

Drawing Your Senator From a Jar:  
Term Length and Legislative Behavior\*

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## **Abstract**

Estimating the effect of term length on legislative behavior poses extraordinary methodological challenges. Previous work on this area has overwhelmingly focused on the U.S. Senate, relying on its long terms and staggered structure to achieve identification. This paper formalizes the implicit assumptions required for these designs to provide reliable causal inferences, which turn out to be strong and unlikely to be satisfied in many applications. Exploiting a unique randomized field experiment that takes place in the state senates of Arkansas and Texas after reapportionment, I provide the first experimental estimates of the causal effect of the duration of terms on the behavior of legislators in the United States. This design overcomes the identification challenges faced by the extant literature. Results show that senators serving shorter terms have higher abstention rates and introduce a significantly lower number of bills during the legislative session, suggesting that electoral pressures may pose an incentive to both avoid position-taking and reallocate resources towards campaign activities.

**Keywords:** Term length, legislative behavior, randomization inference, causal inference.

## 1 Introduction

A central concern of the Framers of the American Constitution was to design representative institutions that would protect the people against the possible degeneracy of their leaders. Granting the people the right to frequently elect their representatives was seen as an essential step towards achieving this goal (*Federalist* 52).

The decision on the frequency of elections was guided by the belief that the shorter the duration of a given representative's appointment, the greater it would be his dependence on the people he represented (*Federalist* 57,63,71). However, the Framers had an opposing concern arising from at least three considerations. First, occasions may arise in which the electorate calls for measures which contradict their own interest (*Federalist* 63,71). Second, certain matters of government have long-term effects and representatives must face the appropriate incentives to engage in them (*Federalist* 63,71). Finally, certain government tasks require significant expertise and information, and representatives must be given the opportunity to acquire those while in office (*Federalist* 64, 53). These considerations suggested that representatives should serve for a long enough term so that they would have the incentives and ability to pursue complex, long-term and unpopular policies when required.

The tenure by which the representatives were to hold their offices was thus carefully designed to serve these two conflicting purposes at once, and the adoption of different term lengths in the two legislative chambers reflected, in part, a compromise between these opposing principles. Whether term lengths operate in the way the Framers intended is ultimately an empirical question, but one that poses extraordinary methodological challenges. When legislators are elected to serve a given period of time for a given office they exhibit certain behavior, but in order to learn whether the observed behavior is being affected by the duration of the term for which they were elected, one would have to know how legislators *would have behaved* if their term length had been shorter or longer. This is of course extremely difficult, since this counterfactual behavior is never observed.

In this paper, I use the gold standard solution to this fundamental problem of causal inference (Holland 1986), namely, the *random assignment* of term lengths. Exploiting a unique randomized field experiment that takes place in the state senates of Arkansas and Texas, I provide the first experimental estimate of the causal effect of the duration of terms on the behavior of legislators in

the United States. In these states, after the elections immediately following decennial reapportionments, senators are randomly assigned to different groups or “classes” according to whether they would serve four years or two years in the legislative period immediately following the enactment of new districts. Between censuses, state senators serve for a period of four years and are staggered so that one half of senate seats are up for election every two years; but in the election following reapportionment, all senate seats are up for election to ensure that senators are representative of their possibly new constituencies. The staggered structure of terms is thus broken, and the procedure by which it is reestablished is random assignment.

I compiled a rich dataset of demographic, socioeconomic and electoral characteristics at the individual-senator level and the state-senate-district level, and use it to verify that the randomization was successfully implemented. As expected, the distribution of observed covariates is found to be statistically indistinguishable for senators and districts in both groups.

This randomized field experiment provides a unique opportunity to contrast alternative hypotheses concerning the behavior of senators serving different term lengths, and thus analyze in detail the incentives introduced by elections. In particular, I study to what extent the proximity of elections changes senators’ incentives to seek or avoid position-taking, as measured by their abstention rates. In addition, I study whether term length affects legislative productivity, as measured by bill sponsorship during the legislative session. Shortterm terms generate increased abstention rates in Texas and a lower rate of bill sponsorship in Arkansas. Taken together, these results suggest that electoral pressures may induce legislators to engage less in formal legislative activities.

