversight hearings). *Ex ante* strategies are distinguished as they take place “before the fact” and involve establishing structures which incentivize, or otherwise promote, desirable agent behavior.

delegate—policy authority to agencies. I then derive unique nonlinear predictions from a general model of legislative policymaking and formulate empirical expectations concerning the mechanism by which the character of state courts might affect statutory discretion across states. Next, I operationalize the key theoretical variables and construct the appropriate empirical models to test these expectations, including the specification of semiparametric Generalized Additive Models (GAM). The fourth section presents the results of the different model specifications, demonstrating broad support for the insights of the theoretical model, and the final section concludes with a discussion of contributions, shortcomings, and implications for future work.

## Variation in Legislative Policymaking Strategies

According to Black’s Law Dictionary, *discretion* (in this sense) is “4. A public official’s power or right to act in certain circumstances according to personal judgement and conscience, often in an official or representative capacity” (Garner, 2006). Especially when considering the “representative” nature of this definition, it is natural to consider this power to be constitutionally in the purview of legislatures in separation of powers systems. Indeed, under a strict separation of powers interpretation of the Constitution of the United States, delegation of discretion from Congress to executive agencies is to be avoided unless “Congress prescribes an intelligible principle to guide an executive agency in making policy” (Garner, 2006, p.362)—which is to say that Congress can only delegate when it does so without granting much or any discretion. In constitutional law, this is known as the non-delegation doctrine,<sup>2</sup> but the practical realities of modern government lead this principle to be mostly ignored. Legislatures (Congress and U.S. state legislatures) do in fact give great discretion to administrative agencies to implement policy that may or may not reflect the will of the legislature. The structure of this problem is precisely what makes it amenable to the principal-agent approach taken in the literature.

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<sup>2</sup>“The principle (based on the separation-of-powers concept) limiting Congress’s ability to transfer its legislative power to another governmental branch, esp. the executive branch” (Garner, 2006, p.362).

Contesting claims that legislative grants of administrative discretion are driven by legislative impotency and lead to administrative dominance (e.g., McConnell, 1966; Lowi, 1969; Niskanen, 1971; Putnam, 1975; O'Connor, 1978; Peters, 1981; Aranson, Gellhorn and Robinson, 1982), McCubbins and Schwartz (1984) and McCubbins, Noll and Weingast (1987, 1989) argued that legislators, as the principals, can alter bureaucratic incentives with statutory language. That is, although legislators need to delegate, they can maintain some degree of control over what their administrative agents do with their delegated discretion. This literature draws on descriptive accounts of the federal Administrative Procedures Act of 1946 (Davis, 1978; Shapiro, 1982; Bonfield, 1986; Gellhorn, 1986) to argue that procedural requirements (stipulated in APAs or in individual statutes) can help to reign in potentially recalcitrant bureaucrats through the “politics of structural choice” (Moe, 1990). In addition to procedural limits on discretion, scholars have proposed that legislators can limit discretion more directly, by controlling the specificity of the legislation delegating authority to bureaucrats (Epstein and O'Halloran, 1994, 1996, 1999; Huber, Shipan and Pfahler, 2001; Huber and Shipan, 2002).<sup>3</sup>

Huber and Shipan (2002) liken statutes to blueprints: by including more specific prescriptive language into a statute, legislators make implementation of the policy program included in that statute unambiguous. When there are fewer specific steps for bureaucrats to follow in implementing a statute, it is natural that they can more readily consider their own “personal judgement and conscience”—that is, their own discretion. Taking these two types of *ex ante* strictures on discretion together (procedural and statutory), de Figueiredo Jr., Jacobi and Weingast (2008) discuss a potential “separation of powers” confounding factor that I explore further in this paper:

By introducing strict limits of discretion, administrative procedures ensure that outcomes will be closer to an elected official's ideal than if the agency had an unlimited range of options. But the mechanism only works if there is *ex post* enforcement of the

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<sup>3</sup>I say that a prescriptive limiting of statutory discretion is more direct than procedural arrangements because they are more specific in nature and the policy outcomes are more certain to legislators. In addition, empirical research has demonstrated that procedural arrangements may be largely ineffectual (Balla, 1998; Hamilton and Schroeder, 1994).

rules. . . If the courts ruled consistently with the intent of Congress, then the bureau would have strong incentives to follow their intent. On the other hand, by implication, if the courts were not aligned with the legislature, such mechanisms would provide the bureau with more latitude to implement policy (p. 214).

Where the structure of my approach is similar to the literature reviewed here, the implications of the model introduced in the next section are unique precisely because I consider the extent to which ex post capacity (through, say, court reversal or oversight hearings) manages to enforce the limits of ex ante discretion. Put simply, my approach is comprehensive in that it incorporates the insights of the works reviewed below in a theoretically synthetic way. Before I translate the theoretical insights into specific empirical expectations and evaluate them in succeeding sections, I first delineate some alternative or complementary explanations for levels of statutory discretion from the literature.

Previous literature suggests that legislatures delegate varying levels of discretion to bureaucrats in accordance with inter- and intra- institutional variation. At the inter-institutional level, a host of scholars have argued that legislators consider the extent to which their policy preferences diverge from the bureaucratic agents' to whom they wish to delegate (Epstein and O'Halloran, 1999; Potoski, 1999; Huber, Shipan and Pfahler, 2001; Huber and Shipan, 2002; Lewis, 2003; Wood and Bohte, 2004). In the context of principal-agent theory, this is an entirely plausible explanation for why some legislators, at some times, write more or less restrictive legislation than others. Legislators simply trust like-minded policy-implementers more than those with starkly different preferences from them and, assuming that restricting discretion with statutory language is costly, seek to minimize their transaction costs without incurring unacceptable policy losses.

In addition to this preference-based story, Bawn (1995), Epstein and O'Halloran (1999), and Huber and Shipan (2002) argue that the cost of restricting discretion can increase with the complexity of a policy area. As a corollary, their arguments imply that variation in statutory discretion across legislatures can be partially attributed to variation in legislative capacity to deal with

(i.e., their ability to design policy that achieves their preferred outcomes) technically/scientifically complex policy areas. A second potential intra-institutional consideration is the extent to which legislatures can rely on alternative, and potentially less costly, means to control bureaucratic decisionmaking. A central insight of Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002) is that as legislatures become better able to control policy ex post (say, through oversight activities), they have fewer incentives to incur the costs of ex ante restrictions on discretion. However, this literature does not go beyond this simple substitution calculus and fails to explicitly consider the extent to which external institutional actors, like the courts (de Figueiredo Jr., Jacobi and Weingast, 2008), can affect this decision.

As much of this literature (with the important exceptions of Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002)) focuses on the relationship between Congress and the federal bureaucracy, it may miss important mediating effects of cross-institutional variation (Huber, Shipan and Pfahler, 2001; Krause and Woods, 2013). Besides these studies, there have not been many cross-state empirical assessments of statutory discretion. In fact, there is a dearth of empirical legislative-executive studies at the state level in general. What we do know is that legislative capacity varies across the states and this has predictable effects on legislative control of state bureaucracies (Elling, 1979; Hamm and Robertson, 1981; Potoski and Woods, 2000; Woods and Baranowski, 2006) and that institutional change, such as the imposition of legislative term limits (Berman, 2004; Carey, Niemi and Powell, 2000; Carey et al., 2006; Farmer and Little, 2004; Kousser, 2005; Kurtz, Cain and Niemi, 2007; Sarbaugh-Thompson et al., 2010), can potentially change the nature of state legislative-executive relationships. The current research adds to this literature by incorporating insights from a cross-institutional theory of statutory control of bureaucracy into a cross-sectional empirical model at the level of the U.S. states.

