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Compensation, Opportunity, and Information: A Comparative Analysis of Legislative Nonresponse in the American States

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Abstract

We present a parsimonious framework for understanding contextual variation in legislative nonresponse. We argue that legislators' propensity to vote is a function of their willingness and ability to be physically present in the chamber and determine the best position to take on a given proposal. From this framework, we derive four hypotheses regarding the compensation legislators receive, their opportunity to pursue work outside of the chamber, and their informational resources. Analyzing data on over seven million voting opportunities across two sessions in ninety-nine chambers, we find robust evidence that longer legislative sessions *decrease* nonresponse and that informational resources *increase* nonresponse, but no evidence that compensation influences nonresponse.

Keywords

institutions, legislative professionalism, legislative voting, nonresponse

Introduction

In the 1999–2000 session of the Alabama House of Representatives, members failed to take a position on over 24 percent of roll-call votes. In the Wisconsin House during that same period, representatives recorded a vote on over 99 percent of roll calls. Legislative nonresponse, whether it is strategic abstention, declining to vote for a lack of information, or simply due to absence from the chamber, is a fundamental failing of legislators to represent their constituents—a break in the process of representative democracy. Each time legislators decline to vote on a legislative proposal, they silence the voice of their constituents, potentially contributing to the realization of an outcome they oppose, and skew the relationship between the public's preferences and the policies its government delivers. Yet, despite its normative salience, the contextual variation observed among the legislative assemblies of the United States (and legislatures around the world, for that matter) has gone largely unexplored. Instead, scholarly examination has predominately argued that the decision to vote is motivated by individual concerns, such as careerism (Cohen and Noll 1991; Hibbing 1986) or dissonance between the preferences of a legislator's party and his or her district (e.g., Rosas and Shomer 2008). By focusing almost exclusively on individual concerns, the extant literature is largely unable to explain variation in aggregate nonresponse rates *across* legislatures and therefore does not allow us to understand how institutional factors, such as compensation, informational resources, and so forth, may shape a legislator's decision to be present and vote—a legislator's decision to represent their constituents. In this manuscript, we address this opportunity in the literature and provide an answer to the question: why are nonresponse rates higher in some legislatures than others?

Our explanation is rooted in simple logic: nonresponse is a function of legislators' willingness and ability to cast a vote. Where past studies have overwhelmingly focused on individual-level factors, however, we step back and consider the contextual factors that contribute to this choice. Building on the existing literature on legislative professionalism, we ask, how may legislative bodies increase their members' willingness and ability to be present in the chamber when the roll is called and take a position on that proposal? We use the ninety-nine state

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legislative chambers of the United States as a testing ground for the general argument and identify four potential answers to these questions. Members may be incentivized to be present by increasing their pay. Members may also be incentivized to be present by curtailing their opportunity to pursue work outside of the legislature. The ability of members to cast a vote may be increased by providing informational resources, such that they may identify which position to take on behalf of their constituents. However, these same informational resources may increase the ability of legislators to identify circumstances in which the preferences of their party and constituents are misaligned and may therefore increase nonresponse as legislators choose not to disappoint one principal for the sake of another. We test these explanations using data on compensation, time to pursue work outside of the legislature, and informational resources on the nonresponse rates of approximately ten thousand legislators over two sessions in ninety-nine chambers, totaling over seven million voting opportunities.[1] The data suggest that compensation exerts no effect on nonresponse; however, increasing the number of days in session (and therefore decreasing legislator's ability to pursue alternative income sources) substantially *decreases* nonresponse, and increasing informational resources substantially *increases* nonresponse. Our analysis also brings evidence to bear on how term limits, electoral competition, seniority, gender, and majority status influence rates of legislative nonresponse as well as the theoretical and empirical benefits of decomposing the Squire Index (Squire 2007)—the industry standard measure of legislative professionalism—into its theoretical components when appropriate.

In the following sections, we first discuss the previous research on nonresponse and institutional variation across the American state legislatures. We then present our contextual explanation of nonresponse and derive hypotheses. Next, we introduce our data and explain our empirical models for testing these hypotheses. Finally, we discuss our empirical findings and conclude with discussion of both the theoretical and normative significance of our findings.

Perspectives on Nonresponse and Variation in Legislative Institutions

The extant literature on nonresponse proposes several potential explanations; however, as noted, nearly all are individual in nature and focus on the role of electoral accountability and competing principals. The accountability argument posits that voting, or any other legislative activity, is costly. Even absent work such as gathering information regarding the real policy effects of a proposal or mapping those effects to constituent preferences, any time spent in the chamber voting is time not spent doing something else. In general, this research has found evidence that once the reins of electoral accountability are off, legislators become less likely to vote. For example, Hibbing (1986, 655) provides evidence that House members seeking election to the senate are far less likely to participate on roll-call votes than their less enterprising counterparts (as their time is consumed by the campaign process), concluding that these ambitious representatives "become virtual truants." Related research has examined the impact of term limits, or the decision not to run for reelection on roll-call behavior. Wright (2007) and Clark and Williams (2013) find evidence that some term-limited state legislators are less likely to participate in roll-call votes, but conclusions on last term effects in the Congress are mixed with Rothenberg and Sanders (2000) finding evidence for the predicted relationship and a subsequent methodological challenge finding no support (Carson et al. 2004). There is similar research from outside the United States as well. Gagliarducci, Nannicini, and Naticchioni (2011) examine the effects of mixed member proportional representation in the Italian Parliament on nonresponse and find that legislators holding first-past-the-post single-member–district seats, rather than seats proportionally allocated from closed lists, exhibit lower rates of nonresponse due to increased accountability to their constituents.