The random assignment of senators to two-year or four-year terms provides a unique opportunity to obtain unbiased empirical evidence, because it ensures that senators in one group are a valid *counterfactual* for senators in the other group. However, the random assignment of term lengths in and of itself is not enough to guarantee a given *interpretation* of the causal parameters identified. In order to interpret these parameters, one needs to rely on a theory of how individual senators behave and interact in the legislature. As discussed in detail in Section 4, theoretical results by Muthoo and Shepsle (2006) suggest that the experiment might be “contaminated” with senators’ intertemporal strategic behavior, which would radically alter the interpretation of the results by inducing a violation of SUTVA, the Stable Unit Treatment Value Assumption.

The rest of the paper is organized as follows. Section 2 presents a review of the existing literature

on the effect of the temporal proximity of elections on legislative behavior. Section 3 presents a rigorous methodological discussion of the identifying assumptions (implicitly) used in previous work and relates them to the conditions provided by the experimental design used in this paper. Section 4 discusses the theoretical results of Muthoo and Shepsle (2006) and Shepsle, Van Houweling, and Dickson (2004) and their implications for the interpretation of the results. Section 5 describes the institutional details surrounding the random assignment of term lengths in Arkansas and Texas, on which the empirical analysis is based, and Section 6 discusses the randomization inference model, on which all statistical inferences are based. The main empirical findings are presented in Section 7. Finally, Section 8 presents some concluding remarks and outlines how this project will be continued.

## 2 Literature Review

Previous scholars of American politics have analyzed the effect of term length on legislative behavior using observational studies, mainly using designs in which the proximity of elections was different over time and/or across legislators<sup>1</sup> (e.g. Amacher and Boyes 1978; Ahuja 1994; Bernhard and Sala 2006; Bernstein 1991; Elling 1982; Jackson 1974; Levitt 1996; Thomas 1985; Wood and Andersson 1998; Wright and Berkman 1986). This literature has overwhelmingly focused on the U.S. Senate<sup>2</sup>, where the long terms and the staggered structure of elections were considered attractive methodological features. It was argued that long terms are attractive because if one expects legislative behavior to change as a function of the proximity of elections, one should consider offices where terms are long enough to induce cyclical behavior (Ahuja 1994; Bernstein 1991; Elling 1982). Furthermore, the staggered structure of elections was seen as spontaneously providing comparison groups, since one can always find senators with different remaining time in office serving in the same Congress (Amacher and Boyes 1978; Thomas 1991; Wright and Berkman 1986).

Inferences that have relied on long terms and staggered classes have been crucial for our understanding of the problem and formulation of the relevant hypotheses. But, as formalized below, the assumptions required for these designs to provide reliable causal inferences are strong and not guaranteed to be satisfied.

A related literature has studied whether legislators engage in opportunistic behavior or *shirking*

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<sup>1</sup>Gordon and Huber (2007) studied this question for trial judges.

<sup>2</sup>One exception is Kuklinsky (1978), who studied California's state senate.

in their last term in office (e.g. Davis and Porter 1989; Kalt and Zupan 1990; Lott 1990, 1987; Vanbeek 1991; Rothenberg and Sanders 2000; Zupan 1990). This question is undoubtedly related to the effect of term length on legislator behavior, since its main focus is to detect whether the threat of reelection prevents certain kinds of behavior. If it were found that legislators were sensitive to the temporal proximity of elections during the course of their careers, then one would expect that the absence of electoral threats altogether would have a significant impact on last-period behavior. Though related, both questions are distinct, and the experiment considered in this paper speaks only indirectly to the question of last-period shirking.

With the exception of Jackson (1974), who studied the effect of election proximity on several aspects of U.S. senators's legislative behavior<sup>3</sup>, the vast majority of the literature has focused only on responsiveness and is based on spatial arguments of moderation and convergence. One group looks at the impact of election proximity on the responsiveness of legislator's ideology to constituency preferences (Bernstein 1991; Glazer and Robbins 1985; Kuklinsky 1978; Levitt 1996), where measures of responsiveness range from fitted residuals of a regression of senators' conservative scores on constituency characteristics (Bernstein 1991) to deviation of senators' scores from the average score of US House delegations (Amacher and Boyes 1978; Levitt 1996), through the deviation of senators' scores from the scores of previously elected senators from the same state (Glazer and Robbins 1985). The hypothesis tested in these studies is that the longer the remaining time in office, the less responsive representatives are to the desires of the polity. In general, these studies show that their proposed measures of responsiveness increase as elections come near.

