Legislative Capacity and Credit Risk

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Abstract

Legislatures differ in their institutional capacity to draft and enact policy. While strong legislatures can increase the congruence of policy outcomes to the electorate's preferences, they can also inject uncertainty into markets with their ability to alter the political economic landscape. We argue that this uncertainty will manifest in a state's ability to borrow and hypothesize a negative relationship between legislative capacity and credit-worthiness. Using ratings of general obligation bonds issued by the American states over nearly two decades and data on the institutional capacity of state legislative assemblies, we find support for the claim that having a legislature that is better equipped to affect policy change increases credit risk evaluations. The results we present broaden our understanding of the importance of legislative institutions and the determinants of credit risk, and also bring evidence to bear on competing expectations for policymaking behavior under representative and responsible models of governance.

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Legislatures vary widely in their ability to translate policy preferences into policy outcomes. Limited by informational resources, time in session, or legislators' incentives to invest in the policymaking process, some legislatures simply do not possess the institutional capacity to effectively bring policy in line with public sentiment. In such contexts, it is difficult to alter policy in response to changing needs or preferences and we are more likely to observe "democratic deficits" (Lax and Phillips 2012). Where legislative capacity is high, however, this translation of preferences into outcomes can be more efficient and legislatures are more likely to produce policies preferred by voters (Maestas 2000). Normatively, the increased democratic responsiveness induced through increased legislative capacity is an invaluable public good. However, the broader political economic implications of legislative capacity are, at present, not well understood.

There is no shortage of research on legislative capacity (or, professionalism), particularly in the context of the American states. For example, previous studies have explored how capacity influences the composition of assemblies in terms of both diversity and partisanship (Squire 1992; Fiorina 1994, respectively), coattail effects and reelection rates (Berry, Berkman and Schneiderman 2000; Carey, Niemi and Powell 2000), patterns of internal organization (Richman 2008), and legislatures' public approval (Kelleher and Wolak 2007; Richardson et al. 2012). But only a very small portion of this research has examined the effects of capacity on policy outcomes. Moreover, these outcome-oriented studies have tended to focus on particular policy issues such as welfare policy (Carmines 1974; LeLoup 1978), anti-smoking policy (Shipan and Volden 2006), or energy regulation (Ka and Teske 2002). When studies have taken a more general approach to policy, their primary concern has been the congruence of citizen preferences and legislative outcomes and there is agreement that congruence increases with capacity (Lax and Phillips 2012; Maestas 2000).[1] Our contribution here is a broader exploration of the political economic implications of the capacity to rapidly and comprehensively change policy that reveals a real monetary cost to democratic responsiveness.

We argue that the responsiveness facilitated by a high capacity legislature, while providing many normative benefits, is not an unfettered public good. High capacity legislatures are more

[1] Interested readers are encouraged to consult Maestas (2000) for a succinct summary of this literature.

likely to alter the political economic landscape by, for example, altering rates of taxation or spending, reshaping regulatory environments, or otherwise channeling preferences into policy outcomes, and this capacity can, in turn, increase credit risk. That is, high capacity legislatures have the ability to rapidly and comprehensively alter policy in response to shifts in public sentiment, while low capacity legislatures lack the ability to deliver the same level of responsiveness. Thus, high capacity legislatures, in expectation, produce more variable policy environments while low capacity legislatures, in expectation, produce more stable environments. This policy uncertainty is of critical importance to state debt markets, where increased uncertainty decreases a lender's ability to forecast the likelihood a state will be willing and able to maintain its debt obligations. We therefore predict that higher levels of legislative capacity lead to higher risk evaluations on state general obligation bonds.

Our theoretical approach presumes that policymakers are responsive to constituent preferences because they are election seekers — a *representative* model of policymaking. In the representative model voters prefer candidates who pursue policy that coincides with their preferences, and politicians respond by attempting to bring policy in line with public opinion. We also consider how assuming a *responsible* model of policymaking — where voter's hold policymakers accountable based on the quality of outcomes, rather than the congruence of policy to preferences — would change our empirical expectations. In short, a responsible model of governance would predict the opposite of our central hypothesis: that legislative capacity would decrease credit risk.

To test our claim, we examine the relationship between legislative capacity and risk evaluations of U.S. state general obligation bonds. We posit that higher levels of state legislative professionalism capture the legislative capacity discussed above and are associated with higher credit risk assessments (i.e., lower credit ratings), making it more difficult for state governments to borrow and increasing the debt burden on taxpayers.[2] The U.S. states provide an ideal testing environment for this argument. Each of the fifty states (forty of which issue general obligation bonds) shares a core institutional context: "presidential" systems with nearly identical electoral rules, party systems, and

[2]Professionalism has become the industry standard proxy for legislative capacity; see Lax and Phillips (2012) Boehmke and Shipan (2015) for recent examples.

competency distributions. Further, all fifty states share a (relatively) common cultural context and have highly correlated economies. For all of these similarities, however, there is significant variation in the legislative capacity (or, professionalism) of America's state legislatures.

The results of the empirical analysis reveal strong support for the central argument that state legislative capacity is significantly associated with lower credit ratings and makes several substantively interesting and normatively significant contributions. First, because we focus on the impact of outcomes, as opposed to, say, legislative efficiency in the form of bill initiation (e.g., Hedlund and Freeman 1981; Squire 1998), we provide evidence that legislative capacity increases the rate and depth of policy change in a manner unique to the literature.[3] Second, the findings suggest that the political economic implications of institutional choice are more far-reaching than previously expected as the substantive effect of capacity on credit risk is even greater than explicit revenue limits, spending limits, and debt restrictions. Third, our findings suggest that real world policymaking follows a representative rather than a responsible government model of electoral accountability. This is of particular interest because, fourth, the empirical results make it clear that the public goods provided by high capacity legislatures come at a cost that is real and long-lasting. High capacity legislatures are instrumental to the quality of democratically desirable outcomes (i.e. Lax and Phillips 2012), however, we provide evidence that that capacity carries a significant monetary cost far in excess of the direct costs associated with increasing professionalism, such as providing competitive legislative salaries and high quality legislative staff.

Legislative capacity, policy responsiveness, and credit risk

Our central focus is on a legislature's capacity to efficiently translate policy preferences into policy outcomes and we argue that this capacity is a function of professionalization. The institutional context that characterizes professionalized legislatures leads to greater legislative capacity through several channels. First, there are institutions that aid in information gathering such as a larger and more able support staff (allocated to committees, caucuses, individual legislators, etc.) or committee

[3] At the very least, we provide evidence that legislative capacity increases the rate and depth of policy change expected by lending markets.

systems with strong property rights that are procedurally autonomous. Second, there are institutions that motivate legislators to personally invest in information and expertise such as higher salaries, more generous benefits, the potential for upward mobility inside the legislature, or, relatedly, strong norms for committee assignments. Finally, there are institutions that support the policymaking process more generally, such as longer sessions which increase the amount of time the plenary (or a committee) has to propose, scrutinize, and pass policy while simultaneously limiting the amount of time legislators have to pursue outside occupations. All of these institutional parameters improve legislators' abilities to pursue and produce policies they believe will benefit their constituents and/or electoral fortunes.[4]

The majority of previous research on professionalization has focused on issues such as its impact on recruitment (Fiorina 1994), electoral results (Berry, Berkman and Schneiderman 2000), or membership composition more generally (Squire 1992; Sanbonmatsu 2002; Meinke and Hasecke 2003). Our focus here, however, is policymaking, which Huber, Shipan and Pfahler (2001) argue is shaped by professionalization, noting:

> "the expertise of legislators may also be a function of the legislative institutions themselves. If the legislature is highly institutionalized, with a large number of specialized committees and support staff, then it may be easier for the legislature to draft detailed legislation" (p. 334).

We build on, and contribute to, this research connecting legislative professionalism to policymaking capacity and argue that it ultimately affects the broader political economic landscape by considering the interplay of the electorate's preferences with these institutional structures.