Competing principal explanations argue that legislators face voting pressures from multiple sources. Interest groups or donor coalitions may pressure legislators, but this struggle is typically discussed in terms of party unity being pitted against constituent preferences. When the preferences of constituents and party are discordant, legislators may view abstention as the most effective way to avoid disappointing their constituents without having to vote against their party (e.g., Cohen and Noll 1991). The related literature takes these competing demands as given and attempts to uncover when one side or the other will win out. Forgette and Sala (1999) argue that parties will exert more pressure on their members when predicted margins are narrow and the ideological divide between parties is wide. An analysis of participation patterns in the senate between the Civil War and World War II uncovers support for their arguments, and this finding is corroborated by research on the modern House of Representatives by Rothenberg and Sanders (1999). Similarly, Poole and Rosenthal (2000) argue, and find supporting evidence, that the probability of a given legislator being pivotal to the outcome of the roll call drives one's incentive to vote.

Outside of careerist and competing (ideological) principal explanations in political science, there is also public economics literature on legislative shirking (where shirking is meant to explain nonresponse, rather than ideological drift). For example, Gagliarducci, Nannicini, and

Naticchioni (2010) find that legislators holding high-paying jobs prior to election are more prone to nonresponse as spending their time outside of parliament is likely to be more valuable than spending it within. Subsequent research by Hoffman and Lyons (2013) finds a connection between legislator compensation and roll-call participation, implying that states can offer financial incentives to their legislators to increase their attendance.

Apart from the research cited above, there has been little or no work on how the institutional parameters of legislative chambers affects legislators' propensity to vote. This is particularly surprising given the vibrant literature on legislative professionalism in the American state assemblies. In concept, "professionalism" is the degree to which the institutional structures of assemblies allow legislators to invest in the policy-making process. Operationally, professionalism has come to represent the degree to which a given legislature resembles the Congress in its informational resources, days in session, and the compensation it provides its members—parameters that we will explore in relation to nonresponse below.

The deep and well-known literature on the professionalization of state assemblies has investigated its impact on a wide array of political phenomena, though the literature has tended to focus on electoral outcomes—typically reelection rates, recruitment, or other substantively interesting aspects of chamber composition. For example, Squire (1992) analyzed the impact of professionalism on the recruitment of underrepresented groups finding that more professional chambers tend to have more black representatives, but fewer female representatives. Fiorina (1994, 1997) has argued that more professional assemblies are more likely to attract a fundamentally different type of legislator, one whose alternative career options are less attractive financially, and that this has resulted in disproportionate Democratic success in state legislatures, as Republicans would, on average, have more lucrative job prospects.[2] In related literature, Berry, Berkman, and Schneiderman (2000) and Carey, Niemi, and Powell (2000) argue that professionalization should isolate state legislators from national trends or otherwise increase the incumbent advantage.

Comparatively little research has been directed at understanding how professionalization shapes legislative *behaviors*. Notable exceptions include Huber and Shipan's (2002) research on how more professionalized legislatures enable the creation of higher quality statutory instruments in the policy-making process, Richman's (2008) arguments that professionalization decreases uncertainty regarding the impact of policy outcomes, Gamm and Kousser's (2010) investigation of particularistic versus programmatic policy making in the states, or research demonstrating the positive influence of professionalization on policy congruence—the extent to which policy outcomes reflect voters' preferences (Lax and Phillips 2012; Maestas 2000).

On voting in particular, there has been, to our knowledge, one study that is of present interest. In examining the impact of term limits on legislative participation, Wright (2007) hypothesizes that the effects of term limits should be more pronounced in more professionalized assemblies, as these legislators have greater incentives to perform while reelection is possible—this argument complements the careerist explanations for shirking discussed above. Wright finds support for this claim that professionalism conditions the impact of term limits and also finds lower nonresponse rates in professionalized assemblies more generally. This is informative and comports with the general argument we make below; however, the results Wright uncovers do not allow us to adjudicate between the competing explanations for nonresponse that we offer. Furthermore, analyzing professionalism as an index when there is theoretical incentive (an incentive that is present here, though not in Wright's [2007] work) to disaggregate the measure may obscure our substantive conclusions. More specifically, while Wright finds an overall negative relationship between professionalism and nonresponse, our analysis finds a robust *negative* relationship between nonresponse and one component of professionalism (time in session), a *positive* relationship between nonresponse and another component of professionalism (informational resources), and no relationship at all between nonresponse and the final component of professionalism (compensation).

Contextual Parameters and Nonresponse

In his work on the contextual determinants of nonresponse, Wright (2007) argues that term limits should increase shirking, but only in professionalized legislatures. The intuition is that in citizen legislatures, where meetings are rare, the ability to divine constituent preferences is constrained by a lack of informational resources, the value of reelection is low, and the value legislators derive from voting is already sufficiently low that the introduction of term limits should have little discernible effect. Using the Squire Index (Squire 2007) as a measure of professionalism, Wright finds support for this argument. However, the information contained in the individual components of that index—legislative salary, days in session, and legislative staff—may not all function in the same way. If this is case, then analyzing the relationship between these components and nonresponse via index may lead us to miss certain nuance of the relationship, or, worse, lead us to believe that all components are exerting equal effects in the same direction, when in truth that is not the case. Our approach here is to walk through the general determinants of a legislator's choice to vote and relate these to components of professionalism. Then, in the Empirical Model Specification and Estimation section, we discuss other contextual factors (formal rules and others) factors that may play a role in this choice and model them appropriately to get clean estimates of our variables of interest.