Another group ignores constituency preferences (and thus avoids significant measurement problems), and simply focuses on the effects of election proximity on senator ideology (Ahuja 1994; Bernhard and Sala 2006; Elling 1982; Thomas 1985; Wright and Berkman 1986). The hypothesis of these scholars is that as elections approach, senators become more ideologically moderate. In these studies, the evidence suggests that senators moderate their ideology as elections approach, although in some congresses the ideological shift goes in the opposite direction (see, for example, Elling 1982). Moreover, there is disagreement as to whether moderation should be expected; as some authors correctly mention, the hypothesis of moderation rests in an oversimplified version of

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<sup>3</sup>Jackson (1974) studied effects on responsiveness to constituency positions, the party leadership, senior committee partners, and the president.

the spatial analysis model which need not be true in the elections under analysis (Bernstein 1988).

Although most of the literature studies the effects on strategic moderation and responsiveness, a few authors consider different hypotheses. For example, Thomas (1991) finds that US senators up for reelection have a significantly higher rate of nonvoting and some of his evidence suggests that avoidance of position-taking increases during the last year in office relative to the previous five. In the context of Latin American politics, a recent contribution by Dal Bo and Rossi (2008) studies the effect of term length on several measures of legislative participation in Argentina, where term lengths were randomly assigned at the level of legislator groups.

### 3 The methodological problem

As mentioned above, previous empirical attempts to analyze the effect of remaining time in office on legislative behavior have overwhelmingly focused on the U.S. Senate. The cited reasons are mainly two. First, the longer terms served by senators as compared to House members makes it more likely that differences in behavior will arise at different points in their terms. Second, the staggered structure of the U.S. Senate guarantees that in any given Congress there coexist senators with different time horizons.

Many scholars saw these two features of the U.S. Senate as providing an ideal setting to test their hypotheses, since it seemed to allow for the analysis of *both* the behavior of a given senator at different points in his term and the behavior of different senators facing different time horizons at the same point in time.<sup>4</sup> The institutional structure of the U.S. Senate was thus treated as a sort of “natural experiment” that provided researchers with different sources of variation that could be exploited to answer fundamental questions about term length and legislative behavior.

Some early authors, however, recognized that calling these strategies ideal might have been an overstatement. After studying the change in the behavior of senators between the first and last year of their terms for several classes elected at different points in time, Elling (1982) recognized that this approach was not without limitations:

*It might be objected that the shifting of each class is simply a result of more general forces that*

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<sup>4</sup>Some studies chose the first strategy (e.g. Elling 1982; Kuklinsky 1978; Bernhard and Sala 2006), some chose the second strategy (e.g. Amacher and Boyes 1978; Ahuja 1994; Wright and Berkman 1986), and others used some combination of both (e.g. Bernstein 1988, 1991; Wood and Andersson 1998).

*affected all senators whether or not they were up for reelection. This possibility cannot be completely dismissed since it is impossible to come up with what is, strictly speaking, a control group. Ideally what is required is a group of senators serving coterminously with senators in a particular class but who do not need to stand for reelection. However imperfect, the “control” group is all senators not in a particular class in a particular year, despite the fact that the composition of this group changes from year to year while class composition is essentially unchanged over six years (Elling, 1982).*

What Eling recognized was effectively a missing data problem: the ideal control group with which one would want to compare a given group of senators is just unavailable given the characteristics of the design. In the remainder of this section, I formally discuss the assumptions that are needed for these designs to answer the questions of interest.