Beginning with Erikson, Wright and McIver (1993), scholars have shown that state-level public opinion influences state-level policy choice — that state governments are *responsive* to constituent preferences. More recently, Maestas (2000, 2003) and Lax and Phillips (2012) provide compelling evidence that professional legislatures (i.e., high capacity legislatures) are more likely

[4]Importantly, all of these parameters tend to covary. For more information, see Squire and Hamm (2005), the canonical work on the institutionalization of state legislatures.

to change policy to increase congruence with public opinion. Professionalized legislatures have greater resources — e.g., time, staff, information, etc. — at their disposal to track public opinion, develop policy proposals that satisfy constituent preferences, initiate and scrutinize these proposals, and ultimately enact policy change in response to shifting preferences in the electorate. Unprofessional legislatures, by contrast, are simply ill-equipped to efficiently engage in these behaviors and therefore unable to rapidly pursue policy change to please voters or satisfy public sentiment. This is key to understanding how high levels of legislative capacity can inject uncertainty into the political economic environment. Even if we believe that the preferences of legislators or political parties are quite stable over time, voters are notoriously fickle and shocks to public opinion are common;[5] the impact of these changes will vary systematically according to the institutional context.

Imagine, for example, a shock to public opinion that uniformly shifts preferences on energy regulation in all states. This shock is much more likely to have deep policy effects in Michigan and New York than in Ohio and New Jersey — despite the socio-economic similarities between Michigan and Ohio or New York and New Jersey — because the institutional context of the Michigan and New York legislatures make those states much more able to alter the regulatory environment in response to shifting preferences.[6] Because public opinion shocks are unpredictable, high capacity legislatures make forecasting future policy environments more difficult as the efficient translation of policy preferences to policy outcomes necessitates the incorporation of such shocks into the forecast. Conversely, unprofessional "citizen legislatures," or those legislatures with low capacity, lead to a more stable political economic environment and increase the certainty of forecasts precisely because they are ill-equipped to respond to these shocks. Legislatures like the New Hampshire General Court, which meets for only 45 days over a two-year session, pays its members $100 per year, and has a representative to staff ratio greater than 3 to 1, simply lack the ability to efficiently respond to shifting preferences. In other words, unprofessional legislatures create an extraordinarily sticky status quo by denying their members the resources necessary to effectively legislate and this, in turn, makes for stable, predictable political economic environments.

[5] See Page, Shapiro and Dempsey (1987) and Lax and Phillips (2009) for classic and contemporary examples.

[6] See Squire (2007) for detailed descriptive analysis of professionalism across states and over time.

This brings us to the connection between legislative capacity and evaluations of state credit risk. States generally borrow for one of two reasons: first, they may be forced to borrow because obligatory or preferred spending levels outpace available (or politically viable) revenues; second, they may wish to borrow to finance some type of investment (infrastructure, education, etc.) that demands more funds than the state has on hand. General obligation bonds are backed by the full faith and credit of the state and are nearly always repaid exclusively through tax revenue. As such, general obligation bonds issued by U.S. states provide a unique opportunity to study market responses to policy, or more precisely, policy change. As Lewis (2012) succinctly notes, "bond ratings provide information about risk and uncertainty that result from the interaction between economic and political factors" (p. 309).[7] Because a state's ability to repay its debts is a direct function of its fiscal policy, its overall economic health, and its willingness to maintain its debts,[8] the policy environment created by a state's government should be a factor bond markets weigh heavily when they evaluate the risk of lending to that state.

Existing scholarship shows that a state's fiscal tastes and behaviors have a robust impact on credit risk (Lowry and Alt 2001; Andersen, Lassen and Nielsen 2014), but a state's preferences for taxation and spending are only one part of the policy equation. Risk evaluations also include the constitutional context that helps determine the future supply of debt and the probability of honoring current debt (e.g., balanced budget rules, deficit carry-over limits), economic factors that contribute to a state's ability to honor debt obligations, the state's current level of outstanding debt, and a state's propensity to incur or repay debt (Poterba and Rueben 1999, 2001). More salient for present purposes, governments also impact economic well-being through their investments and regulatory choices and this well-being, in turn, determines the pool of potential revenues. Thus, while overall levels of taxation and spending are perhaps the most important choice a government makes, this choice is still but one piece of the broader policy environment that determines a state's credit risk.

[7] See also Depken and Lafountain (2006); Ingram, Brooks and Copeland (1983); Krueger and Walker (2008); Lowry and Alt (2001).

[8] Krueger and Walker note that "[w]hile actual default may be an option only in the extreme, governments can and do change their taxing and spending priorities in ways that can threaten the long-term viability of repayment" (2008, p. 263–264) and states commonly restructure their debt obligations.

Legislative capacity is an antecedent to these risk factors. Without the capacity to substantively alter existing fiscal policy or update the state regulatory environment, risk assessments can be made with the expectation that the policy environment will remain stable. In contrast, high capacity legislatures allow elected officials to rapidly and comprehensively respond to shifts in public opinion (or preferences within the legislature) and therefore lead to much more uncertain future policy environments. In a sense, low capacity legislatures provide a credible commitment to maintain status quo policies into the future since they 'tie the hands' of both present and future representatives by inhibiting the ability of current members to learn and respond to changing preferences in the electorate in their pursuit of reelection while also inhibiting the ability of future members to pursue their potentially different policy goals upon entering office.

This means that whether one subscribes to a standard representative government model of democracy — wherein legislators act in response to public opinion (Erikson, Wright and McIver 1993) — or a mandate model of democracy — wherein party preferences are fixed and policy change is realized through electorally facilitated changes to legislative coalitions (Powell 2000) — a low capacity legislature will always impede policy change. Both of these functions are salient as general obligation bond maturities range from 1-30 years (though maturities of less than 5 years are rare and ranges of 15-20 years are most common), which means that lending markets must consider both present and expected future contexts; and both are consistent with the finding that increased capacity leads to increased responsiveness. Further, both of these mechanisms are analogous to previous research on the impact of institutional constraints on fiscal policy and the ability to borrow. That is, a low capacity legislature should have a similar impact on a state's credit rating as, for example, constitutional restrictions on spending (Poterba and Rueben 1999, 2001) in that low capacity legislatures, like balanced budget amendments, reduce credit risk by curtailing legislators' ability to meddle with the political economic environment. Paraphrasing from above, a low capacity legislature is an institutional commitment to the status quo.

In summary, we argue that high capacity legislatures create more variable political economic environments by facilitating legislators' ability to respond to preference shocks or, more generally,

shifts in public sentiment. This variability creates market uncertainty which is manifest in lower ratings of state general obligation bonds. This yields the following prediction:

- **Capacity hypothesis.** All else equal, as legislative capacity increases, evaluations of general obligation bond risk increase.

The theoretical discussion, and resulting hypothesis, presented above presume a representative model of government where either legislators are responsive to the policy preferences of voters or voters choose legislators to enact certain policies. A second view of democratic accountability is the responsible government model in which voters elect representatives that they believe are competent and then evaluate them on the basis of observable outcomes, typically growth or unemployment (e.g. Duch and Stevenson 2008).[9] In this model, where voters are agnostic over (or unresponsive to) specific policy choices and concerned only with salient observable outcomes, legislative capacity should be an asset to high credit ratings as the incentives of legislators become aligned with the preferences of lending markets: stable, sustained growth, fiscal discipline, etc. After all, voters are nearly unanimous in their preferences over salient outcomes (low unemployment, low crime rates, good public health outcomes, high educational attainment, etc.), but are quite fractious in their preferences over how these goals are pursued. Thus, a responsible government environment divorces public opinion from the policymaking process and encourages technocratic, rather than ideological legislative behaviors, and high capacity legislatures are critical to technocratic governance. As such, to the extent that the data bear evidence for our hypothesis we provide support for the argument that policymaking in the American states follows a representative, rather than a responsible government model, or, at the very least, that lending markets believe repersentatives in the American states behave as predicted under the representative government model.

[9]The representative/responsible government typologies are sometimes referred to as delegate/trustee representation models in the American politics literature (e.g. Fox and Shotts 2009) or mandate/accountability models in the comparative politics literature (e.g. Przeworski and Stokes 1999).

Empirical assessment of legislative capacity and credit risk

We now test the *capacity hypothesis* by analyzing the relationship between legislative capacity and credit risk in the American states. We begin investigating the hypothesis with a simple examination of the raw data. Then, we analyze a fully specified model, present the main results, and note alternative specifications that interested readers may find in the appendix.