We argue that the propensity of legislators to participate in roll-call voting is a function of their willingness and ability to be present in the chamber and their willingness and ability to take a position on the proposal in question. These are, of course, two different types of nonresponse. The first refers to absence, or what is commonly referred to as shirking. Legislators who are unwilling or unable to appear in the capital for roll call are not performing the duties for which they have been contracted and, more normatively salient, are leaving their constituents unrepresented. The second refers to abstention, the unwillingness or inability to take a position on a bill *given* that the legislator is present for roll call. In this case, the legislator's constituents are still unrepresented, though the cause is different. Unfortunately, our data do not allow us to explicitly differentiate between absence and abstention as they are coded identically (this is a common issue in studies of nonresponse, though it is most often left unmentioned); however, our theoretical argument provides differing expectations for differing institutional constructs. Thus, our empirical analysis, as informed by our arguments, will allow us leverage in inferring whether absence or abstention is the cause of nonresponse. Furthermore, we incorporate data on voting procedures across chambers that provide additional leverage (more on this below).

Again, our argument is that the propensity of legislators to participate in roll-call voting is a function of their willingness and ability to be present in the chamber and take a position, and that this willingness and ability is shaped by institutional context. Institutional structures, particularly those determining compensation and time spent on legislative work, can be set to make pursuing alternative careers while serving in the legislature more or less necessary and more or less difficult. Institutions may also be structured to provide more informational resources to legislators, such that they may be better able to identify the best position (broadly interpreted) to take on a given roll call. We first discuss the pursuit of employment outside of the legislature.

We assume that legislators are driven to pursue earnings outside of the legislature and that their propensity to pursue these earnings is a function of two factors: their need and their opportunity. The impact of need is clear—if legislatures do not pay their members enough to make a comfortable living, then legislators will have no choice but to pursue other sources of income, unless, of course, they are independently wealthy, have a partner that earns sufficient income to provide for them, and so forth. The more competing demands they have on their time as a function of pursuing work outside the legislature to make ends meet, the more likely they are to be absent for roll calls.

Stated alternatively, as the relative value of spending time outside of the chamber, rather than in, increases, so too should the aggregate rate of nonresponse:

> **Need hypothesis:** As legislative income increases, the likelihood of nonresponse should decrease.

It is possible, however, that the need for alternative income streams, does not, in fact, drive nonresponse, but opportunity does, that is, members of the New Hampshire General Court, who meet for only forty-five days per two-year legislative session, or members of the Georgia General Assembly, who meet for only forty days per two-year session, have much more latitude to accept work with high time demands than members of the California State Legislature or the Ohio General Assembly, who have no such limitation on the number of days they may spend in session. Holding salary constant, members of assemblies that meet infrequently, or for few days, are more likely to take on competing demands for their time than members of assemblies that are effectively always in session.[3] This is not dissimilar from the argument Fiorina (1994) makes when theorizing that the institutional roots of Democratic advantage in state legislatures—an advantage that has since eroded—lie in the professionalization of assemblies, reducing the time available to legislators to tend to other business interests and therefore attracting candidates with less lucrative employment opportunities. Our expectation, then, is that longer sessions should deter nonresponse by making it more difficult for legislators to accumulate competing obligations:

> **Opportunity hypothesis:** As the number of days in session increases, the likelihood of non- response should decrease.

The other component of nonresponse is a legislator's ability to determine the best position (broadly interpreted) to take on the bill in question and, of course, one's willingness to take that position (we discuss this more below). The breadth and depth of the policies legislators must vote on is substantial and inferring how a piece of legal text will translate into real-world outcomes is difficult. Legislators cannot reasonably be expected to be experts in every policy area. Indeed, it may be unreasonable to even expect legislators to be able to determine their constituency's most preferred outcome *given* that they had the information necessary to infer the real-world impact of every bill. Of course, legislatures are organized to alleviate these informational difficulties (e.g., Gilligan and Krehbiel 1990), but these organizational choices may not be successful if members are deprived of the necessary resources—resources including, but not limited to, staff to share the burden of accumulating policy information, reading bills and inferring their policy outcomes, and marrying those policy outcome inferences to constituency preferences.

There are other institutional parameters that may help individual legislators alleviate their information deficiencies. Resources allocated to party caucuses or leadership positions that may enable them to invest in information or disseminate word on the "party line"—or, where circumstances and resources allow, the "caucus line"—could also help individual legislators decide which position to take.[4] What the nearly all of these of these resources have in common is that they must be paid for. Thus, rather than counting staff allocations across individual members, committees, parties, caucuses, chambers, and leadership, we follow the suggestion of Bowen and Greene (2014) and measure all (non-legislative salary) expenditures made by the legislature and divide this figure by the number of total legislators. This provides a summary estimate of the total resources, staff and otherwise, available for each member of the legislature to call on for assistance in determining the best position to take on a particular vote. Our expectation, then, is that if inability to make an informed choice on roll calls drives nonresponse, legislators with greater informational resources at their disposal will be more able to identify the right position to take on bills before them, and therefore less likely to abstain from voting[5]:

> **Informed decision hypothesis:** As expenditures per legislator increase, the likelihood of non-response should decrease.

There is a competing informational hypothesis. Representatives have multiple demands on their votes: the preferences of their party and the preferences of their constituents.[6] These competing demands may be discordant, and in these situations, legislators may find abstention preferable to taking a position that may inhibit their party's chances of success or disappoint their constituents and therefore impose electoral risk. But recognizing these circumstances is, in and of itself, a challenge. As in the above discussion of the informational requirements for choosing the "right" position, the legislator must be able to map legislative proposals to policy outcomes and then calculate the most preferred position of their party and constituency to recognize that the interests of their competing principals are misaligned.[7]

In the absence of informational resources, legislators may exist in a state of blissful ignorance, unaware that a "yea" vote *with* their party may be a "yea" vote *against* their district. Maestas (2000) makes a similar argument in her analysis of the congruence of policy outcomes to citizens' preferences: more professionalized legislators are better able to learn the policy preferences of their constituents and act on them. Thus, we may expect that legislators with greater access to informational resources are better able to learn the policy preferences of their constituents and understand when they are discordant with the preferences of their party. In such contexts, abstention is likely preferable to voting. We therefore have a second (competing) informational expectation: that legislators with greater informational resources at their disposal will be better able to identify conflicting preferences of party and constituency and therefore more likely to abstain from voting:

> **Informed abstention hypothesis:** As expenditures per legislator increase, the likelihood of nonresponse should increase.