# A Theory of Strategic Delegation and Oversight

The theoretical model from which I derive empirical expectations for this paper is influenced by the delegation models in Huber and Shipan (2002). This work emphasizes the importance of statutory means for controlling bureaucratic action. Legislators write laws that delegate variably broad authority to bureaucrats. If they want to more closely control bureaucratic behavior, they can write more detailed laws, thus constricting the scope of an agency's discretion. However, this literature has established that constricting discretion can sometimes be superfluous; that is if legislators think that bureaucrats, acting with their own self-interest in mind, will implement policies in line with the preferences of the legislators. In such a context of complete delegation, oversight may be an even more essential tool for legislators than it would be if they had delegated less discretion. Under different conditions, though, oversight may be just as superfluous as statutory constraints on agency action. In order to determine whether this is the case and to establish the conditions, I consider both *ex ante* (delegation of statutory discretion) and *ex post* (legislative oversight) mechanisms of control simultaneously in an analytical model. The full treatment of the model can be found in the Appendix to this article, but I will briefly summarize the logic of the empirical predictions it yields.

As is standard in principal-agent models of legislative policy control, I make a number of simplifying assumptions to keep the model tractable enough to yield testable hypotheses. There are two types of players, "Legislators" and "Bureaucrats." The Legislator is considered to be a pivotal legislator in a legislature or committee and the Bureaucrat a key decision-maker in an executive agency. Quite simply, Legislators design *policy*, which the Bureaucrats implement, resulting in *policy outcomes*. I assume that both players care solely about policy outcomes, but that Bureaucrats are always better informed about the mapping of policy to policy outcomes. Legislators and Bureaucrats need not have the same policy preferences as each other, but they may.

I assume that writing statutes is costly for the Legislator and that the cost increases as the

capacity of the Legislator to write detailed laws decreases and as the extent to which these laws are specific increases. I also assume that it is costly for a Legislator to investigate if she thinks that the Bureaucrat has acted illegally (i.e., outside of the bounds of discretion). This cost is also increasing with the extent to which the Legislator is generally unable to write detailed laws. Bureaucrats are not literally bound by delegated limits on discretion and may or may not choose to implement the policy chosen by the Legislator. Nevertheless, acting in a way that the Legislator disapproves of can lead to an investigation (i.e., an oversight hearing), which will be costly to the Bureaucrat. I assume that both players have linear spatial utilities and that they each have one ideal policy outcome.

In general, the sequence is simple and the details are included in the Appendix, so I will briefly outline how the game is played out. First, Nature<sup>4</sup> determines a policy shock. Either the policy outcome will equal what the Bureaucrat implements or the outcome will shift one unit to the left of where it is implemented. The Bureaucrat knows the value of this shock, but the Legislator can only use the the Bureaucrat's behavior to infer its value. The first strategic action is taken by the Legislator who writes a law delegating an amount of policymaking discretion to the Bureaucrat. She takes into account how costly it is to write the law and her expectations that more restrictive laws could constrain the Bureaucrat's behavior in ways favorable to her. Next, the Bureaucrat implements a policy, be it either legal or illegal (i.e., outside of the bounds of discretion set in the previous stage). Finally, the Legislator observes which policy has been implemented and can choose to investigate (hold an oversight hearing) or not. If she investigates the outcome goes to her ideal point, but if she does not, the outcome is what the Bureaucrat implements with or without the policy shock. If the Legislator investigates and the Bureaucrat has acted illegally, he must also pay a cost, so he prefers to not be investigated. As the rounds of play are completed, there is an exogenous chance (call this probability  $\gamma$ ) that some nonstatutory, nonoversight mechanism benefits the Legislator and reverts the outcome to her ideal point. This

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<sup>4</sup>This is a standard way of introducing uncertainty and informational asymmetry into the structure of the model.

is an important factor that produces the implications for the role of courts in the policymaking process as examined below.

Having introduced the model in broad strokes, I will characterize how it differs from Huber and Shipan's (2002) model and describe how this is consequential. First, Legislators can engage in strategic oversight here—Huber and Shipan (2002) include legislative oversight behavior strictly exogenously. The upshot is that Huber and Shipan (2002) predict a pure substitutive relationship between ex post oversight and ex ante restriction of statutory discretion. When ex post capacity increases, legislatures should be less likely to pay the costs of restricting discretion. In contrast, the implication of the model summarized above is that the two strategies can often be complementary, with ex post oversight used to enforce the limits of stipulated levels of ex ante discretion. In a complementary paper (McGrath, 2013, *forthcoming*), I examine the determinants of oversight activity. Interestingly, the model predicts differing determinants of oversight and of legislative restriction of statutory discretion. In contrast with the literature reviewed above—which focuses on policy conflict and legislative capacity, both of which are found to affect oversight activity—the model presented here highlights the importance of the potential for nonlegislative oversight activity (the  $\gamma$  from above) to affect delegation of discretion. In finding that strategic complementarities exist between ex ante and ex post means of legislative control, I argue that it might be unreasonable to expect unconditionally that there is a pure substitutive relationship between them.

Propositions 3 and 4 (found in the Appendix) make general comparative statics statements about how exogenous extra-legislative oversight does or does not affect the initial legislative delegation of discretion. For the purposes of the current paper, it should be useful to translate these abstract propositions into a testable empirical hypothesis. First, I should note that the model itself makes predictions about statutory control under conditions of differing levels of ex post monitoring by an extra-legislative actor. There is nothing in the model that stipulates that this *has* to be the courts. However, in operationalizing the implications of the model, I think of the empirical referents of these propositions as state courts. Again, this is not strictly and implicitly modeled, but it is consistent with the role that courts play in the separation of

powers systems across the U.S. states.<sup>5</sup> In addition, it is consistent with Krause and Woods’s (2013) call for incorporating relevant, but heretofore understudied, state-level institutions into our understanding of comparative delegation research.

Proposition 3 stipulates that when  $\gamma$  is either sufficiently low *or* sufficiently high, legislatures have very little incentive to limit agency discretion.<sup>6</sup> In these circumstances, when legislatures perceive the courts to be a strong threat to overturn agency actions (when  $\gamma$  is high), they have little incentive to pay the costs of writing detailed legislation. Likewise, when they foresee an unsympathetic or restrained court (when  $\gamma$  is low), legislators have incentives to crack down on agencies with their own ex post enforcement measures (i.e., frequent oversight hearings) instead of relying on the courts to monitor agencies on their behalf. In contrast, Proposition 4 holds that legislatures are most likely to try to control bureaucratic behavior via detailed statutes when  $\gamma$  is *neither* sufficiently low nor sufficiently high. The model therefore predicts that legislators make the most vigorous efforts to limit statutory discretion ex ante when they are most unsure of the level of exogenous assistance they will receive from the courts. While it is important to note that the proposed mechanism rests on the agency’s strategic ability to moderate their implementation strategies in the face of punishment, the doling out of such punishment is not the empirical focus of this paper.<sup>7</sup> To review, I expect that *when the likelihood that the courts intervene on behalf of a legislature is neither too low nor too high, the legislature will write detailed statutes restricting the scope of the bureaucracy’s discretion.* This prediction is novel in the literature, but it is a

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<sup>5</sup>It is important to note that states differ with respect to selection methods for their high courts. 38 states rely on some form of popular electoral control through challengeable elections (be they partisan, nonpartisan, or retention), while the remaining 12 utilize an appointment procedure allowing for life-like terms (roughly analogous to U.S. federal courts). To account for this variation in the exogeneity of judicial selection, I have estimated the models below on that subset of states with judicial elections for the state supreme court. The results reported herein hold on this subset where we can be more sure of institutional exogeneity.