Data and preliminary analysis

Testing the *capacity hypothesis* requires data on state general obligation bond risk as well as data on state legislative capacity. For risk evaluations, we utilize ratings assigned to each state's general obligation bonds by the three major rating houses (Fitch, Moody's, and S&P) as recorded by the U.S. Census Bureau's Statistical Abstract of the States from 1995–2010 for all forty states issuing general obligation bonds.[10] Following Krueger and Walker (2008, 2010), we consider risk a latent construct — a stimulus to which bond ratings are a response.[11] Where the true risk of lending to state i in year t is θ_{it}, we can consider each rating y_{itb}, from each rating house b, an ordinal estimate of θ_{it}, conditioned on aspects of the rating house and some error. In the regression framework this is given by,

$$y_{itb} = \alpha_b + \theta_{it}\beta_b + \varepsilon_{itb},$$

where y_{itb} is observed and each rating house intercept α_b, rating house slope β_b, and rating house-state-year error term ε_{itb}, as well as the underlying stimulus θ_{it} are unobserved. This equation can be estimated as a hierarchical ordered probit model to recover estimates of θ_{it}, however, the number of parameters to estimate, problems resulting from each house not issuing ratings for every state in every year, and problems resulting from some potential rating values going unobserved over the

[10] Colorado, Idaho, Indiana, Iowa, Kansas, Kentucky, Nebraska, North Dakota, South Dakota, and Wyoming did not issue general obligation bonds during our sample period. These states do receive ratings, however, and estimates from models including these states are very similar to the results we present here in the main text.

[11] One may question the use of bond ratings, rather than interest rates, as the dependent variable. Where bond ratings are a direct response to risk, interest rates are a function of risk and several other factors unrelated to risk, such as the supply of available debt, expectations for inflation, or relative risk in the stock market, to name only a few. As such, bond ratings supply a much more direct estimate of our theoretical concept.

entire sample,[12] make the probit model a cumbersome choice.[13] However, the underlying risk θ_{it} may be efficiently estimated via MCMC hierarchical ordered factor analysis (Quinn 2004).[14] This process, and similar variants, have been used to estimate an array of unobservable, yet substantively interesting political economic concepts including a state's level of democracy (Treier and Jackman 2008), individuals' motivation to be productive at work (Bertelli 2007), and, most salient for our purposes, state credit risk (Krueger and Walker 2008, 2010).

There are several benefits to this approach. First, the recovered values of θ_{it} are on a common scale that is comparable across states and years and theoretically bounded $\left(\theta_{it} \in (-\infty, \infty)\right)$, which dramatically eases the estimation of its predictors in the analysis to follow. Second, the rating houses do not rate each state in each year, as mentioned above. This may lead to an abundance of missing data in alternative approaches, such as analyzing each individual rating, or force the researcher to make assumptions about the nature of missingness, as would be the case in analyzing numerical means of the ratings. But the estimation procedure employed here does not use listwise deletion to account for missingness, nor does it ignore missingness. Instead, missingness merely impacts the certainty of our θ_{it} estimates. This leads to the third advantage: estimating θ_{it} yields a distribution of errors that may be modeled explicitly to better estimate uncertainty about the relationship between legislative capacity and credit risk. Finally, the most popular approach, taking a numerical mean of the ratings (Depken and Lafountain 2006), or even a more coarse rank mean (Kelemen and Teo 2014), makes powerful linearity assumptions *within* scales and comparability assumptions *across* scales. That is, this approach assumes that the difference between S&P AAA and AA+ ratings is the same as the difference between S&P A+ and A ratings *and* that these differences are both equivalent

[12] That is, while every rating house has about 10 distinct ratings they may issue, only about 8 ratings per house are actually issued over the period of analysis.

[13] To clarify what data would actually enter the right-hand side of the equation in an ordered probit model used to estimate θ_{it} note the following. Where missingness and sample size are not problems, the model would take the shape of a hierarchical ordered probit where the dependent variable is the ordered bond rating and the independent variables are vectors of state-year indicators and random slopes and intercepts are estimated for rating houses. Then, the state-year parameters could be used to construct estimates of θ_{it} free of rating house effects. This process would require more than three ratings per year to efficiently identify the state-year parameters. Thus, we employ the empirical strategy described below.

[14] The model described by Quinn (2004) is estimated using the MCMCpack software package (Martin, Quinn and Park 2011).

to the difference between a Moody's A3 and Baa1. This assumption is unlikely to hold, but does not have to be made with the approach we employ.[15]

To illustrate the variability of the credit ratings across states and over time, Figure 1 plots the annual risk estimates for all general obligation bond issuing states. The red line in each pane represents the mean risk estimate for that state over all years, while the gray band shows the difference between the minimum and maximum risk estimate for that state. As the plot shows, there is significant variation within states over time, as one would expect, but much more variation *across* states. Indeed, roughly 80% of the observed variation is attributable to cross-sectional, rather than temporal, factors.[16] This implies that the factors explaining the majority of the variation are more likely to be variables that are relatively consistent within states over time, but vary across states. Variables of this sort include legislative capacity as well as constitutional constraints on fiscal policy and budgeting procedure.

For our focal independent variable, *legislative capacity*, we utilize the 'industry standard' measure from the literature on legislative professionalization in the U.S. states: the Squire Index (Squire 1992, 2007). In both theoretical and quantitative construction, the Squire Index is meant to capture a legislator's motivation to invest in policy expertise, as well as the informational resources at her disposal and time to affect policy change. This measure, which is bounded $[0, 1]$, assumes that the U.S. Congress is "the archetypal professional legislature" (Squire 2007, p. 212) and is meant to capture the degree to which a legislature resembles the Congress on three dimensions: legislator compensation, days in session, and staff per member. On this scale, 1 indicates that a state legislature has policymaking resources on par with Congress and 0 indicates an effective absence of resources. Thus, this measure captures the key institutional components discussed in our theory above. Additionally, this measure has been used to capture legislative capacity in recent work on state politics, allowing us to add comparable results to existing research (e.g., Boehmke and Shipan

[15] All that said, Krueger and Walker (2008) provide compelling evidence that the measure we use is highly correlated with the numerical mean and that correlation is also present in our sample. Interested readers may find more information on this in the appendix.

[16] Following Fortunato, Stevenson and Vonnahme (2016) we can come to this estimate of the cross-sectional and temporal variation by estimating a hierarchical model of bond risk including only random intercepts for states and years.

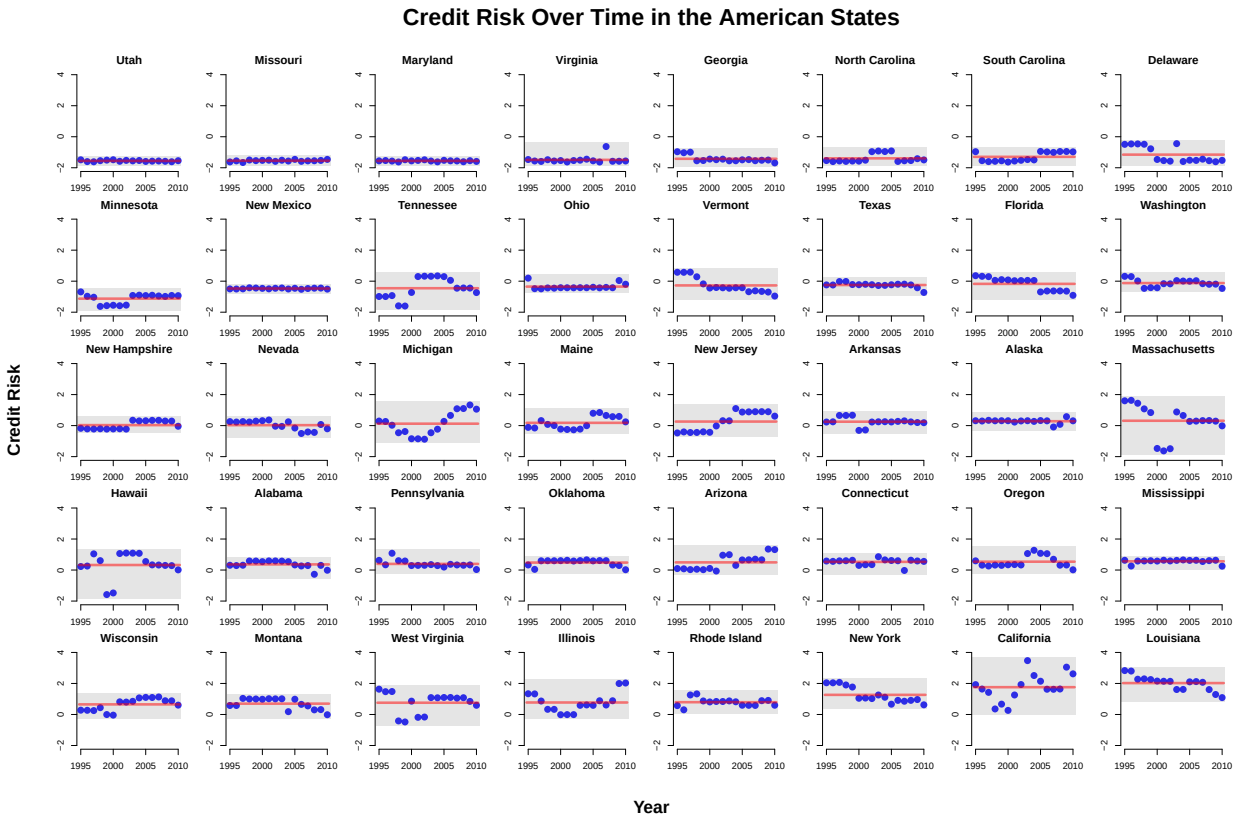

Figure 1

2015; Lax and Phillips 2012).