Moving forward, we estimate the impact of our focal variables on nonresponse while accounting as best we can for possible confounding factors at the individual and contextual level by including appropriate control variables and estimating error components models. Before discussing model specification and estimation, however, we first discuss our data and their structure.

Data

We use the ninety-nine chambers of the American states to evaluate our hypotheses. These chambers provide a nearly ideal testing ground as almost all of the contextual variation that may perturb cross-national analysis—for example, government type (presidential vs. parliamentary), party systems, and so forth—are held constant, yet there is still substantial variation observed over our covariates of interest. For these ninety-nine chambers, we gather data on roll-call participation and our focal variables—compensation, session length, and legislative expenditures—as well as information on the legislators themselves across two sessions.

Our roll-call data are the votes of all legislators in the ninety-nine chambers during the 1999–2000 and 2003–2004 sessions collected and made available by Wright (2004) and Clark et al. (2009).[8] These data have been used to study a wealth of substantively interesting questions from understanding the role of electoral competition in individual and collective legislative behavior (Carroll and Eichorst 2013) to the impact of gatekeeping institutions on majority agenda control (Anzia and Jackman

2013). All told, these data contain information on well over seven million individual voting opportunities and will help us to track correlations between context and aggregate, as well as individual, trends in voting behavior. The richness of the data, however, comes with an associated cost as its hierarchical structure (aggregating the behavior of many legislators, on many votes, nested within chambers, which are nested within states), along with its sheer size and the shape of the dependent variable presents a modeling challenge. We discuss our solution to this challenge in detail below.

Our focal covariates are borrowed from Bowen and Greene (2014), who present data on total annual salary, days in session, and expenditures—the total spent on legislative resources, not including legislator salary, in 2010 dollars. There were a few missing observations in their data; however, we are able to remedy this by consulting the Book of the States or reaching out to the chamber directly. Note that we convert the annual salary into a daily salary by combining the regular salary with any additional stipend income and dividing by the appropriate number of days in session. The idea here is to approximate the legislator's incentive to be present on any given day. As it turns out, however, there is no substantive difference in the recovered relationships when using our daily salary measure, or Bowen and Greene's original annual salary measure.

Empirical Model Specification and Estimation

With our focal variables in hand, we may now address potential confounders. First, as previous research on the role of majorities in organizing legislative behavior and outcomes has taught us, members of the majority party are far more likely to find themselves on the winning side of any given vote as a function of their caucus' size and the majority's manipulation of the legislative agenda (Cox and McCubbins 1993). Furthermore, majority members also have privileged access to information (Fortunato 2013). As minority members are perennial policy losers and are rarely, if ever, able to block the majority from imposing its will, it is possible that they may be less likely to bother voting, particularly when seat margins are wide, and less likely still that they are able to identify their constituents' preferred position on a roll call. We therefore account for the majority status of representatives in the analysis below.[9]

The impact of majority status, however, should be conditioned by the relative size of the party. Conditional party government predicts that parties are more likely to flex their muscles and pressure their members to vote the party line when seat margins are narrow (Aldrich and Battista 2002; Aldrich and Rohde 2000).[10] By including the strength of the majority party in our model, we should be able to control for fluctuations in nonresponse as a result of party pressures to get a cleaner estimate of our focal variables. We therefore include a measure of majority strength—the logged ratio of majority seat share to minority seat share—and interact it with majority status.[11] These variables should also provide leverage in identifying the tendency of legislators to abstain under opposed competing principals, that is, parties should be more willing to tolerate defection as the majority-minority size differential increases because individual votes matter less in determining legislative outcomes.[12]

Moving on to institutional characteristics, we include the duration of the legislative term served by chambers (either two or four years) and we account for the presence of term limits by including a binary variable indicating that term limits have taken effect in the chamber as well as the proportion of legislators who will be "termed out" at the end of the session (Clark and Williams 2013). Note that there is no need to formally interact these variables as they constitute an "implied interaction," where the proportion termed out variable may only take on a value greater than 0 when the term limits indicator is turned on.[13] The expectation from the extant literature is that observed nonresponse should increase with the proportion of termed out legislators as the reins of accountability have been removed.

We also include three institutional characteristics of voting procedure. First, we include the chamber's allowance of a "present" vote, a vote that allows the legislator to signal their attendance—and therefore guard against accusations of shirking come campaign season—without formally contributing a "yea" or "nay" to the outcome; this is allowed in most chambers. The second voting procedure is "proxy" voting, where a legislator may designate another person in the chamber (typically a copartisan legislator, or party leader) to vote on his or her behalf; this is permitted, for example, in the Florida House. Finally, we include an indicator for "paired" voting, a procedure that allows a legislator to "pair" his or her vote to another member of the assembly and thus records the vote of the present legislator for both the present and the absent legislator. Typically, the absent legislator must file a "pair" form in advance indicating the specific motions on which one will pair and the legislator he or she will pair to; this is allowed, for example, in the Connecticut Senate.[14]

To these contextual factors, we add data on the legislators themselves. We incorporate data on the competitiveness of each district—the legislators' voteshare and number of candidates they faced in the previous election—as well as the district's magnitude (the number of candidates elected from each district) from state legislative election returns data (Klarner et al. 2013).[15] The total (logged) number of candidates enters the model as an implied interaction with a binary variable indicating that the legislator was opposed at all. Likewise, the (logged) district magnitude enters as an implied interaction with a binary variable indicating a multimember district. These implied interactions are important to bear in mind while looking over the results as logged number of candidates and logged district magnitude may only take on a value greater than 0 when the multimember district or opposed indicators are turned on. These data also allow us to differentiate experienced legislators from freshmen and identify the gender of the legislators.