<sup>6</sup>The model technically predicts this relationship to hold when there is “sufficient” policy conflict (i.e., when the Legislator and Bureaucrat do not perfectly share each other’s preferred policies). The empirical analyses below assume, justified by the fact that agencies often serve multiple, competing, principals, that perfect policy agreement does not exist in the real world. I therefore assume enough policy conflict for Propositions 3 and 4 to be the relevant comparative statics analyzed here.

<sup>7</sup>Again, see McGrath (2013, *forthcoming*) as a complementary empirical paper to this, focusing more on the predictions of ex post punishment than on ex ante restriction of discretion. As further discussed in the conclusion to this article, collecting information on oversight activity at the state level is too time-consuming and arduous a process for the current paper.

clear implication of the theoretical approach that I have followed in this project. In the next section, I turn to assessing the empirical veracity of this argument and use it to explain legislative policymaking in the U.S. states for a particular policy in a particular time period.

## Data and Methods

The U.S. states provide the ideal context in which to test the expectations of the theory. First, as we will see, there is great variation in the extent to which state legislatures constrict agency action by limiting statutory discretion. Although it is true that congressional bills also vary in this regard, there exists no cross-sectional or regularly changing temporal variation in the institutional context of Congress. Most importantly for the analyses required here, it is difficult to operationalize the theoretical  $\gamma$  term in a way that yields variation at the congressional level. Crucially, there are myriad potential ways to consider differences in the effectiveness of exogenous nonstatutory controls across the states.

Research by Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002) similarly considers the effects of institutional variables on statutory discretion across the states. Therefore, I reassess the data<sup>8</sup> used in these works in light of the expectations derived from the theory described above. Before I describe the Huber and Shipan [and Pfahler, in 2001] independent variables and their expectations regarding them, I will describe the dependent variable and its measurement and explain how I choose to operationalize nonstatutory controls (the theoretical  $\gamma$ ) as the role of the courts across the states.

The dependent variable is the total number of new words that a state legislature put into law in the Medicaid (nonappropriations) policy area in 1995-96. When comparing statutory content, it is essential to control for issue area. A natural way to do that is to focus on a reasonably narrow issue that all states must deal with contemporaneously— Huber, Shipan and Pfahler (2001) argue convincingly for the appropriateness of Medicaid data from this time period on

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<sup>8</sup>Acquired via personal correspondence with the authors.

these grounds.<sup>9</sup>

These authors argue that the length of a statute, controlling for narrow policy area, is at least a proxy for the amount of statutory discretion given to a state health agency. The idea is that the longer a law is, the more detailed it should be in terms of instructing and directing (constricting) agency action. It certainly could be the case for a law to be relatively short in length, but full of discretion-limiting procedures. This hypothetical law would be more restrictive than a much longer, but procedure-less law, yet Huber and Shipan (2002) find that, at least for the sample they analyze, “procedures seem to play a minor role, relative to policy instructions, in all contexts” (p.72).<sup>10</sup> Since I am using the same sample of data, I argue with Huber and Shipan (2002) for the appropriateness of statute length as a proxy for the amount of statutory control exercised by a legislature in this policy area. It is also possible that laws from the 1995-1996 sessions of state legislatures are laden with references to previously passed legislation. If this were true, then what seems to be a permissive statute (containing few words) might in fact be far more restrictive than its length indicates. In their initial justification for this measure, Huber, Shipan and Pfahler (2001) recognize this potential problem, but report (p. 336) that state legislatures had done relatively little lawmaking on Medicaid policy prior to the dual pressures of rising costs and federal pressure to focus on the issue.

In their research, Huber and Shipan [and Pfahler, in 2001] give examples of these bills and

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<sup>9</sup>Here is a description of the coding rule for the dependent variable:

We identified relevant legislation in each state by searching Lexis’s “Advanced Legislative Service” database. For each state we used the search terms “Medicaid” and “medical assistance,” which are used interchangeably by states to refer to the Medicaid program, as well as any state-specific names for Medicaid programs (such as “Medical” in California or “MC+” in Missouri). We retained any nonappropriations bills that turned up in this search that were related to the provision of medical care for Medicaid participants. We then examined the content of the bill for relevance, and if it was only partially relevant (i.e., only partly about Medicaid health care) we edited out the irrelevant portions. We then used a macro in Microsoft Word to count all the words in the legislation that were new. This count of new words is the dependent variable, *Statutory Control*, that we focus on in our empirical tests (2001, p. 336)

<sup>10</sup>See Huber and Shipan (2002, pp. 56-72) for a series of diagnostics showing the relatively scant use of procedural language in the state Medicaid statutes and demonstrating that, in any event, procedural restrictions tend to be correlated with statutory control.

the extent to which they constrain bureaucratic policymaking. Huber, Shipan and Pfahler (2001) use Massachusetts House Bill 6107 as an exemplar of a highly restrictive piece of legislation. This particular bill exceeds 3,000 words and specifies “precisely who should be enrolled in managed care and how enrollment must occur for the chronically ill, the disabled and the long-term unemployed... conditions under which potential clients may be denied eligibility... where clinics should be located, how potential clients should be notified about the program, the role of school-based clinics..., among other things” (Huber, Shipan and Pfahler, 2001, p. 337). In contrast, Alaska Bill 393 barely exceeds 600 words and excludes specific details such as the preferred locations of pilot projects or eligibility requirements. Huber and Shipan (2002, Chapter 1) further identify exemplars of restrictive legislation (Texas Senate Bill 1574, exceeding 2,000 words, mostly specific mandates) and discretion-heavy legislation (Idaho House Bill 421, a mere 35 words in length). These examples are meant to highlight the face validity of the word count measure, but should also indicate the *importance* of the types of bureaucratic policymaking that discretion-granting laws allow. By allowing the Idaho Department of Health and Welfare to decide eligibility requirements and to designate sites for pilot programs, the state legislature cedes a good amount of distributive power to unelected government actors. It is important to highlight here that word counts are meant to be proxies for *actual* policies with *actual* consequences, rather than the quantities of substantive interest in themselves.

Although the previous empirical research on the determinants of such statutory discretion considers the effects of nonstatutory means of control, it usually does so with a indicator variable for the presence or absence of some extra-legislative power. For example, Huber, Shipan and Pfahler (2001) argue that the ability for state legislatures to veto administrative rules should mitigate their need to impose statutory constraints. Since I predict that statutory discretion is driven by the extent to which nonstatutory factors vary continuously, an indicator variable would be of little use to test the theory. I therefore need to construct a continuous measure of nonstatutory factors that may reduce [or increase] the incentive for legislators to write detailed statutes.

Decisions by the courts, especially concerning the appropriateness of administrative rules made by state agencies, affect policy outcomes well after laws have been written by legislatures. Therefore, the extent to which state courts may be favorable to legislative preferences should affect the initial delegation of discretion to state agencies. To capture the extent to which courts might constitute an ex post ally of the legislature (i.e., act like the theoretical  $\gamma$ ), I calculate the proportion<sup>11</sup> of state supreme court cases that involved an agency where the court reversed agency action. These data are made available by the State Supreme Court Data Project, managed by Paul Brace and Melinda Gann Hall.<sup>12</sup> This measure captures judicial antagonism to agency action, which is an important variable given the concept of nonstatutory control. So, very high values of *Percent Overturned Agency Decisions* indicate a state where the supreme court overturns agency action at a high rate, where very low values denote a relatively agency-friendly court. As developed above, my expectations regarding this variable are parabolic, rather than linear, so it is important that there exist values across its range. The summary information found in Table 1 confirms that there is much variation in this measure across the states for the time period studied. To review, my expectation is that *Statutory Control*, measured by the number of words added, should increase only when nonstatutory controls—operationalized here as the activism of a state’s supreme court<sup>13</sup>—are neither too high nor too low.