With both the operationalized dependent and focal independent variable in hand, we can begin to assess the relationship between legislative capacity and debt risk. Figure 2 plots the estimated risk values for all states against that state's Squire Index value for each year in the sample, where greater values on the $y-axis$ indicate riskier borrowers and greater values on the $x-axis$ indicate higher capacity legislatures. The blue points represent states that issue general obligation bonds and the red points represent states that do not issue general obligation bonds, but are nonetheless issued debt ratings.[17] As the plot of the raw values in Figure 2 clearly shows, there is a positive relationship between bond risk and legislative professionalization in each year save one and the relationship is quite strong in most years. Though this is by no means conclusive, it is certainly encouraging that our hypothesized relationship is so apparent in the raw data. The one year in which the hypothe-

[17]Omitting these states only makes for a more clear positive relationship between capacity and risk

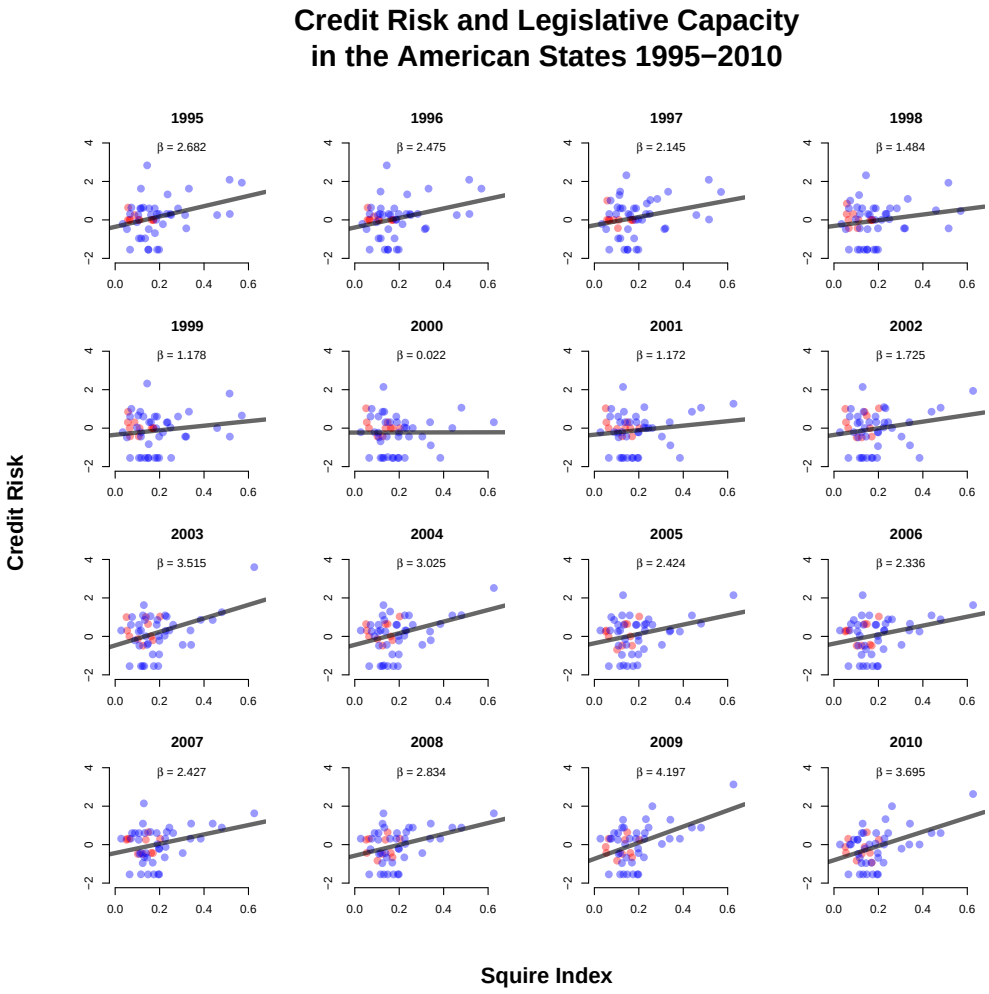

Figure 2

sized relationship is not clearly shown is 2000, when the booming economy led to large surpluses that smoothed credit risk across states (indeed, the mean budget surplus in 2000 was 4.35 times greater than the mean surplus in all other years). Below, we estimate a fully specified model that accounts for such economic factors as well as other potential confounders, thereby providing a clearer picture of the relationship of interest.

Model specification

In an effort to better identify the magnitude and certainty of the relationship between credit risk and legislative capacity, we gather data on an array of potential confounders, many noted in the discussion above. The model dictates inclusion of variables capturing states' institutional prerogatives toward accumulating and repaying debt, its ability to generate the required revenues to maintain its debts, and its level of outstanding debt to complement the measure of legislative capacity. Fortunately, there is ample guidance in extant literature as to which variables are important to include — we build on two recent studies in particular: Krueger and Walker (2008) and Kelemen and Teo (2014).

The institutional variables include several binaries indicating the presence of constraints to fiscal policy. If a state has a legally imposed *revenue limit*, its ability to raise sufficient funds to maintain its debt may be impeded. State's with a *spending limit* may be less likely to incur debt, however, these states may also be less able to make long or short term investments in production (infrastructure or education, for example), which could retard debt maintenance or increase the probability of incurring future debt in order to meet spending obligations. Similarly, states with an explicit *debt restriction* may be less likely to borrow irresponsibly, but may also face difficulties in making investments when the state of the lending market makes doing so advantageous. The final institutional variable regarding fiscal policy is the Advisory Council on Intergovernmental Relations index of fiscal rule rigidity, ranging from 0–10, where 0 is indicative of the least regimented budgetary procedure and 10 is indicative of the most constrained procedure. This serves as a type of summary measure for a state's ability to credibly commit to a balanced budget moving forward. Following Poterba and Rueben (1999), this variable enters the model folded into a binary where 1 indicates a score of *less* than six — representing lax budgetary guidelines — and 0 indicates otherwise. This variable is referred to as *ACIR lax* in the table below.

Krueger and Walker (2008) and Kelemen and Teo (2014), and nearly every other political science investigation of bond ratings or yields, employ a nearly identical set of economic variables, each measured at the level of the state-year, to account for a state's ability to generate sufficient

revenue to repay its debts. *Per capita income* provides a measure of state wealth, or, the potential revenues that states may collect to service their debts or otherwise harness to improve productivity in either the long or short term. The *unemployment rate* serves as a similar proxy. More importantly, it provides leverage on the size of the potential welfare state as well as the size of the voting coalition that may demand transfers, which could potentially come at the expense of debt maintenance. Further, while per capita income provides information on the depth of wealth that can be taxed, the unemployment rate provides information on the size of the taxable population, which is particularly informative given variation in how progressive tax codes are across states.

The present rates of *per capita revenue*, *average tax rate*, and *total per capita spending* provide information on a state's present rate of taxation and spending, which, as we know, is the best predictor of the future balance of revenue to debt obligation. These variables also provide information on a state's willingness or propensity to collect the revenues required to service its debt as well as its willingness or propensity to direct those revenues towards servicing its debt. The final economic control variable is a state's level of outstanding *debt*. Income, unemployment rate, revenue, and spending are rescaled to be standard normal before entering the statistical model to aid estimation efficiency and the interpretation of the estimates, though the substantive results do not change if this rescaling is omitted.