In sum, we estimate the probability of nonresponse by regressing observed nonresponse on term length, the presence of term limits and the proportion of legislators termed out, voting procedures, majority status interacted with majority strength, district type (single member or multimember), district magnitude, the total number of candidates the legislator faced, the legislator's voteshare, binary variables indicating whether the legislator is experienced or a freshman and whether they are male or female, and our focal variables days in session, salary, and informational expenditures. Because our focal variables are on such dramatically different scales, they are rescaled to a standard normal distribution before estimation, which allows for easier interpretation and increases estimation efficiency. Finally, we include the (logged) dimensionality of the roll-call data (the ratio of variance explained by the first dimension to the variance explained by all subsequent dimensions taken from a decomposition of the roll-call matrix), measured at the chamber-session level. This serves as a measure of issue complexity, where higher values indicate more predictable voting, and therefore a less complex typical choice space.

Our dependent variable is the observed number of roll calls for which a given legislator in a particular session of a particular chamber failed to record a vote out of the total number of roll calls taken during that session. This is a binomial process where the total number of roll calls is the number of "trials" and the number of nonresponses is the number of "successes." Thus, each line of data represents a legislator's nonresponse record and, in the parlance of hierarchical modeling, that legislator is nested within a chamber, which is nested within a state, and those states and chambers are crossed with sessions. The typical modeling strategy for this data structure would be to estimate chamber effects, which are a nested subset of state effects, that are crossed with session effects. However, given that there are only two chambers and only two sessions, identifying these effects is exceptionally difficult and cannot be done efficiently. Our solution, then, is to assume that each session-chamber is a unit onto itself and estimate a single series of random intercepts for each of our 197 session-chambers. We believe that this approach is reasonable given that our independent variables of interest (all of our aggregate-level covariates, in fact) are measured at the session-chamber level, rather than the state level, and because there is a reasonable amount of variation across sessions, within chambers. We fit this hierarchical binomial model via Markov Chain Monte Carlo and report the posterior summaries in Table 1 along with a second model using the Squire Index rather than the three individual components for the sake of comparison—we note that, though the Squire Index is signed in the correct direction, the estimate is not robust.[16] While examining the results described in Table 1, recall that the dependent variable is the rate of nonresponse; therefore, positive parameters are associated with increased likelihood of nonresponse and negative parameters are associated with decreased likelihood of nonresponse.

We begin with the *need hypothesis* (nonresponse should decrease as legislative income increases) and viewing Table 1 reveals that the data bear no support for this hypothesis. The parameter is not signed in the predicted direction and the distribution of estimates for it is centered very nearly on zero with almost symmetric tails on either side. In other words, the data are telling us that there is no relationship between compensation and roll-call participation in our sample.

The data do, however, offer quite robust support for our *opportunity hypothesis* (nonresponse should decrease as days in session increase). A change from the sample mean (158 days per two-year session, roughly equivalent to the Iowa General Assembly) to 1 standard deviation above (260 days per two-year session, a few days greater than the South Carolina Legislature) reduces nonresponse probability by 0.021. Averaging across the sample (where the mean nonresponse rate is 0.085), this is a decrease over 25 percent—a robust reduction in substantive terms.[17] We plot this relationship over a large range of session length in Figure 1 for both majority (red) and minority members (blue) using a typical case of a legislator: an experienced male legislator, who won 55 percent of the vote in a two-candidate race for a single-member–district seat in a chamber where legislators serve two-year terms, without term limits, that does not permit present, proxy, or paired voting. We plot these effects holding majority seat share at 0.55 and our remaining focal variables (salary and expenditures) constant at their mean. As the figure shows, when session days near their lowest observed value, about fifty days over a two-year session, the typical legislator fails to vote on about 13 percent of roll calls. When session days approach the maximum, about five hundred days over a two-year session, that falls substantially to about 4 percent—a dramatic reduction in shirking and a marked improvement in representation.

Our last two hypotheses, regarding the provision of informational resources, are sharp directional tests—the

Table 1. Model Results.

	Estimates			
Covariate	*M*	*SD*	*M*	*SD*
Squire Index			−0.132	(0.167)
Salary	0.025	(0.159)		
Days in session	−0.492	(0.159)		
Expenditures	0.400	(0.160)		
Term limits	0.134	(0.579)	−0.019	(0.565)
Portion termed out	2.667	(2.404)	3.782	(2.417)
ln(Length of term)	0.100	(0.425)	0.269	(0.427)
Present vote	−0.036	(0.388)	−0.126	(0.396)
Proxy vote	−9.586	(0.471)	−9.550	(0.466)
Paired vote	−3.370	(0.157)	−3.398	(0.159)
Majority	−0.204	(0.005)	−0.203	(0.005)
Majority strength	0.461	(0.336)	0.327	(0.336)
Majority × Majority strength	0.181	(0.008)	0.178	(0.009)
Multimember district	−0.061	(0.444)	−0.047	(0.464)
ln(District magnitude)	−0.046	(0.032)	−0.015	(0.033)
ln(Total competition)	−0.113	(0.009)	−0.093	(0.008)
Opposed	0.103	(0.007)	0.134	(0.008)
Vote share	−0.024	(0.013)	0.108	(0.018)
Experienced	0.061	(0.003)	0.055	(0.004)
Male	−0.067	(0.003)	−0.066	(0.004)
ln(Low dimensionality)	0.022	(0.114)	−0.003	(0.120)
Intercept	−2.420	(0.623)	−2.597	(0.643)
Random effects Chamber (197) variance	3.708	(0.454)	3.970	(0.489)
N	14,418		14,418	
ln(likelihood)	−319,369		−312,492	