In what follows, I use the Rubin Causal Model and distinguish between the *potential* and *observed* outcomes of each unit (Holland 1986; Rubin 1974, 1978). Define a treatment indicator  $T$  as follows:

$$T_{it} = \begin{cases} 1 & \text{if, at } t, \text{ reelection is 2 years away for senator } i \\ 0 & \text{if, at } t, \text{ reelection is 4 years away for senator } i \end{cases} \quad (1)$$

Let  $Y_{it}^1$  be the potential outcome of interest for senator  $i$  in period  $t$  when  $T_{it} = 1$ , and  $Y_{it}^0$  be the potential outcome for senator  $i$  in period  $t$  when  $T_{it} = 0$ . Also, for a senator elected in period  $t$  who is running for reelection at the end of  $t + 6$ , let  $t + 6$  refer to the fifth and sixth years of his term,  $t + 4$  to the third and fourth years of his term, and  $t + 2$  refer to the first and second years. In other words,  $t + 6$ ,  $t + 4$  and  $t + 2$  refer to the three consecutive two-year periods of senator’s  $i$  six-year term. Finally, let the random variable  $C$  indicate the year when senators were elected, with  $C_i \in \{\tau, \tau + 2, \tau + 4\}$  and  $\tau$  indexing an arbitrary time period. Thus,  $C_i$  indicates the “class” to which any senator  $i$  belongs.

The variables  $(Y_{i0}, Y_{i1})$  are referred to as *potential outcomes* because they refer to the outcomes that a unit *could* have if assigned to each of the treatment regimes. Note that this definition of the potential outcomes makes an implicit assumption, namely that each unit  $i$  has only two potential outcomes depending on which treatment status unit  $i$  actually receives. Thus, the potential

outcomes of any unit are defined independently of the treatment status of all other units. This is the Stable Unit Treatment Value Assumption (SUTVA) (see Rubin 1986, 1990), which is needed for the causal effect of term length on legislative behavior to be properly defined. As discussed extensively in Section 4, this assumption can be considerably restrictive in chambers where terms are staggered, but will be assumed in this section in order to define the parameters of interest.

Given the notation defined above and assuming SUTVA, the population parameter we are interested in estimating is

$$\lambda_t \equiv \mathbb{E}(Y_{it}^1 | C_i = t) - \mathbb{E}(Y_{it}^0 | C_i = t) \quad (2)$$

This is, at time  $t$  one wants to compare the behavior of legislator  $i$  when he is elected at election  $t$  and elections are two years away, to the behavior of legislator  $i$  when he is elected at election  $t$  and elections are four years away.<sup>5</sup> Note that we are requiring that the counterfactual be elected at the same time, but for a different *period* of time. It is clear, though, that for any senator  $i$  elected at time  $t$ , he is either running for reelection at  $t + 2$  *or* he is running for reelection at  $t + 4$ . In other words, at any period  $t$  and for any senator  $i$ , elections are either two years away *or* four years away, but never both. This is the well known fundamental problem of causal inference. The *observed* outcome is therefore

$$Y_{it} = Y_{it}^1 \cdot T_{it} + (1 - T_{it}) \cdot Y_{it}^0 \quad (3)$$

This means that the parameter of interest,  $\lambda_t$ , cannot be recovered from the observed outcomes without further assumptions. As mentioned above, recognizing this missing data problem, previous work adopted mainly one of two strategies.

### 3.1 Strategy one: senators in different classes at same point in time

The first strategy compares the behavior of senators who belong to different classes in the same period of time. Assuming that senator  $i$  belongs to class  $C_i = t$  and senator  $j$  belongs to class

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<sup>5</sup>In what follows, I will concentrate on senators whose reelection is four years away and two years away. I will ignore senators whose reelection is six years away only for clarity purposes, so that the treatment indicator can be defined as binary. Adding the third possibility does not change any of the conclusions in this section.

$C_j = t + 2$ , this strategy compares senator  $i$  at  $t + 4$  to senator  $j$  at  $t + 4$ . Since at  $t + 4$  senator  $i$ 's reelection is only two years away while senator  $j$ 's reelection is four years away, this strategy was designed to capture the difference in behavior that is caused by the temporal proximity of elections.