**Table 1 goes here.**

I begin by following Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002) and

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<sup>11</sup>To ease interpretation, I later transform this variable into a percentage by simply multiplying it by 100.

<sup>12</sup>Available online at: <http://www.ruf.rice.edu/~pbrace/statecourt/>. These data extend backwards through 1995, but are not available for previous years. Given this limitation, I cannot construct this measure to be lagged before the 1995 dependent variable to assuage endogeneity concerns. However, given high correlations between the 1995 *State Court Index* and its 1996 and 1997 values, I am reasonably confident that a lag to 1994 would not add much variation to this independent variable.

<sup>13</sup>Alternatively, I have also created an index of *State Court Activism* including the measure used here combined with The PAJID (Brace, Langer and Hall, 2000) supreme court ideology measure. Although policy-preferential and jurisprudential ideology are not synonymous, since the PAJID measure takes into account legislative preferences and not executive preferences, I use it as a proxy for the extent to which state supreme courts are willing to actively address (executive) governmental action. This strategy addresses the possibly selection issues involved with discretionary dockets. That the results reported below are substantively identical whether using this complex index or the simpler measure of overturning agency action lends confidence that any selection issues do not significantly bias the results.

capture policy conflict rudimentarily with indicator variables for party control of institutions. *Unified Legislature* takes a value of 1 when a state governor's party controls neither legislative chamber. Similarly, the *Divided Legislature* variable takes the value of 1 when the governor's party controls one of the legislative chambers. Therefore, completely unified government is indicated when both of these variables take the value of 0. Lacking better measures of state legislative and agency ideology, these variables are meant to capture the extent to which we can consider legislatures and executive agencies ideological allies or foes. Given the crudeness of this measure and that it consumes scarce degrees of freedom, I also measure the percentage of legislative seats held by the governor's party and use this *Governor's Copartisans* variable to alternatively measure policy conflict. Also following the Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002) convention, I operationalize legislative capacity as the 1995 amount of legislative *Compensation*—the annual salary plus per diem expenses for lower house members. Since it is reasonable to expect that the effects of capacity may diminish over its range, I performed all of the analyses with an untransformed *Compensation* term and a logged one. The results are substantively similar, so I present coefficients for the the untransformed compensation variable in the analyses below. I subsequently add important information regarding state legislative capacity by including a *Legislative Professionalism* variable in lieu of *Compensation* (Squire, 1992, 2007).<sup>14</sup>

As in the previous research, I model the potential interdependent effects of these important theoretical variables with multiplicative interaction terms. In particular, *Compensation* [*Legislative Professionalism*] is interacted with both *Unified Legislature* and *Divided Legislature* [alternatively, just with *Governor's Copartisans*] to capture the extent to which policy conflict may only become important when legislative capacity is sufficiently high. Previous research has also identified the legislative veto as an important nonstatutory factor that influences *Statutory Control*. Therefore, I include an interaction between a *Legislative Veto* indicator variable and *Unified Legislature* [*Governor's Copartisans*], with the idea that a unified legislature can wield this tool against a bureaucracy controlled by an opposition party governor, thereby lessening the need to control

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<sup>14</sup>This index is comprised of measures of legislator pay, session length, and staff resources of state legislatures.

policy ex ante.

Including institutional interaction terms in cross-sectional studies at the state level eats up precious degrees of freedom. Therefore, I include only three true control variables in the models below. In the Huber and Shipan empirical work on statutory discretion, per capita *Medicaid Expenditures* is the only consistently important control variable. Inclusion of this variable should isolate statutory discretion from policy change by controlling for the size of a state's Medicaid program.<sup>15</sup> I have estimated models using all of the control variables found in Huber, Shipan and Pfahler (2001), but these results are never substantively different from the more parsimonious models presented here. Due to their unique political history and the possibility that divided government means something different in southern states than it does in northern ones (Erikson, Wright and McIver, 1993), I include an indicator for the *South*. Finally, I include an indicator for *California* to control for that state's voluminous legislation, which has nearly three times more added words than the next largest amount.

I have good theoretical reason to expect that the effect of the primary independent variable, *Percent Overturned Agency Decisions*, is nonlinearly related to the total number of new words added to state Medicaid policy. The theory I have explicated predicts that this relationship is discontinuous (i.e., there should be no effect for very low and very high values of the variable, and a positive effect for intermediate values), but due to random error and other unmodeled determinants of *Statutory Control*, including the likely latent variation in the points of discontinuity across the states, this is unlikely to bear out perfectly. Therefore, it may be more reasonable to test whether there is at least a parabolic relationship between the variables. Among others, Keele (2008) warns against assuming strictly linear specifications when we suspect that the true data-generating process implies a nonlinear relationship. As an alternative, in the next section I use nonparametric techniques to diagnose the expected nonlinearity from the data and semiparametric ones to model

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<sup>15</sup>Concerned that medicaid expenditures might, in part, be determined by the activism of the courts in overturning agency action, I have estimated all models reported below while excluding *Medicaid Expenditures*. Doing so only serves to more strongly confirm the theoretical expectation developed above, so to maintain comparability with previous work (Huber, Shipan and Pfahler, 2001; Huber and Shipan, 2002), I present those models which include *Medicaid Expenditures*.

the appropriate functional form.

## Results

Figures 1 and 2 examine the functional form of the bivariate relationship between *Percent Overturned Agency Decisions* and *Statutory Control*. Here, I use local weighted polynomial regression (*lowess*) to get a sense of the relationship between the two variables. Since local regression is nonparametric, there do not exist global summary parameters that allow us to assess the relationship with a single number or coefficient. Instead, we can use the plot in Figure 1 to get a sense of the relationship in the full data.<sup>16</sup> Visual inspection suggests that *Statutory Control* increases only slightly with *Percent Overturned Agency Decisions* until it reaches a threshold near 0.2 on the x-axis. The dependent variable then rises logarithmically until *State Court Index* hits another threshold at 0.35, at which point, *Statutory Control* decreases until it levels off at 1,000 added words. This plot displays strong evidence of nonlinearity, at least in the bivariate relationship. Figure 2 displays the *lowess* estimates for different levels of policy conflict. The figure on the left plots the clearly nonlinear relationship for the data from states with divided government in 1995.<sup>17</sup> Similarly, the figure on the right confirms that the same pattern holds under non-divided government. In either of these figures, we see that the states where supreme courts overturn agency action to a moderate degree are the ones that limit discretion most severely.

**Figure 1 goes here.**

**Figure 2 goes here.**

Local smoothing techniques such as local regression and spline smoothing are useful for diagnoses of nonlinearity, but since they can only summarize bivariate relationships, they are not

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<sup>16</sup>As there is no way to control for outliers in a bivariate framework, I omit California from the *lowess* results.

<sup>17</sup>Divided government is considered to be when a state governor's party controls neither legislative chamber, i.e., when *Unified Legislature*= 1 or when *Divided Legislature*= 1. The non-divided government plot includes states where *Unified Legislature*= 0 and *Divided Legislature*= 0.

very useful for modeling social science data (Keele, 2008, p. 109). Fortunately, these techniques can be approximated in the standard parametric regression framework. Below, I estimate standard parametric models of the determinants of *Statutory Control* and compare them to models that also include quadratic transformations of *Percent Overturned Agency Decisions*, as well as to a Generalized Additive Model (GAM) which estimates a smoothed functional form for *Percent Overturned Agency Decisions* while simultaneously estimating the remaining variables parametrically. By taking a diversity of approaches, I can more confidently confirm the extent of nonlinearity in the primary independent variable, while simultaneously allowing for statistical falsification.