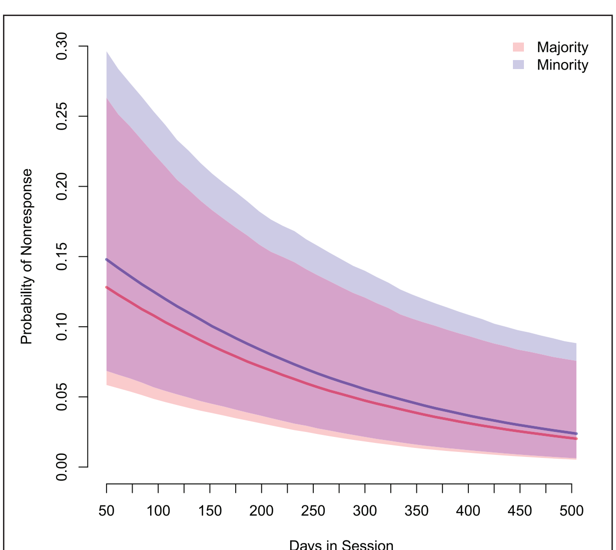

Figure 1. Effect of session length on nonresponse probability.

informed decision hypothesis predicts a negative relationship between informational resources and nonresponse, whereas the *informed abstention hypothesis* predicts a positive relationship between informational resources and nonresponse. The recovered relationship is positive and quite robust both statically and substantively. The data suggest that increasing chamber expenditures by 1 standard deviation from the mean (a substantive change from resources roughly equivalent to the Kentucky General Assembly to resources roughly equivalent to the Texas Legislature) increases nonresponse probability by 0.058, or—where the mean number of roll calls in a session is 478—about twenty-eight more instances of nonresponse, per legislator, per session.

This result comports with previous research investigating the agency dilemmas of legislators: there are times when the interests of party and constituency are opposed and legislators may choose to simply withdraw from voting to avoid disappointing one of their principals. Increased access to informational resources may help legislators map the policy effects of legislation to their constituents' preferences and therefore alert them to discord between what is best for the district and what the party wants. A similar learning mechanism was proposed by Maestas (2000) and Lax and Phillips (2012).

Table 2. Mitigating Effect of Present Vote on Informational Resources.

	Voting allowance	
	None	Present
Mean information	0.097	0.085
	(0.048, 0.169)	(0.049, 0.133)
High information	0.231	0.112
	(0.077, 0.460)	(0.059, 0.182)
Information effect	0.134	0.027
	(0.006, 0.331)	(0.001, 0.064)

Furthermore, if we interact expenditures and voting procedure, the data reveal that the positive effects of informational resources on nonresponse are mitigated by the allowance of a present vote. These differences are given in Table 2, which shows how increasing informational resources from the mean by 1 standard deviation influence nonresponse probability.[18] In the absence of a present vote, an increase in informational resources more than doubles nonresponse—an increase of nearly 140 percent. However, when a present vote is allowed, the effect is much more modest—about a 30 percent increase. Although the difference in differences across institutional contexts fails to reach typical levels of significance (p = .11)—in part, we believe, because the pervasiveness of the present vote allowance (less than 9% of our chambers do not allow a present vote) makes identifying the effects difficult—the results are nonetheless instructive and provide evidence of the mechanism at play: increasing informational resources alerts legislators to the potential discord between the preferences of their constituents and their party.

To summarize the primary findings, we find no evidence that compensation affects the probability of nonresponse, indicating that simply increasing legislative pay will not improve representation. However, we find robust evidence that curtailing the ability of legislators to build commitments outside of the legislature by increasing the number of days they are required to be in the capital does significantly decrease nonresponse. Finally, we find that improving the informational resources at the disposal of legislators only helps them to discover conflicts of interest between party and constituency and drives nonresponse upward.

Before moving on to our discussion and concluding remarks, we briefly consider how the other parameters in the model shape nonresponse, beginning with majority status and margins. Figure 2 plots the marginal effect of majority status on nonresponse probability for the same typical legislator described above (experienced, male, who won 55% of the vote in a two-candidate race, etc.), over the observed range of majority seat share (holding all other variables constant at their mean) with a 95

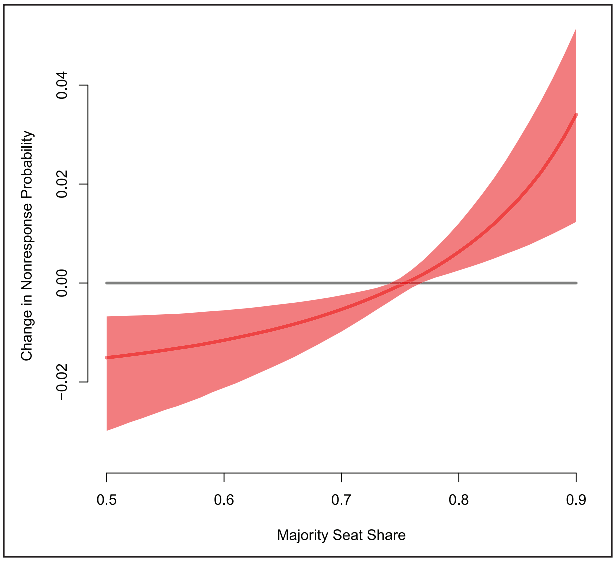

Figure 2. Marginal effect of majority status over majority seat share.

percent confidence interval. As the plot shows, when vote margins are tight, majority members are more likely to vote than their minority counterparts—about 0.015 more likely to vote when margins are at their slimmest. As margins grow, however, and the relative importance of each majority member's vote to bill passage decreases, majority members become substantially more likely to miss roll calls than their minority counterparts—they are nearly 0.035 less likely to vote when margins are at their widest. This supports the predictions of the conditional party government model of legislative organization (Aldrich and Battista 2002; Aldrich and Rohde 2000) and also suggests that our choice of control variables is properly accounting for party pressures on the decision to participate.