We also include several political variables that may influence legislative productivity or otherwise contribute to policy uncertainty. The first is *divided government*, which may both decrease productivity (Tsebelis 2002), particularly in the short term, and increase uncertainty. As Krueger and Walker (2008) note, small shocks in public support may change a comparably unproductive divided government into a very productive unified government, and, more importantly, it is difficult to forecast the direction of future policy change when the government is divided. The model also accounts for the likelihood of government *turnover*, that is, a change from Democratic to Republican, or Republican to divided control, etc., by including information covering the last several decades of elections. For this, we gather the results of all state elections from 1970 to the present and compute the number of changes in the partisan alignment of government over the number of elections in a

given state in that period for each year in the sample period (1995-2010). This means that a 1 indicates that every election has resulted in a power transition, 0 indicates that there have been no power transitions, and 0.5 indicates that one out of every two elections has resulted in a power transition. This measure is then weighted such that the result of more recent elections count more heavily than more distant elections.[18]

Finally, we include an indicator for the presence of legislative *term limits*. Previous research has hypothesized that term limits increase the risk of lending to states by limiting the accumulation of policy expertise by individual legislators and decreasing incentives for cooperation across caucuses, and therefore decreasing ability to set good fiscal policy (Lewis 2012). This could be viewed as at odds with the argument we have made here. However, an uncertainty based model (rather than an information deficiency model) of how term limits should impact credit risk would yield the same empirical implication. Term limits inherently increase the uncertainty of future policy outcomes by *mandating* change in the membership of the legislature. Markets are simply incapable of forecasting policy outcomes when the composition of the assembly itself is unknown. It is therefore possible, that any preference for dilettante legislators driven by their comparative inability to reshape policy, may be overcome by a preference for stable legislative memberships.

Results

Our data are a balanced cross-sectional time series of the forty states that issue general obligation debt over 16 years and we estimate feasible generalized least squares (FGLS) models that are, for our sample, robust to autocorrelation in the dependent variable and heteroskedasticity in the error structure (Greene 2003).[19] Because the dependent variable, risk, is an estimate, it is important to account for the error structure of that estimate. To this end, we estimate 1,000 models using a unique

[18]That is, the value of the variable for a given state in 2000 is equivalent to $\frac{to_{2000}+0.9*to_{1998}+0.9^2*to_{1996}+...0.9^n*to_{1970}}{n}$, where *to* indicates that the election resulted in a turnover and n is the number of observed elections. The choice of which weight to employ, if any, is arbitrary to a degree, therefore the model is estimated with several different weights and we present the estimates using the weight that generates the best predicted values (0.9). Though the choice of weight affects the estimate on the turnover parameter, it is largely irrelevant for the rest of the parameters.

[19]The substantive results are robust to model choice. Interested readers may find alternative estimations in the appendix. We return to discussion of robustness below.

draw from the distribution of risk estimates derived from the factor analysis described above as the dependent variable in each model. For each iteration, we take 100 draws from the model posterior.[20] This process yields a matrix of 100,000 parameter estimates that are summarized in Table 1 below.

Covariate	Mean	(sd)
Intercept	-0.886	(0.449)
Squire Index	2.154	(0.713)
Term Limits	0.077	(0.064)
Divided Government	0.050	(0.032)
Historical Turnover	0.738	(0.937)
ACIR Lax	1.082	(0.351)
Unemployment Rate	0.070	(0.022)
Per Capita Income	-0.233	(0.052)
Average Tax Burden	2.734	(3.612)
Per Capita Spending	0.190	(0.184)
Per Capita Revenue	-0.044	(0.058)
Per Capita Debt	-0.042	(0.102)
Revenue Limit	-0.196	(0.284)
Spending Limit	0.138	(0.222)
Debt Restriction	-0.243	(0.291)
N	640	
$\tilde{R}^2$	0.271	

Table 1: Main Analysis: Estimates of Annual State Credit Risk.

Before examining the hypothesized relationship, there are a few general observations to be made. First, the estimate of nearly every parameter is in the direction the extant literature would predict, even if their effects are statistically negligible. The sensibility of the results verify that the dependent variable is truly measuring lending risk and also suggest that the model is properly specified. If, for example, the model suggested that high unemployment rates and low per capita incomes significantly reduced lending risk, then one would have reason to be skeptical of the model results in general. This is not the case — the models suggest just the opposite. Lax fiscal policy guidelines, as identified by the ACIR, also substantially increase lending risk, and, in general, our results comport with recent research (e.g., Kelemen and Teo 2014).

Our focal variable, the *Squire Index* is in the predicted direction and is quite robust statisti-

[20]The posterior draws are taken in the usual way (King, Tomz and Wittenberg 2000).

cally. Indeed, over 99.5% of the posterior draws for legislative capacity are positive. Perhaps more importantly, the effects are substantively quite large. Averaging over the sample, increasing legislative capacity from its mean by one standard deviation effectively doubles credit risk. This change is almost precisely the equivalent of changing a state's legislative capacity from that of Texas to that of New Jersey. This is very strong support for the capacity hypothesis in both statistical and substantive terms. Lending markets evaluate states with greater legislative capacity as riskier borrowers.

We can place this finding in context by calling upon previous research to put it in more concrete terms. Using the data on state bond yields presented in Poterba and Rueben (2001), we can predict the change in real borrowing costs levied by the risk increase for the case of the middle 1990's. The data suggest that change in real borrowing costs created by a first difference increase in legislative capacity is an increase of over 4 basis points. Averaging over the states, this amounts to an increase of approximately $0.80, per person, per year, in 1998 dollars, which, as Poterba and Rueben (2001) point out, is a self-perpetuating rise in cost as interest is partially a function of indebtedness. This substantive effect is greater than the effect of a 5% increase to the deficit and nearly twice as large as a 1% increase in unemployment as predicted by Poterba and Rueben (2001). That is, there is a very real political economic downside in the form of riskier credit evaluations for having a high capacity legislature, the burden of which falls largely upon taxpayers.

Other political characteristics included in the model are positively correlated with risk evaluations, although the relationships do not reach traditional levels of significance. States with divided governments, where forecasting the direction of policy change is more difficult, are characterized as riskier borrowers and the data also suggest a positive correlation between term limits and turnover and credit risk. The direction of these results comports with the theoretical arguments here, as well as previous research (e.g., Krueger and Walker 2008; Lewis 2012). Importantly, while models interacting divided government (or historical turnover) with capacity show no signs of an interactive relationship, a model interacting capacity with term limits produces robust evidence for a positive interactive relationship. It is possible that this is due to the nature of the uncertainty induced by divided government and term limits. Divided government makes forecasting the direction of policy

change in the short term more difficult, whereas term limits *mandate* routinized changes in the preferences of the legislature, subject to shocks in the preferences of the electorate, in perpetuity. Given the presence of legislative institutions that allow these perpetually changing coalitions to rapidly and comprehensively change policy, forecasting future political economic environments becomes exceedingly difficult. However, a legislature that lacks the capacity to translate changing distributions of legislative preferences into new policy outcomes serves as a de facto credible commitment to the status quo—mitigating the credit risk induced by institutionalized uncertainty of the legislature's composition.

Robustness

While the majority of the variation in our dependent variable and covariate of interest comes from cross-sectional differences, a great deal of the power in our data comes as a result of having multiple years for each state and, although there is variation within states over time in credit risk, there is less variation within states over time in capacity. As such, we discuss other model specifications, handling the structure of the data in different ways, in order to alleviate any concerns the reader may have regarding the mistreament of our data or misspecification of our models. The details of these robustness checks can be found in the appendix, but we briefly summarize those efforts here.

First, we account for time by estimating results from a more standard OLS model with "panel corrected" standard errors (Beck and Katz 1995) and by estimating models with lagged dependent variables. Second, we model out temporal factors altogether by estimating a two-stage model where, in the first stage, we regress risk in a particular state over time conditioned on its time-varying factors (economic variables, divided government, term limits, and turnover). We then use these estimates to predict a typical risk estimate for each state and regress that on its mean Squire Index score and institutional factors that do not vary over time (debt, revenue, and spending limitations in addition to its ACIR score). We also simply ignore time-varying factors and collapse risk estimates to their state means and regress these on the institutional parameters. Finally, we assess the data dependence of our results by bootstrapping both the full models and the two-stage model just described. In every

estimation there is a robust, positive relationship between legislative capacity and credit risk, in line with our argument.