Model 1, presented in the first column of Table 2, generates results for a basic OLS model of the determinants of *Statutory Control*.<sup>18</sup> Here, I sought to replicate the results found in Huber, Shipan and Pfahler's (2001) Model 2 (Table 2, p. 343). The negative coefficient on the *Unified Legislature* and the positive and significant coefficient on *Unified Legislature* x *Compensation* indicates that statutory control increases *only* for sufficiently compensated legislatures. These results comport with a story about the interaction of policy conflict and legislative capacity driving efforts to control bureaucratic behavior ex ante. Nevertheless, this set of results does not offer strong support for their expectation that there is a substitutive relationship between ex ante and ex post mechanisms of legislative control. The coefficient for *Legislative Veto* x *Unified Legislature* is in the expected (by Huber and Shipan's (2002) theory) direction, but lacks significance against a two-tailed hypothesis test.<sup>19</sup> Model 2 presents results from essentially the same specification as in Model 1. Here, instead of excluding the constitutive terms (*Compensation* and *Legislative Veto*) of the included multiplicative interactions, I include them, per convention (e.g., Brambor, Clark and Golder, 2006). These results show that, besides the clearly influential *California* indicator,

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<sup>18</sup>I also specified Poisson and negative binomial regression models, but the results were substantially substantively similar across parametric specifications. These maximum-likelihood estimators generally produced more statistical significance across coefficients, but ML count models can be inconsistent in samples as small as I have here (Cameron and Trivedi, 1998). Results are available upon request.

<sup>19</sup>Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002) emphasize that this meets one-tailed standards of significance and interpret it as evidence in support of their theory. They also find stronger support for this argument, but only in analyses that exclude southern states.

only *Medicaid Expenditures* even approaches a standard level of statistical significance. I present these results to argue that to the extent that previous literature has provided support for the linear substitutive relationship between ex ante and ex post methods of control, this support has not been overwhelmingly strong.

**Table 2 goes here.**

Again, the consequential difference between my theory and theories of substitution effects is that, here, legislatures limit discretion to complement their strategic oversight activity, not to substitute for it. I have argued that the empirical implication of the complementarity perspective is that the likelihood of extra-legislative assistance in ex post oversight should have a curvilinear effect on the legislature's initial grant of statutory discretion. Before I directly assess this expectation, I will test whether their linear substitution story holds with regard to the novel courts variable I propose as indicating such extra legislative assistance. For Model 3, I introduce the *Percent Overturned Agency Decisions* variable into the Model 2 specification. A negative and significant coefficient on this variable would indicate support for the substitution argument. We see here, however, that this coefficient, as well as that for *Legislative Veto x Unified Legislature*, is not statistically different from zero. In fact, the only variables in Model 3 to influence *Statutory Control* are the controls for the voluminous legislation in *California* and the amount of a states's *Medicaid Expenditures*. There is, then, no empirical support for substitution theories in this model of the data.

Model 4 relaxes the assumption of a linear effect of *Percent Overturned Agency Decisions* on *Statutory Control*. Here, I include both the original variable meant to capture nonlegislative ex post oversight and a squared transformation of it. In order to test whether the curvilinear effect has the same functional form as demonstrated bivariately in Figures 1 and 2, we can examine the coefficients on these terms. The positive and significant coefficient on the untransformed term and the negative and significant coefficient on the squared term indicate that the effect of *Percent Overturned Agency Decisions* on *Statutory Control* rises initially and then tails off for

higher values. This is consistent with the theory of strategic complementarity I have explicated, but is wholly inconsistent with the substitution theory. If ex post oversight was to be relied on to the exclusion of ex ante statutory control, then the squared term would not be negative and significant (and the untransformed term from Model 3 would be positive and significant). Although this pattern refutes substitution as a mechanism, it does not unambiguously confirm the present theoretical expectations. The  $F$ -statistic reported in the bottom row of column 4 reflects results from a statistical test for whether the coefficients for *Percent Overturned Agency Decisions* and its squared transformation are jointly significant. That the  $p$ -value for this test statistic does not reach conventional levels of statistical significance temporarily precludes us from interpreting these results as support for the theory.<sup>20</sup>

Model 5, found in the leftmost column of Table 3, presents results for a specification analogous to that found in Model 4 but with the aforementioned alternative specifications of legislative capacity and policy conflict. Here, instead of *Compensation* and the indicators for splits in party control of the executive and legislative branches, I operationalize these concepts with *Legislative Professionalism* and *Governor's Copartisans*. First, we notice that the coefficient for the interaction of these two is negative and statistically significant. This, along with the positive and statistically significant constitutive term on *Legislative Professionalism*, means that (since higher values for *Governor's Copartisans* indicates less policy conflict between the branch) legislative capacity positively determines the verbosity of legislation when there is policy conflict. More importantly for assessing the theory presented in this paper, the *Percent Overturned Agency Decisions* and *Percent Overturned Agency Decisions*<sup>2</sup> variables remain as they were in Model 4

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<sup>20</sup>In fact, the initial individual significance of the two coefficients might simply be an artifact of their joint distribution, rather than support for the nonlinear hypothesis. The joint insignificance of the two variables in Table 2 does not dispel this suspicion. Table 3 (below) eventually shows support for the joint significance of the two *Percent Overturned Agency Decisions* coefficients and thus provides evidence that this significance is not driven by the specific joint distribution with the dependent variable. Yet, it does so only under alternative operationalizations of legislative capacity and policy conflict, highlighting that parametric support for the nonlinear hypothesis is sensitive to model specification. Defending the robustness of the results, I would argue that the alternative specifications are more properly operationalized (than the models in Table 2) with respect to legislative capacity and policy conflict and that the semiparametric results presented below are robust to changes in model specification.

and the joint  $F$ -test (bottom rows of Table 3) indicates that they are jointly significantly different from zero in this specification. This column provides strong statistical support confirming that *Percent Overturned Agency Decisions* is curvilinearly related to *Statutory Discretion*. Model 6 presents results from a specification excluding the *Medicaid Expenditures* control. It is possible that this level of spending is in part determined by the behavior of the courts striking down or allowing particularly expensive Medicaid spending. Therefore, the concern is whether it is endogenous to the primary independent variable of interest, rather than indicative of unobserved state-level variation in policy environments. The middle column of Table 3 shows that the results are robust to the omission of *Medicaid Expenditures* and we actually gain more confidence in the level of statistical significance from this specification.

**Table 3 goes here.**

In addition to these parametric specifications, the final column (Model 7) of Table 3 presents results from a semiparametric Generalized Additive Model (GAM) of *Statutory Control*. This model estimates a smoothed functional form for *Percent Overturned Agency Decisions* without assuming as much structure as the parametric models with quadratic terms did. The GAM framework is flexible enough to allow parametric estimation of linear terms simultaneously with the nonparametric estimation of smoothing splines for nonlinear terms.<sup>21</sup> Keele (2008) advises for the stepwise estimation of each term as nonparametric followed by likelihood ratio tests of model fit to find the correct specification. Such an exercise is largely atheoretical and the theory presented herein identifies *Percent Overturned Agency Decisions* as a likely nonlinear term.<sup>22</sup> The rightmost column (denoted Model 7) of Table 3 shows the results of an identical specification to Model 6 with a nonparametric estimation of *Percent Overturned Agency Decisions*. The first

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<sup>21</sup>Keele (2008)(pp. 140-141) describes some estimation procedures for GAMs and notes that different software may estimate these models differently. I have used the `mgcv` package (Wood, 2006) in `R` to estimate the semiparametric model below. This particular specification uses generalized cross-validation (GCV) scores for automated smoothing of the nonparametric term, but the substantive results are robust to manual selection of spline knots as well.