The effect of competition is similarly robust. For a single-member–district legislator, running against a single opponent, an increase in voteshare from 0.5 to 0.75 increases the probability of nonresponse by about 0.014. Likewise, experienced legislators are less likely to respond than their freshman counterparts, perhaps as a function of the electoral safety that comes with a longer tenure. Taken to together with the direct effects of competition, these results comport with the previous conclusions drawn by Hibbing (1986) and Rothenberg and Sanders (2000), who find strong increases to shirking once a legislator has set their eyes on other office or chosen to retire, respectively.

The model results also reveal a strong impact on voting procedure. Although the allowance of a present vote does not, in and of itself, reduce nonresponse, both proxy and paired voting exert powerful effects. Indeed, these are the strongest effects of any variable in the model, including our focal variables. Allowing a proxy vote decreases nonresponse by 0.089 and allowing a paired vote decreases nonresponse by 0.085 for our model legislator. Of course, this is reasonable as these two provisions, combined with a bit of foresight, could negate a substantial portion of nonresponse resulting from an inability to be in the chamber. Although, it is important to bear in mind that many chambers mandate that the proxy or pair be designated on the day of the vote in question and that these rules are therefore unlikely to make for widespread absenteeism.

Term limits exert a robust positive effect on nonresponse, conditioned on the portion of legislators being termed out. When 20 percent or more legislators are termed out, aggregate nonresponse rates increase between 5 and 15 percent. Of course, this comports with previous research on the effect of term limits on nonresponse in particular (Clark and Williams 2013; Wright 2007) and on the effects of electoral accountability more generally (Hibbing 1986).

Finally, we find an unanticipated large and robust negative effect on nonresponse for males as compared with their female counterparts. We can think of two possible explanations for this effect, but more research is needed to discover the true cause. First, it is possible that women may be burdened with more familial responsibility than their male counterparts, perhaps a product of an asymmetric division of caretaker responsibilities for women with partners, or a higher proportion of primary caretakers among women without partners as compared with their single male counterparts (see Sanchez and Thomson 1997), and this manifests in differing attendance patterns. Second, this result may have roots in the well-documented propensity for women to be substantially more likely to respond "I don't know" than their male counterparts when they are uncertain of the correct response (Mondak and Anderson 2004). We can get a feel whether or not this explanation holds water by estimating how gender effects are conditioned by the ability to vote present. We estimate that roughly 60 percent of the gender difference is negated by this voting allowance, but caution readers to take this figure with a grain of salt because, as we noted above, there is little variation on the allowance of a present vote and explaining these differences are not our focus here.

Our findings regarding the role of gender and experience are novel to the American literature, and discovery of the effects of competition and voting procedure are novel to the literature as a whole, though unsurprising. For our central focus here, though, the sensibility of the recovered estimates on the control variables is salient to our design as they suggest that our model is well specified. That is, if the model were to find that term-limited legislators were substantially *less likely* to shirk and that majority members become less likely to vote as majority margins tighten—two results that would contradict accepted theoretical predictions—we may conclude that the model was suffering from omitted variable bias or some other form of misspecification. This is not the case. Furthermore, we find that the evidence that more experienced legislators, who are presumably better informed, are more likely to shirk provides further support for our findings regarding informational resources. Indeed, if one interacts experience with informational resources, the interaction reveals a modest, but robust positive interaction effect, suggesting that the effects of informational resources are more pronounced for experienced, rather than inexperienced legislators, which seems intuitive to us.

Discussion

There is a substantial variation in aggregate patterns of nonresponse across the legislative chambers of the United States. Indeed, observed rates of nonresponse range from an effective 0 in the Delaware Senate to over 28 percent in the Alabama House. In this manuscript, we took an important first step toward explaining this variation by analyzing the effect of important institutional parameters on legislative nonresponse. Our analyses reveal that short legislative sessions drive substantial increases in nonresponse rates and argue that the underlying cause is the freedom short sessions create for legislators to commit to more demanding occupations outside of the legislature, which, in turn, make it more difficult for them to be present in the chamber when the roll is called. Taken together with our finding that salary does not incentivize members to vote more often, the data suggest that financial opportunity, but not financial need, drive nonresponse. We believe that this comports with Fiorina's (1994, 1997) arguments that different legislatures attract different candidate types conditioned on the obligations of office. Furthermore, this result suggests that a substantial portion of nonresponse is likely due to legislators simply deciding not show up in the chamber, that is, holding voting procedures and informational resources (and therefore, presumably, the legislator's ability to choose the correct vote for their party and constituents) constant, there should be little impact of days in session on a legislator's choice to vote, *given* that they are in the chamber, but a substantial impact of days in session on a legislator's choice to be present in the chamber. The fewer days in session, the larger the non-legislative responsibilities representatives may commit to, and therefore the greater the competing demands on their time. Thus, the data suggest that a substantial challenge to representation may lie in compelling representatives to make the journey to the capital, rather than compelling them to respond when the roll is called.

Our analysis also revealed a strong impact of informational resources on nonresponse. Our estimates present a compelling case that members with greater informational resources are less likely to participate in roll-call votes. This suggests, on average, that informational resources serve to alert legislators of discordance between their constituency's preference and their party's preference, more often than they serve to aid legislators in discovering the best position to take—the evidence for this mechanism is buttressed by the attenuating effect of a present vote on information resources. Note that both mechanisms involve learning and therefore both correspond to previous arguments (e.g., Lax and Phillips 2012; Maestas 2000), but the discovery that informational resources contribute to nonresponse, rather than ameliorating it, adds nuance to our understanding of how information plays into legislative voting as well as our understanding of the struggle between competing principals in representative democracy. This directional difference in the effects of components of the Squire Index on nonresponse reinforces previous arguments that there are theoretical contexts in which indices should be analyzed as a single unit and theoretical contexts in which indices should be disaggregated (e.g., Bowen and Greene 2014).