The parameter that this strategy estimates is therefore:

$$\mu_t \equiv \mathbb{E}(Y_{it}^1 | C_i = t - 4) - \mathbb{E}(Y_{it}^0 | C_i = t - 2) \quad (4)$$

Note this strategy has no missing data problem, since<sup>6</sup>

$$\mathbb{E}(Y_{it}^1 | C_i = t - 4) = \mathbb{E}(Y_{it} | T_{it} = 1, C_i = t - 4) \quad (5)$$

$$\mathbb{E}(Y_{it}^0 | C_i = t - 2) = \mathbb{E}(Y_{it} | T_{it} = 0, C_i = t - 2)$$

and therefore the parameter of interest  $\mu_t$  can be written solely in terms of observable variables:

$$\mu_t = \mathbb{E}(Y_{it} | T_{it} = 1, C_i = t - 4) - \mathbb{E}(Y_{it} | T_{it} = 0, C_i = t - 2) \quad (6)$$

### 3.2 Strategy two: senators in same class at different points in time

The second strategy is to compare senator  $i$ 's behavior at  $t - 2$  with senator  $i$ 's behavior at  $t - 4$ , this is, to compare senator  $i$ 's behavior at different moments of this term. Thus, the parameter that this strategy estimates is defined by<sup>7</sup>

$$\gamma_t \equiv \mathbb{E}(Y_{it}^1 | C_i = t - 4) - \mathbb{E}(Y_{it-2}^0 | C_i = t - 4) \quad (7)$$

Again, as in the case of the parameter  $\mu_t$ , the parameter  $\gamma_t$  involves no missing data problem since<sup>8</sup>

$$\mathbb{E}(Y_{it}^1 | C_i = t - 4) = \mathbb{E}(Y_{it} | T_{it} = 1, C_i = t - 4) \quad (8)$$

$$\mathbb{E}(Y_{it-2}^0 | C_i = t - 4) = \mathbb{E}(Y_{it-2} | T_{it-2} = 0, C_i = t - 4)$$

<sup>6</sup>Conditions (5) follow from the fact that for any senator  $i$ , when  $C_i = t - 4$  we have  $T_{it} = 1$  and hence  $Y_{it}^1 = Y_{it}$ . Similarly, when  $C_i = t - 2$  we have  $T_{it} = 0$  and hence  $Y_{it}^0 = Y_{it}$ .

<sup>7</sup>Equation (11) could of course be defined for any  $C_i$ . No results depend on conditioning on  $C_i = t - 4$  in particular.

<sup>8</sup>Conditions (8) follow from the fact that for any senator  $i$ , when  $C_i = t - 4$  we have  $T_{it} = 1$  and  $T_{it-2} = 0$ ; therefore,  $Y_{it}^1 = Y_{it}$  and  $Y_{it-2}^0 = Y_{it-2}$ .

It follows that the parameter  $\gamma_t$  is identified, since it can be written in terms of observable variables

$$\gamma_t = \mathbb{E}(Y_{it} | T_{it} = 1, C_i = t - 4) - \mathbb{E}(Y_{it-2} | T_{it-2} = 0, C_i = t - 4) \quad (9)$$

### 3.3 Comparing both strategies

The two strategies outlined above seek to identify the impact of the temporal proximity to elections on a senator's legislative behavior. Equation (6) makes clear that in the first strategy, when analyzing the behavior at time  $t$  of those senators who are elected at  $t - 4$ , the counterfactual used are those senators elected a period later, at  $t - 2$ , and who are hence four years away from reelection at  $t$ . It is clear that this strategy exploits the staggered structure of the senate, using senators in one class as counterfactuals for senators in another.

On the other hand, equation (9) shows that in the second strategy, when analyzing the behavior at time  $t$  of those senators who are elected at  $t - 4$ , the counterfactual used are those *same* senators a period before. This strategy exploits the length of the senators terms and not the staggered structure of the senate, and uses as counterfactual the behavior at a previous point in time.

It follows from equations (6) and (9) that these two strategies estimate two parameters that in general will be different. A necessary and sufficient condition for  $\mu_t$  and  $\gamma_t$  to be equal is

$$\mathbb{E}(Y_{it-2}^0 | C_i = t - 4) = \mathbb{E}(Y_{it}^0 | C_i = t - 2) \quad (10)$$

This is, if senators elected at  $t - 4$  behave in period  $t - 2$  in the same way as senators elected in period  $t - 2$  behave in  $t$ , then both strategies will be estimating the same parameter. Clearly, this is a strong assumption that may not be satisfied in many cases, and hence in general we will have  $\mu_t \neq \gamma_t$ .