<sup>22</sup>That said, I did test the linearity of the other covariates and the theoretically preferred model (Model 7) is also the best fit to the data.

important thing to notice is that this model fits the data significantly better than the parametric model (likelihood-ratio test  $p < .09$ ). Given what we know about the nonlinearity in *Percent Overturned Agency Decisions* from Figures 1 and 2, this is not surprising. In addition to providing an overall better fit, the GAM leads to reinterpretations of the effects of the parametric terms. In particular, and despite the overall improvement in model fit, all previously statistically significant effects (except for the *California* indicator) disappear in the the GAM specification.

**Figure 3 goes here.**

Since the GAM estimates a spline for the effect of *Percent Overturned Agency Decisions* on *Statutory Control*, we cannot summarize the relationship with a coefficient and standard error. The “NP spline” in the table denotes that there is no coefficient, yet a partial  $F$ -test against a model which omits the nonparametric term indicates that the effect is statistically significant (at the .10 level). Instead, the standard way to substantively interpret nonparametric terms in GAMs is to inspect a plot of their effects across a range of values. Figure 3 plots the nonparametric estimates for the effect of *Percent Overturned Agency Decisions* on *Statutory Control*, along with a bias-corrected 95% confidence band. These effects, obtained after controlling for other potential determinants of the dependent variable, mirror the relationship displayed in Figures 1 and 2. In particular, we see that the effect of the *Percent Overturned Decisions* significantly increases *Statutory Control* in the range from around 20 to 35%, until it begins reverting to a zero, or insignificantly (given the coverage of 0 by the confidence bands) negative effect. The GAM results further confirm the nonlinear predictions from the theory and suggested by the bivararate nonparametric exploration of Figures 1 and 2 and supported by the fully parametric models in Tables 2 and 3. All told, the diversity of approaches taken here increase the confidence with which we can support the perspective introduced by the theory.

## Conclusion

Many citizens assume that legislatures—be they Congress or the state legislatures—simply get what they want when they agree on legislative solutions to policy problems. As elected representative branches, this conventional account is as democratically attractive as it is simplistic and wrong. Importantly, the assumption of legislative dominance glosses over many inter-governmental and inter-branch complexities of the policymaking process. As a salient recent example, the Patient protection and Affordable Care Act of 2010 seeks to expand Medicaid to include previously excluded potential recipients of government-provided health care. Rather visibly, Congress has successfully weathered constitutional challenges to this act, most notably to the “individual mandate” to purchase health insurance, but also regarding its expansion of Medicaid. As Medicaid is a joint federal-state program, some states promise to fight against this expansion and the Supreme Court has limited Congress’s ability to punish those states that fail to implement the program as harshly as it might have wanted. Governors and legislatures in these recalcitrant states may wish to pursue alternative policies and can attempt to direct their state bureaucracies to ignore the wishes of President Obama and congressional Democrats. On the other hand, there are bureaucratic actors even within these states (departments of health and human services, for example) that may wish to expand Medicaid in defiance of the will of their state political principals. The policymaking process does not end when Congress passes landmark legislation; in fact, it can be seen to only just begin. As the example of Medicaid expansion involves legislatures, the courts, and bureaucratic agencies at both the national and state levels, so does the general policymaking environment in separation of powers systems like that found in the United States.

In this paper, I have attempted to elucidate some of the ways in which institutional variation can condition the strategies that legislatures might rationally pursue to achieve political and policy goals. These strategies, including writing more or less specific legislation and conducting oversight hearings, have real consequences on policy outcomes, so understanding how and why they are chosen is a critically important endeavor. This research has contributed to the study of

state-level executive-legislative relations in a number of ways. First, it approaches questions of delegation and oversight with a general theoretical framework that generates novel predictions about the relationship between nonstatutory controls and the incentive to write statutory controls into legislation. I have presented a two-player game between a unicameral legislature and an agency, where the courts serve as an exogenous constraint on strategic behavior. In the spirit of Krause and Woods's (2013) call to take seriously differences in institutional capacity, I have accounted for legislative capacity and judicial capacity. In addition, I have simultaneously modeled a legislature's choice between ex ante and ex post strategies of policy control, allowing me to extend and modify the influential Huber and Shipan (2002) model of delegation. In particular, I show that these two canonical types of policy control need not be mutually exclusive and argue that we should not necessarily expect them to substitute for one another. Taken together, these theoretical contributions generate the novel empirical expectations described in the paper.

In addition, I test the implications of this analytical model on statutory discretion across the U.S. states in the Medicaid policy area in 1995-1996. I replicate and modify the empirical analyses found in Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002) by creating a novel measure of state court activism as a nonstatutory control across the states. I demonstrate that this variable is, as expected, nonlinearly related to statutory control and I appropriately model the empirical relationship nonparametrically and using standard econometric models. When we analyze the same data used in these studies in light of the predictions generated from the model presented here, I contend that we see strong evidence of the proposed nonlinearity of effects. If we were to—without the theoretical guidance animating this research—test these predictions using a strictly linear framework, we would instead find some limited support for the linear substitutive predictions of Huber, Shipan and Pfahler (2001) and Huber and Shipan (2002).

While not every study of legislative policy control (e.g., MacDonald and Franko, 2007) uses the length of legislative statutes as a measure of control, it would be a difficult, yet potentially rewarding extension to test this theory using alternative data. For example, Epstein and O'Halloran (1999) measure policymaking autonomy by identifying, through *Congressional Quarterly* reports,

the extent to which statutes both delegate policy authority and set procedural limits on that authority. While this would no doubt be an innovative complement to the data on statutory discretion used in Huber and Shipan (2002) and in the current paper, it would be exceedingly difficult to collect at the state level, since there is no uniform state-level equivalent to *Congressional Quarterly*. Besides, Huber and Shipan (2002, pp. 56-72) go through pains to highlight the importance of prescriptive language, and not procedural language, in these state Medicaid statutes.<sup>23</sup> Had they not, there would be much more reason to suspect that statute length may not be a good proxy for delegation of discretion. Therefore, to the extent that future research delves into new policy areas, we must be keen to diagnose the extent to which legislation in those areas rely on relatively brief, yet highly restrictive, procedural language.

Among many directions for future research, a few seem especially important. As I have assessed the theoretical model's predictions regarding ex ante discretion in this paper, I have previously shown support for the ex post oversight hypotheses at the congressional level (McGrath, 2013, *forthcoming*). However, a more stringent test of the general theory would require one to collect data on ex ante discretion in a policy area along with ex post monitoring activities in the same area and assess the theoretical expectations simultaneously. Unfortunately, this more stringent test is practically impossible given the current dearth of reliable information on state legislative oversight. Finally, while explaining executive-legislative relations and legislative strategies of control are important topics in themselves, future work should integrate this research with policy studies to assess whether control has discernible effects on policy outcomes (Krause and Woods, 2013). While I show that legislatures act while considering the likely behavior of state executive and judicial branches, it remains to be seen if institutions that facilitate this strategic behavior (e.g., legislative professionalism) have positive effects on policy outcomes in the states.