Our findings on expenditures reveal a paradoxical relationship between informational resources and democratic responsiveness. As Maestas (2000) and Lax and Phillips (2012) have argued and supported empirically, increasing the informational resources available to legislators substantially increases the congruence between real policy outcomes and voter preferences. Normatively, this is a very desirable outcome. However, our findings here suggest that the same increase in informational resources significantly lowers the probability of roll-call participation, meaning that fewer voters are being adequately represented in the policy-making process. Is sacrificing representative quality in the policy-making processing worth a more congruent set of outcomes? Is it possible that the nature of political geography in the American states is such that decreasing effective representation (by discouraging legislators for whom constituent and party preferences are in conflict) is a necessary condition for improving policy congruence? More research on which legislators are choosing to not to vote and when is needed to answer these important questions.

Finally, the evaluation of our hypotheses led us to corroborate previous findings and discover several new, robust correlates to nonresponse. The data suggest that term-limited legislators are more prone to nonresponse and that there is a conditional relationship between majority status and nonresponse: members of the majority are more likely to vote than their minority counterparts when margins are tight, but become increasingly less likely to vote as their partisan advantage widens. The data also revealed that female legislators, more experienced legislators, and legislators facing less competition are substantially less likely to vote, all else equal. More substantively significant than any of these factors, however, is the role of voting procedure. The ability to designate a proxy or pair reduces nonresponse more than any other covariate in the model.

On a normative level, our analyses bear important implications for institutional design. Fiorina (1994) argued that, as a result of the professionalization of state legislatures in the years after the Second World War, the chambers were attracting different types of candidates—candidates who were willing to meet the increased time commitments of these professionalizing bodies. Beginning in the 1990s, however, that trend has slowed or even reversed with the mean score of the Squire Index falling from its high of 0.22 in 1986 to 0.18 in 2003 (Squire 2007). The analyses that we present here suggest that this decline will not only hamper the ability of state legislatures to convert the preferences of their constituents into policy outcomes, as the literature has already found, but may also fundamentally decrease the quality of representation citizens receive. As states decrease the legislative workload placed on legislators and legislators, in turn, are more able to pursue alternative income streams, legislators become substantially more apt *not* to participate in roll-call votes and therefore leave their district unrepresented.

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Notes

1. We write "approximately ten thousand legislators" because we cannot be certain of the true number due to idiosyncrasies in spelling across sessions, legislators switching chambers, and the high frequency of very common names (i.e., the name *Smith* occurs eighty-nine times). As such, ten thousand is a best guess. We can be certain, however, that the true number is over 7,500 but under thirteen thousand.

Also, the second session of the Nevada Senate was lost due to missingness.
2. Of course, this Democratic advantage is no longer apparent in the composition of state legislatures.
3. A previous reader of this manuscript notes that, holding the number of days constant, the arrangement of those days in session may make pursuing non-legislative employment more or less difficult. For example, a forty-day (over two years) legislative session where all days are held in July and August of the odd year creates a different kind of time obligation than the same forty days scheduled as one day every other week. However, the investigation of session structure shows that there is little variation across states. The assemblies overwhelmingly cluster their legislative days into one or two compact meeting schedules.
4. For example, the California Legislature has an array of formally organized caucuses including, but not limited to Black, Rural, LGBT, Technology and Innovation, and Mental Health.
5. This measurement approach does not capture informational resources that do not have to be paid for, for instance, the power of a committee to compel departmental testimony. We attempt to account for these potential unmeasured sources of influence on nonresponse through the estimation of error components, which we discuss below.
6. To say nothing of their own personal preferences and the pressures of lobbying groups, and so forth. In the interest of simplicity, however, we discuss only party and constituency here.
7. Of course, parties have an institutional presence in the chamber that constituents lack and we may therefore suspect that legislators in low-information environments are more likely to know the party position than their constituency's position.
8. We lose one session of the Nevada Senate due to missing data. The session years for Arkansas are year greater than the rest of the states (i.e., 2000–2001 rather than 1999–2000). For more information on these data, see Wright (2004) and Clark et al. (2009).
9. There are a few third-party or nonpartisan members scattered through our data—we place them in the "minority" category. Omitting them from the analysis does not substantively change the results.
10. Of course, the empirical focus of most conditional party government (CPG) tests is on the direction of votes, rather than the action of voting itself. However, member absence or abstention, though not as harmful as defection, still inhibits parties from achieving their goals. Thus, the logic of CPG should apply to nonresponse.
11. Using the logged majority/minority ratio, rather than majority seat share, improves estimation efficiency substantially.
12. As most readers know, the Nebraska Unicameral is technically a nonpartisan chamber. Even so, nearly all legislators have party affiliations and these affiliations are noted in the original data. We therefore treat this chamber as we treat all the others. Readers concerned by this choice will be relieved to know that we estimate chamber-level error components below and that our substantive results do not change if we drop Nebraska from the analysis.
13. Ideally, we would be able to account for whether each individual member is affected by term limits and when. Unfortunately, the data do not allow for this and collecting this information on over fourteen thousand session-legislators is beyond the scope of our study.
14. We are very grateful to an anonymous reviewer for suggesting that we consider the role of these voting procedures.
15. Eleven chambers used multimember districts, ranging from two to eleven in magnitude, at the time of our sample. Furthermore, the voteshare that we include in the models is the legislator's total voteshare weighted by the district magnitude.
16. We allow for diffuse priors of 0 parameters with certainty equivalent to twice the stand deviation of the dependent variable—the default values of the software we use to fit the model (Stan Development Team 2016).
17. This probability change is robust at the $p < .001$ level.
18. The full model results are given in the online appendix. The inclusion of the interaction does not significantly change the estimates on the focal covariates.

Supplemental Material

Replication data for this article are available at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/ZPVLVH.

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