Discussion and conclusions

This manuscript argued that high capacity legislatures — legislatures that provide robust informational resources and incentivize legislators to invest in the policymaking process — increase responsiveness by providing legislators with the resources needed to not only pursue their own policy goals, but also to identify changes in the preferences of the electorate and legislate accordingly. This, in turn, makes predicting future political economic environments in states with high capacity legislatures much more difficult and, as a result, markets become more reluctant to lend to these states. To test this argument we compared the capacity of legislatures in the U.S. states to market evaluations of their credit risk and found a robust, positive relationship: states with higher capacity legislatures are viewed as riskier borrowers. Indeed, averaging over the sample, a first difference increase in legislative capacity nearly doubles the predicted risk estimate and we noted that this increase (in the 1998 debt market) translates into a roughly $0.80 per capita increase in annual debt maintenance payments for citizens — an increase that compounds over time.

One substantively and theoretically interesting implication of our principal finding regards the nature of democratic representation. As noted above, our theoretical framework posits a representative (mandate/delegate) model of legislative selection and behavior, that implies electorally motivated legislators pursuing the policies preferred by their constituencies. There are competing expectations to our hypothesis rooted in a responsible (accountable/trustee) government model of policymaking, where voters are attentive to outcomes rather than instruments, or, at least that legislators are more concerned with achieving certain outcomes than with implementing certain policies. These typologies, which are critical to understanding the nature of democratic governance, have been the subject of intense theoretical debate in political science (e.g., Rehfeld 2009; Mansbridge 2011). However, evidence prioritizing one model over another has been largely limited to formal (e.g., Fox and Shotts 2009; De Mesquita and Friedenberg 2011) or experimental analysis (e.g., Woon

2012, 2014), while the majority of observational research tends to focus on whether or not leaders are held accountable for outcomes or whether or not voters seem to issue policy mandates.[21] Our empirical findings in this paper bring new evidence to bear on this conversation and suggest that legislators tend to act as delegates, delivering different types of policies in response to shifting preferences in the electorate. That is, they seem to be responsive to public sentiment in terms of policy choice, in line with previous research (Lax and Phillips 2012). At the very least, we present robust evidence that lending markets believe legislators behave this way and issue credit ratings accordingly.

There are several other substantive conclusions to draw from this study, apart from the effects on risk and real borrowing costs and their implications for representation. First, the manuscript provides evidence that legislative capacity increases the rate and depth of policy change. As mentioned earlier, previous research has found a positive relationship between capacity and the number of bill submissions (Hedlund and Freeman 1981; Squire 1998), yet it is difficult to determine the degree of policy change without examining outcomes as legislative proposals can and do vary substantially in both their scope and their probability of becoming law. Here, by examining market reactions to a legislature's ability to change policy, the study provides evidence that capacity not only increases the speed of bill initiation, but also increases the rate and depth of policy change.

The analysis also provides further evidence that the structure of political institutions has broad and wide-ranging political economic consequences. Previous literature has already documented how high capacity legislatures can increase congruence between public preferences and policy outcomes (Lax and Phillips 2012; Maestas 2000), just as it has documented the power of fiscal institutions, particularly those that increase transparency and constrain deficits, in affecting borrowing costs by reducing market uncertainty (Alt and Lassen 2006; Lowry and Alt 2001). This study has married these two streams of the literature and in so doing found that the normative benefits of a professionalized assembly come at a real public cost. This is novel on two levels.

First, the overwhelming majority of fiscal instruments employed to either credibly commit

[21] There are also empirical accounts comparing the institutional contexts that make one type of selection more likely than the other (i.e., Przeworski and Stokes 1999; Powell 2000).

to sensible fiscal policy or alleviate market information deficiencies come at some cost. But these costs are typically characterized in terms of the probability of political survival — as a function of inhibited ability to engage in electoral budget cycling — or an increased probability of austerity in times of economic difficulty. But if we consider the possibility of decreasing legislative capacity in an effort to decrease lender uncertainty and secure better borrowing rates, that particular change comes at a completely different type of cost, one that inhibits the governing coalition's ability to enact its policy program *even in times of great economic prosperity* — a cost that is not imposed by balanced budget rules or high transparency. From a normative standpoint, many would consider this particular cost unbearable.

The second novel implication of this finding is the realization that providing the policies that citizens prefer is more expensive than previously understood. Not only must states weigh the relative costs and benefits of a particular policy, but they must assume a significant cost for the privilege of making that choice. In a sense, lending markets punish governments for doing precisely what they are designed to do.

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Supplemental appendix

Description of risk estimates.

Figure 3 compares our factor analysis derived risk estimates to the numerical means of the bond ratings and also shows the distribution of the error estimates across the estimates. The correlation between the two measures is quite high and, indeed, as the next section shows, results of a model using the numerical mean as the dependent variable deliver the same substantive results that we present and discuss in the main text. We also note the outlier error estimates in the lower left pane of the figure — these are estimates from 11 states, the 10 non-issuing states, thus omitted from the main analysis, and Arizona, which does issue bonds, but was nonetheless unrated by two of the ratings houses for a large portion of our data.

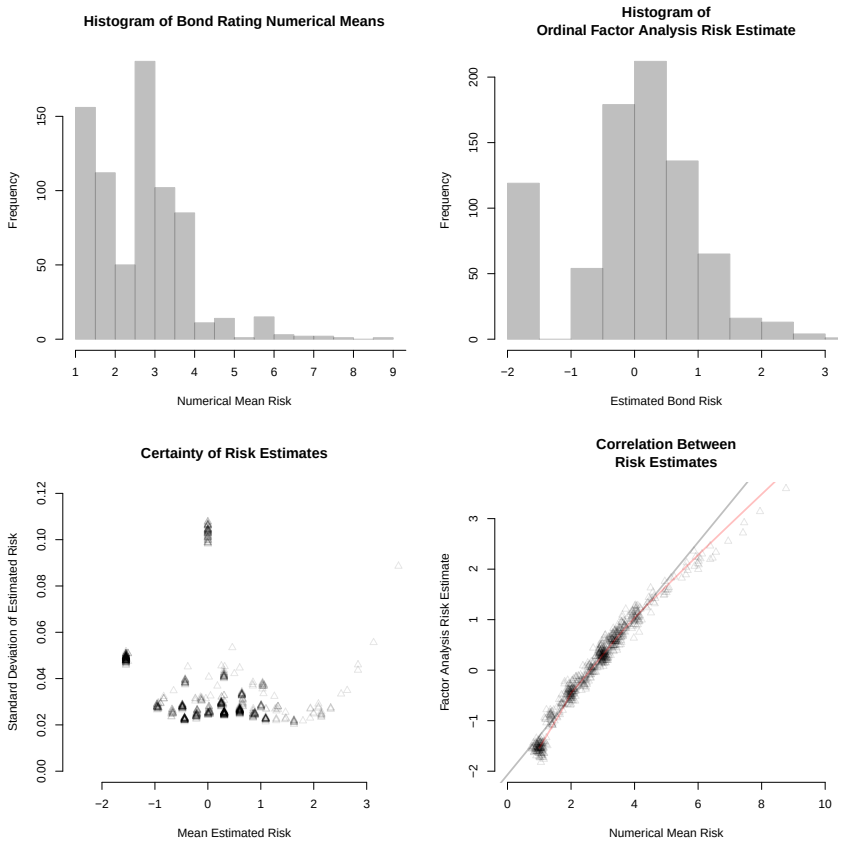

Figure 3

Table 2 estimates the model presented in the main text, but substitutes the estimated risk dependent variable for the numerical mean of the bond ratings. The recovered effects are of largely the same magnitude and similarly robust.

Covariate	Mean	(SE)
Intercept	1.573	(0.527)
Squire Index	2.715	(0.854)
Divided Government	0.095	(0.077)
Historical Turnover	0.091	(0.038)
Term Limits	1.487	(0.420)
ACIR Lax	1.310	(0.561)
Unemployment Rate	0.091	(0.031)
Income	-0.283	(0.064)
Average Tax Rate	2.375	(4.309)
Spending	0.047	(0.207)
Revenue	-0.015	(0.063)
Debt	0.061	(0.119)
Revenue Limit	-0.311	(0.336)
Spending Limit	0.452	(0.272)
Debt Restriction	-0.266	(0.345)
N	633	
R^2	0.293	

Table 2: FGLS Estimates of Numerical Average of State Credit Risk.