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<sup>23</sup>As well as they show that "...longer legislation does not consist of mostly general language ... [and] that longer legislation does not contain proportionally more procedural language" (Huber and Shipan, 2002, p.74).

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Table 1: Descriptive Statistics

	N	Mean	SD	Min	Max
Total Added Words, 1995-96	48	24,683	44,344	216	277,495
% Overturned Agency Decisions	48	34.1	12.2	6.78	57.7
Compensation	48	22,795	14,785	100	57,500
Legislative Professionalism	48	0.18	0.12	0.03	0.57
Unified Leg.	48	0.31	0.47	0	1
Divided Leg.	48	0.19	0.39	0	1
Governor's Copartisans	48	0.57	0.16	0.21	0.91
Legislative Veto	48	0.60	0.49	0	1
Medicaid Expenditures	48	0.57	0.18	0.34	1.33

Table 2: OLS Models of State Statutory Control

	(1)	(2)	(3)	(4)
% Overturned Agency Decisions	—	—	131 (285)	1,680* (939)
% Overturned Agency Decisions <sup>2</sup>	—	—	—	-23.26* (12.98)
Unified Leg.	-23,101** (10,249)	-16,996 (17,050)	-18,704 (18,717)	-13,710 (17,267)
Compensation	—	0.361 (0.346)	0.344 (0.356)	0.327 (0.388)
Unified Leg. x Compensation	1.634** (0.784)	1.279 (0.875)	1.368 (0.854)	1.093 (0.806)
Divided Leg.	-16,112 (9,858)	-8,045 (12,047)	-8,732 (12,606)	-17,558 (12,626)
Divided Leg. x Compensation	0.467 (0.294)	0.078 (0.454)	0.113 (0.464)	0.418 (0.452)
Legislative Veto	—	-3,089 (12,138)	-2,242 (11,319)	-5,004 (12,026)
Legislative Veto x Unified Leg.	-13,972 (11,256)	-10,836 (17,092)	-11,828 (15,596)	-5,197 (15,441)
Medicaid Expenditures	35,653** (17,333)	35,002* (17,564)	36,672** (16,650)	20,579 (17,040)
South	-1,962 (5,277)	-1,363 (5,181)	-1,076 (5,574)	2,281 (5,100)
California	249,018*** (8,782)	248,758*** (9,002)	248,383*** (9,562)	235,530*** (11,159)
Constant	732 (11,920)	-5,270 (19,586)	-10,935 (16,781)	-21,076 (15,976)
Observations	48	48	48	48
Adjusted $R^2$	0.722	0.718	0.712	0.701
$F$ ( $\beta_1 = \beta_2 = 0$ )	—	—	—	1.64
$P > F$				.21

Robust standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 3: OLS and GAM Models of State Statutory Control

Independent Variables	(5)	(6)	(7)
% Overturned Agency Decisions	2,149** (853)	2,263** (795)	NP spline*
% Overturned Agency Decisions <sup>2</sup>	-30.05** (11.98)	-31.81** (11.6)	—
Governor's Copartisans	58,544 (39,708)	65,882** (32,688)	64,979 (53,910)
Legislative Professionalism	334,875** 158,916	367,779** (109,433)	362,329 (212,115)
Governor's Copartisans x Leg. Prof.	-339,519* (215,570)	-430,579** (170,248)	-420,678 (326,520)
Legislative Veto	9,691 (11,354)	11,305 (10,471)	11,116 (15,024)
Legislative Veto x Governor's Copartisans	-79,789 (85,116)	-91,171 (70,672)	46,484 (49,196)
Medicaid Expenditures	6,865 (21,375)	—	—
South	-3,728 (5,524)	-3,981 (5,491)	-6,359 (10,005)
California	180,920*** (36,107)	174,009*** (23, 141)	201,742*** (32,348)
Constant	-69,135 (27,499)	-72,428 (25,304)	-36,493 (35,300)
Observations	48	48	48
Adjusted $R^2$	0.713	0.721	0.729
$F$ ( $\beta_1 = \beta_2 = 0$ )	3.23	4.06	—
$P > F$	.05	.025	—
LR test against previous model ( $p$ -value)			.09

Robust standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

NP spline: Model (7) is a Generalized Additive Model (GAM) of the determinants of state statutory control. % *Overturned Agency Decisions* estimated as a nonparametric spline, while all other included covariates are linear parametric estimates. The  $p$ -value for significance of this effect is .093.