But if generally  $\mu_t \neq \gamma_t$ , which of these two parameters captures the effect of term length on legislative behavior? Is one strategy better than the other? The first strategy has the advantage of comparing senators at the same point in time, which implies that any time shock will affect both types of senators in the same way. The problem of this strategy is that it compares senators who were elected at *different* elections, and hence may have very different (unobserved) characteristics.

The second strategy avoids this problem by considering, at different points in time, the behavior of senators who are all elected in the same election. The disadvantage of this second strategy is that it does not have the advantage of the first one: by comparing the behavior of senators at different points in time, it may capture not only the effect of the temporal proximity elections but also the effect of time-varying confounding factors.

We can see this more formally by noting that the parameter of interest is  $\lambda_t$ , and in general  $\lambda_t \neq \mu_t$  and  $\lambda_t \neq \gamma_t$ . In fact,  $\mu_t$  and  $\gamma_t$  were solely proposed to overcome the missing data problem, which implies that  $\lambda_t$  is not generally identified. But without further assumptions,  $\mu_t$  and  $\gamma_t$  do not capture the effect of the temporal proximity of elections on senators' behavior. In what follows, I consider the conditions under which the parameter  $\lambda_t$  is identified, and discuss how SUTVA restricts the set of institutional arrangements that can be allowed among legislators.<sup>9</sup>

### 3.4 Strategy three: random assignment

Consider what would happen if we randomly assigned the length of terms at period  $t$ . More precisely, assume that at time  $t$  *all senate seats* are up for election, and at the moment of the election voters ignore the term length which will be assigned to the senator representing their district. Assume further that after the election, senators are randomly assigned to serving either two-year terms or four-year terms.

If the randomization is successful, we would have (for each period  $t$ )

$$Y_{it}^1, Y_{it}^0 \perp\!\!\!\perp T_{it} \mid C_i \tag{12}$$

In other words, a successful randomization guarantees that potential outcomes be independent of

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<sup>9</sup>Combining both strategies would entail adopting a differences-in-difference strategy, whose identification identification assumption would require that trends would have been identical between both groups compared. If senators who are not up for reelection in  $t+2$  behave between  $t-2$  and  $t$  in the same way in which senators up for reelection at  $t+2$  would have behaved between  $t-2$  and  $t$  had they not been up for reelection, then using the diff-in-diff estimator would “remove” the time effect from  $\gamma_t$  and now the new parameter,  $\alpha_t$ , would capture *only* the change of behavior due to elections being closer.

$$\alpha_t \equiv \{\mathbb{E}(Y_{it}^1 \mid C_i = t-4) - \mathbb{E}(Y_{it-2}^0 \mid C_i = t-4)\} - \{\mathbb{E}(Y_{it}^0 \mid C_i = t-2) - \mathbb{E}(Y_{it-2}^{0'} \mid C_i = t-2)\} \tag{11}$$

Requiring that these trends be equal is a strong assumption, since under this differences-in-difference design the control group is still composed of senators who were elected in a *different* election and are likely to have different characteristics that will not guarantee that these trends be equal.

the assigned term length.

Condition (12) implies

$$\begin{aligned} \mathbb{E}(Y_{it} | T_{it} = 1, C_i = t) &= \mathbb{E}(Y_{it}^1 | T_{it} = 1, C_i = t) = \mathbb{E}(Y_{it}^1 | T_{it} = 0, C_i = t) = \mathbb{E}(Y_{it}^1 | C_i = t) \\ \mathbb{E}(Y_{it} | T_{it} = 0, C_i = t) &= \mathbb{E}(Y_{it}^0 | T_{it} = 0, C_i = t) = \mathbb{E}(Y_{it}^0 | T_{it} = 1, C_i = t) = \mathbb{E}(Y_{it}^0 | C_i = t) \end{aligned} \quad (13)$$

In words, under this model randomization ensures that the behavior of senators who at time  $t$  are four years away from reelection is on average the same behavior that senators two years away from reelection would have exhibited in period  $t$  had elections been four years away instead of two.<sup>10</sup> Senators assigned a four-year term are a valid counterfactual for those senators whose legislative terms last two years and therefore a comparison of the behavior of both types of senators in the two years following the election at  $t$  provides a valid causal estimate of the effect of term length on senators' behavior.