Comparison of capacity estimates.

Below, we compare the Squire Index to alternative decompositions of capacity — Bowen and Greene's (2014) first dimension estimates from classic multi-dimensional factor analysis and Quinn's (2004) Bayesian normal and ordinal factor analysis. Each of these measures employ the same underlying measurements as stimuli (salary, staff, and days in session) and are compared are Figure 4. Clearly, the correlation across the measures is quite high and nearly perfectly linear, with the exception of the ordinal factor analysis. Because the estimates are so similar, we choose to employ Squire Index in the main text because it is most familiar to most readers and it allows our results to be compared to other research on professionalism. However, all measures provide statistical and

substantive support for our theoretical arguments as shown in Table 3, which re-estimates the model shown in the main text with each capacity measure.

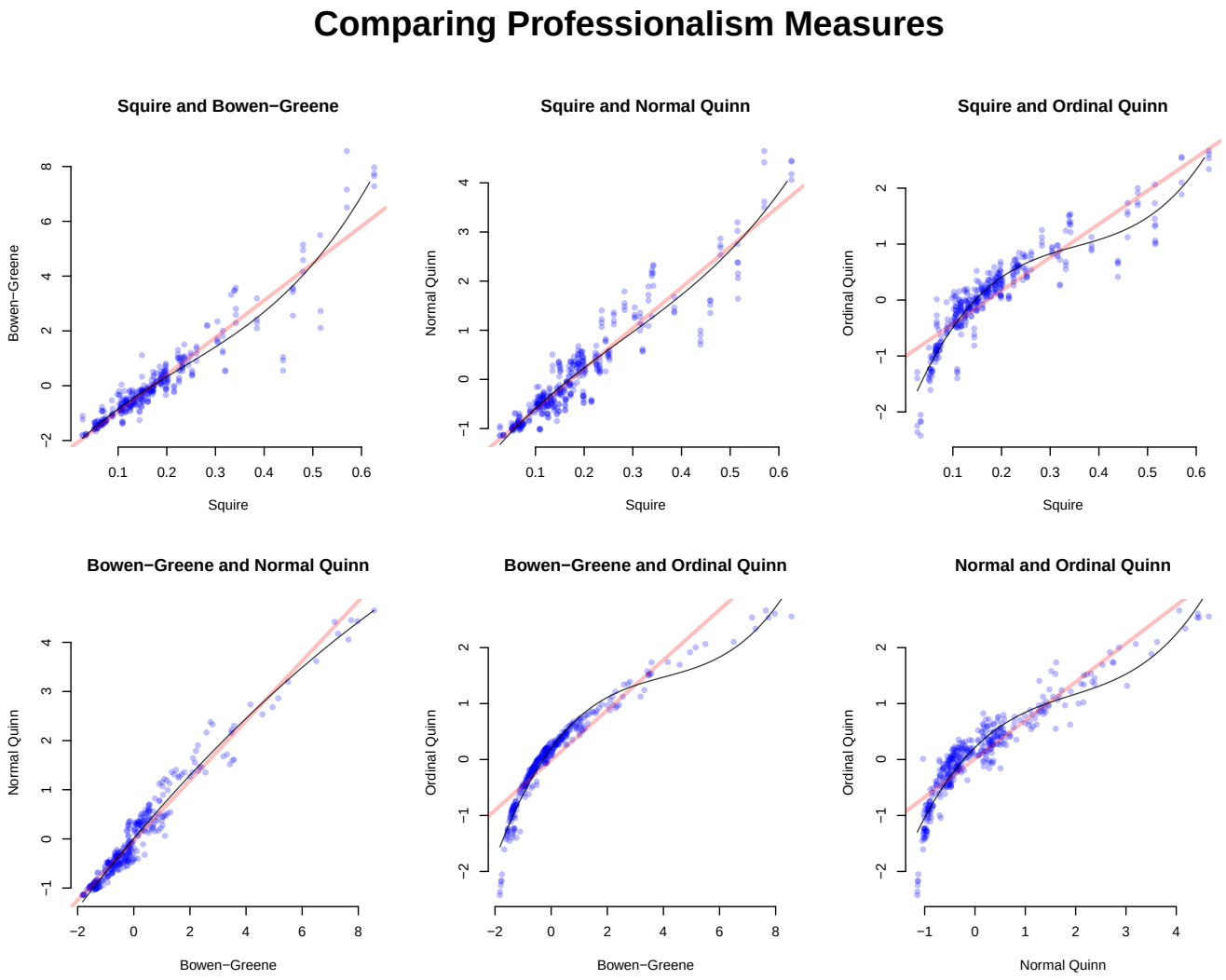

Figure 4

Alternative model specification

In this section we present several different specifications and estimations of the statistical model presented in the main text. The point here is the assure the reader that the evidence we recover in support of our theoretical arguments is firmly "in the data" and not an artifact of model choice. We begin by presenting an OLS model estimated with panel-corrected standard errors following

	Squire Index		Normal Quinn		Ordinal Quinn		Bowen-Greene	
Covariate	Mean	(SE)	Mean	(SE)	Mean	(SE)	Mean	(SE)
Intercept	-0.882	(0.431)	-0.729	(0.434)	-0.625	(0.444)	0.435	(0.380)
Legislative Capacity	2.145	(0.682)	0.208	(0.058)	0.196	(0.066)	0.077	(0.029)
Divided Government	0.050	(0.030)	0.040	(0.030)	0.051	(0.030)	0.037	(0.024)
Historical Turnover	0.765	(0.886)	0.606	(0.882)	0.378	(0.871)	0.918	(0.821)
Term Limits	0.079	(0.062)	0.058	(0.061)	0.067	(0.061)	0.002	(0.063)
ACIR Lax	1.070	(0.340)	1.120	(0.344)	1.041	(0.354)	0.458	(0.284)
Unemployment Rate	0.070	(0.021)	0.070	(0.021)	0.062	(0.021)	0.047	(0.020)
Income	-0.230	(0.049)	-0.234	(0.048)	-0.226	(0.046)	-0.228	(0.045)
Average Tax Burden	2.749	(3.477)	4.196	(3.425)	3.559	(3.447)	-4.404	(3.234)
Spending	0.190	(0.181)	0.193	(0.179)	0.162	(0.178)	0.134	(0.185)
Revenue	-0.044	(0.056)	-0.032	(0.057)	-0.025	(0.055)	-0.026	(0.059)
Debt	-0.042	(0.100)	-0.053	(0.100)	-0.024	(0.098)	-0.024	(0.104)
Revenue Limit	-0.195	(0.275)	-0.194	(0.273)	-0.162	(0.280)	-0.260	(0.259)
Spending Limit	0.133	(0.216)	0.173	(0.217)	0.160	(0.223)	0.817	(0.189)
Debt Restriction	-0.257	(0.282)	-0.162	(0.283)	-0.196	(0.292)	-0.468	(0.224)
N	640		640		640		552	
R^2	0.268		0.250		0.221		0.213	

Table 3: FGLS Estimates of State Credit Risk: Alternative Capacity Measures.

Beck and Katz (1995). This model produces substantially smaller error estimates for nearly every covariate — typically doubling the ratio of the parameter to error estimate.

Covariate	Mean	(SE)
Intercept	-0.949	(0.148)
Squire Index	2.905	(0.265)
Divided Government	0.166	(0.067)
Historical Turnover	4.049	(1.406)
Term Limits	1.188	(0.072)
ACIR Lax	0.161	(0.049)
Unemployment Rate	0.124	(0.046)
Income	-0.345	(0.036)
Average Tax Burden	0.478	(1.529)
Spending	0.242	(0.150)
Revenue	0.011	(0.105)
Debt	-0.031	(0.114)
Revenue Limit	-0.413	(0.065)
Spending Limit	0.264	(0.059)
Debt Restriction	-0.145	(0.056)
N	640	
R^2	0.335	

Table 4: OLS Estimates of Numerical of State Credit Risk with Panel-Corrected Standard Errors.

Table 5 gives the results of the main text model with the addition of a one-year lag on the dependent variable to demonstrate that the results are robust to explicitly modeling auto-correlation in the dependent variable. Of course, our focus is modeling cross-sectional variation in general

obligation bond risk, rather than within-unit changes over time, making this specification a poor match to our theoretical angle. Nonetheless, it is comforting that our hypothesized relationship is robust to this type of model.