Figure 1: Lowess Smoother, No California

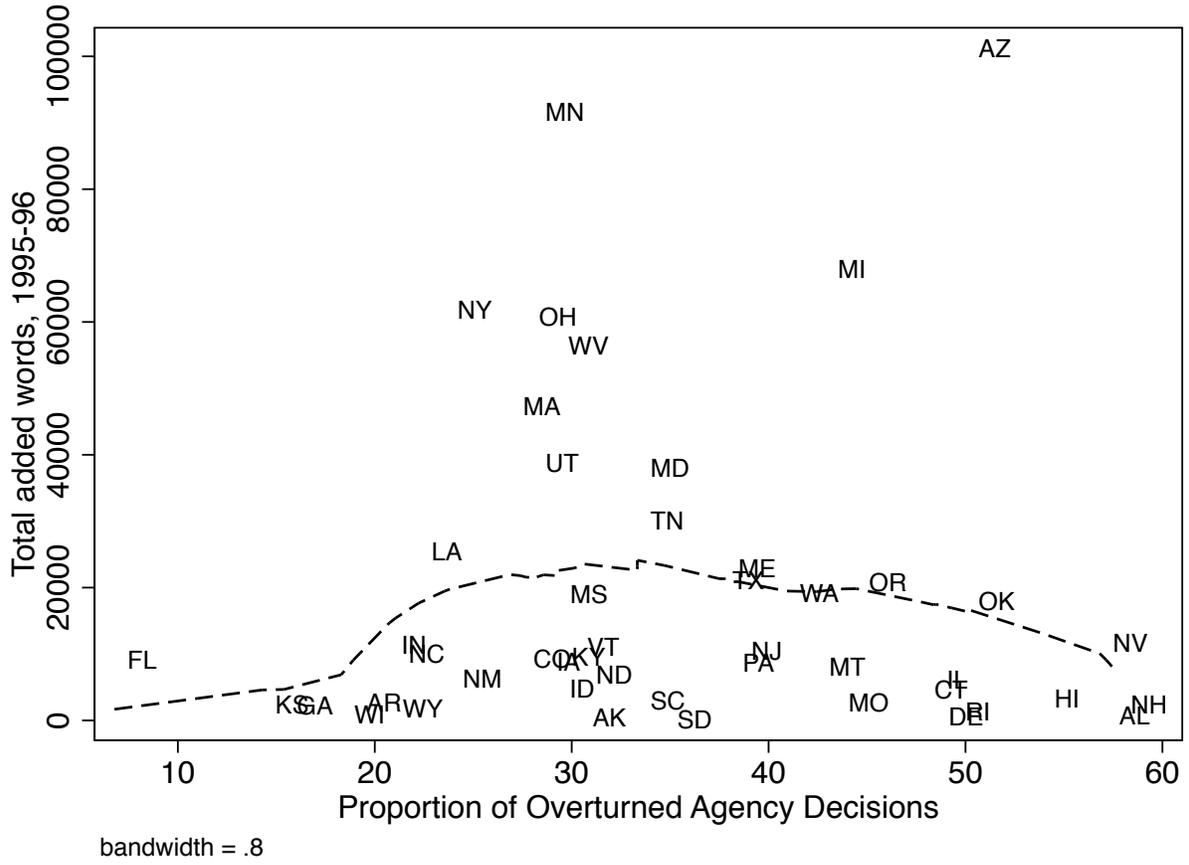


Figure 2: Lowess Smoother, No California

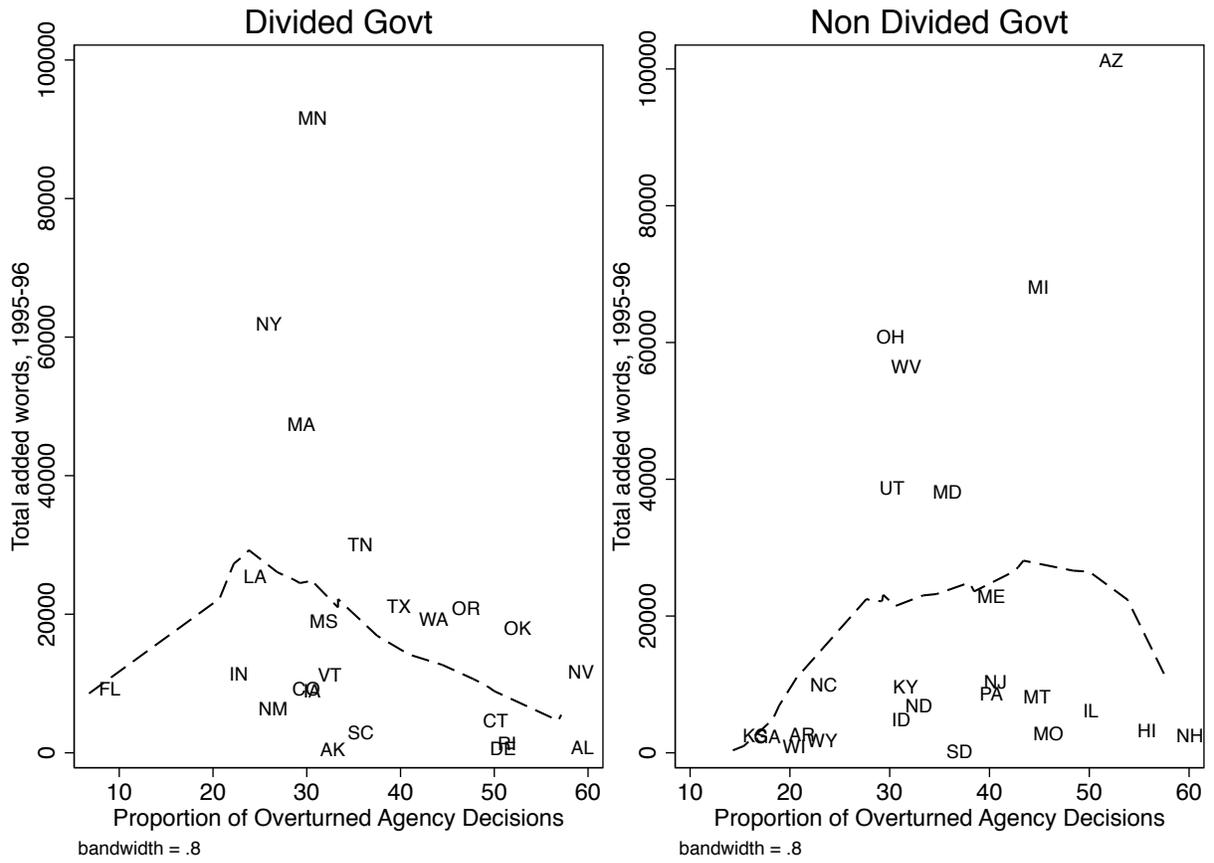
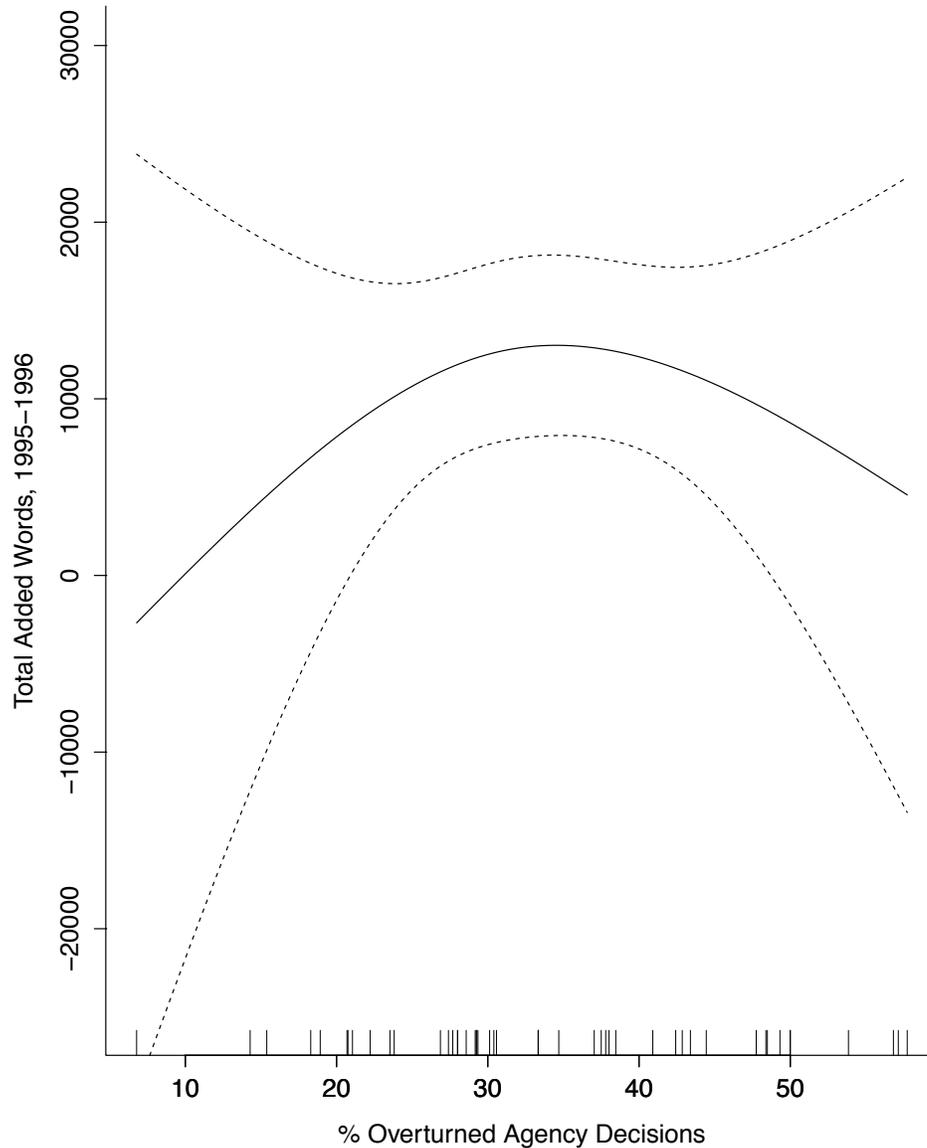


Figure 3: Nonparametric Estimates for Semiparametric GAM of State Statutory Control



Note: Figure generated after estimation of Model (7) from Table 3. This figure gives the nonparametric effects of *% Overturned Agency Decisions* on the dependent variable, *Total Added Words, 1995-1996*. The “rug” gives the points of observation on the independent variable. Automatic smoothing routines (using generalized cross-validation) generate this effect, but nearly identical results can be found when specifying cubic spline smoothing with 3 or 4 degrees of freedom. The dotted lines give bias-corrected 95% confidence bands.