Covariate	Mean	(SE)
Intercept	-0.162	(0.010)
Lagged Risk	0.925	(0.012)
Squire Index	0.192	(0.111)
Divided Government	0.007	(0.019)
Historical Turnover	0.032	(0.522)
Term Limits	0.059	(0.046)
ACIR Lax	0.002	(0.024)
Unemployment Rate	0.026	(0.009)
Income	-0.011	(0.012)
Average Tax Burden	0.644	(0.961)
Spending	-0.075	(0.077)
Revenue	-0.001	(0.063)
Debt	0.075	(0.038)
Revenue Limit	-0.050	(0.037)
Spending Limit	0.048	(0.030)
Debt Restriction	0.009	(0.037)
N	600	
R^2	0.875	

Table 5: FGLS Estimates of State Credit Risk with Lagged Risk Estimate

Table 6 gives the parameter estimates resulting from a model interacting capacity with our other specified sources of political uncertainty: divided government, historical turnover patters, and term limits. We discussed these estimates in the concluding paragraph of the results section in the main text. As discussed in the main text the divided government and historical turnover interactions wash out, but the interaction with term limits produces a robust positive relationship, while substantially reversing the direction of the term limits constituent term and leaving the Squire Index parameter virtually unchanged. Our substantive interpretation of this result is that term limits may increase market uncertainty and therefore increase a state's riskiness, but these effects may only manifest in the presence of a high capacity legislature that offers the resources necessary to convert the turnover in membership yielded by term limits into a corresponding turnover in policy.

In Table 7 we assess the data dependence of our results via non-parametric bootstrap. We randomly sample (with replacement) 160 observations — just 25% of our total sample — and estimate our model via linear regression, take 100 posterior samples and record the results. These

Covariate	Mean	(SE)
Intercept	-0.978	(0.457)
Squire Index	2.183	(0.773)
Divided Government	0.062	(0.059)
Historical Turnover	3.269	(1.933)
Term Limits	-0.146	(0.114)
ACIR Lax	1.118	(0.353)
Unemployment Rate	0.061	(0.021)
Income	-0.246	(0.051)
Average Tax Burden	3.619	(3.661)
Spending	0.171	(0.177)
Revenue	-0.016	(0.057)
Debt	-0.050	(0.099)
Revenue Limit	-0.181	(0.284)
Spending Limit	0.123	(0.222)
Debt Restriction	-0.262	(0.290)
Squire X Divided Government	-0.088	(0.249)
Squire X Historical Turnover	-14.512	(8.819)
Squire X Term Limits	1.156	(0.433)
N	640	
R^2	0.276	

Table 6: FGLS Estimates of State Credit Risk with Interactions.

distributions are summarized in Table 7. We employ OLS, rather than the more standard time-series models because the sampling procedure often yields observation duplicates. Roughly 98% of the models yield support at the $p < 0.05$ level and far less than 1% of the posterior draws are negative. This suggests that are results are robust to changes in the sample.

Covariate	Mean	(SE)
Intercept	-0.929	(0.917)
Squire Index	2.903	(1.028)
Divided Government	0.171	(0.197)
Historical Turnover	3.580	(4.957)
Term Limits	0.168	(0.242)
ACIR Lax	1.225	(0.441)
Unemployment Rate	0.119	(0.106)
Income	-0.352	(0.122)
Average Tax Burden	0.226	(8.666)
Spending	0.554	(1.346)
Revenue	-0.086	(1.095)
Debt	-0.183	(0.970)
Revenue Limit	-0.398	(0.365)
Spending Limit	0.262	(0.284)
Debt Restriction	-0.119	(0.358)
N	160	
$\tilde{R}^2$	0.402	

Table 7: OLS estimate summaries from non-parametric bootstrap assessing data dependence.

Here, we address an issue in our data mentioned in the main text: the majority of our variation is cross-sectional, however, a great deal of our power comes from repeated years. Here, we assess this concern in two stages. First, for each of our forty states, we model its estimated risk as a function

of the parameters that vary substantially over time: divided government, turnover, unemployment, income, tax rate, spending, revenue, and carried debt. We then predict, for each state, its typical credit risk, holding all of those time-varying parameters constant. These predictions then become the dependent variable in a second model that estimates the impact of factors that vary predominantly cross-sectionally: legislative capacity, ACIR, revenue and spending limits, and constitutional debt restrictions. We take the mean of the capacity estimates, but the remaining factors do not vary over time. The results of these second models are given in Table 8 and the results are remarkably consistent with those presented in the main text.

Covariate	Reduced	Full
Intercept	−0.699	−0.991
	(0.298)	(0.547)
Squire Index	3.798	3.612
	(1.276)	(1.314)
ACIR Lax		1.079
		(0.621)
Revenue Limit		−0.127
		(0.502)
Spending Limit		0.159
		(0.398)
Debt Restriction		0.216
		(0.520)
Observations	40	40
R^2	0.189	0.281

Table 8: Second stage results from two-stage modeling of state credit risk.

We can go a step further and assess the sample dependence of these results by randomly sampling (with replacement) 40 observations, estimating the model, and repeating 1,000 times. From each of these models we drew 100 posterior samples and then plotted them in Figure 5. As the figure shows, over 94% of the draws are in the predicted direction, which, given the difficulty of the test, we interpret as very strong support for our central hypothesis, and good evidence that the results are not being driven by outliers in the data.

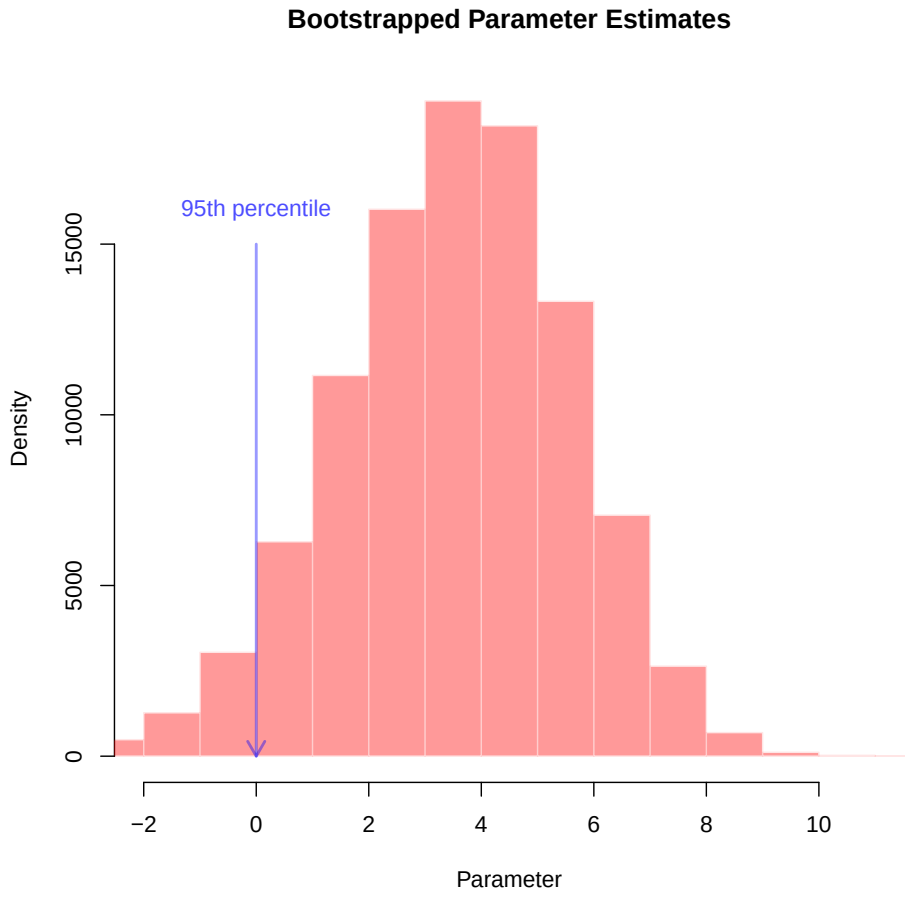
Bootstrapped Parameter Estimates
95th percentile
Density
0
5000
10000
15000
−2
0
2
4
6
8
10
Parameter

Figure 5