Formally, conditions (13) imply

$$\lambda_t = \mathbb{E}(Y_{it} | T_{it} = 1, C_i = t) - \mathbb{E}(Y_{it} | T_{it} = 0, C_i = t) \quad (14)$$

and hence the parameter  $\lambda_t$  is identified.

This paper exploits the random assignment of term lengths that occurs in the State Senates of Arkansas and Texas (described in detail Section 5) to estimate the causal effect of term length on senators' behavior. In these states, senators are randomly assigned to serve a two-year or a four-year term after redistricting, and hence the conditions are just as described in this section. Thus, it follows from the discussion above that the randomization in these states will ensure condition (12) for all  $t$  immediately following reapportionment, and hence the parameter  $\lambda_t$  will be identified. At first glance, it seems that the randomization of term lengths gives us the opportunity of estimating the causal effect of term length on legislative behavior, and thus of testing the assumption made by the Founding Fathers that the frequency of elections affects the behavior of representatives while in office.

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<sup>10</sup>In fact, randomization that the entire distributions of behavior are the same, not only their first moments.

## 4 The limitations imposed by strategic interactions

The potential outcomes framework presented above appears to have made no assumptions regarding the behavior of legislators. However, a more careful consideration of SUTVA reveals that this framework makes *implicit* behavioral assumptions. In this section, I consider whether these assumptions are likely to hold, in light of theoretical results developed by Muthoo and Shepsle (2006) and Shepsle et al. (2004). As shown in detail below, if senators engaged in strategic intertemporal deals with each other, SUTVA would be violated, and the parameter  $\lambda$  would not be identified even with random assignment of term lengths.

Let  $\mathbf{T} = (T_1, T_2, \dots, T_N)$  and  $\mathbf{T}' = (T'_1, T'_2, \dots, T'_N)$  be two  $N$ -dimensional vectors that contain the treatment assignment of each of the  $N$  units in the experiment. SUTVA is formally defined as (Rubin 1990):

$$Y_i^{\mathbf{T}} = Y_i^{\mathbf{T}'} \text{ if } T_i = T'_i \tag{15}$$

When SUTVA holds, units do not interfere with each other, in the sense that the outcome of every unit is solely affected by the treatment received by that unit, regardless of the treatment status assigned to the rest of the units participating in the experiment. In the experiment considered here, SUTVA requires that a senator assigned a four-year term behave identically whether all other senators are assigned two-year terms, half of the remaining senators is assigned four-year terms and the other half is assigned two-year terms, or any other possible arrangement of treatment assignment among the remaining senators.

Theoretical results developed by Muthoo and Shepsle (2006) and Shepsle et al. (2004)<sup>11</sup> suggest that the staggered structure of terms may induce strategic intertemporal interactions between legislators. As shown below, if their model is correct, SUTVA will be violated in any experiment that assigns legislators to different term lengths in a staggered legislature.

Muthoo and Shepsle (2006) and Shepsle et al. (2004) extend Baron and Ferejohn (1989)'s divide-the-dollar game to allow for elections in an  $N$ -member staggered legislature. Although the authors consider a legislature in which each member serves three sessions, in what follows I adapt their results to a two-session legislature in order to resemble the structure of the Texas and Arkansas

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<sup>11</sup>Shepsle, Van Houweling, Abrams, and Hanson (2007) extend the argument to allow for bicameral interactions.

Senates. The basic structure of the game is as follows. A dollar is to be divided between the  $N$  members of the legislature by majority rule, and each member serves a term of two sessions. At the end of each session  $N/2$  legislators face reelection.

A proposal  $p_i$  is defined as  $p_i : m \rightarrow \Delta$ , where  $\Delta$  is the  $N - 1$  simplex and  $m$  is the round of the game, and a vote  $v_i$  is defined as  $v_i : p \times m$ , where  $p$  denotes the proposal under consideration,  $v_i = 1$  means that legislator  $i$  votes “yes” on the proposal  $p$ , and  $v_i = 0$  means that legislator  $i$  votes “no” on the proposal  $p$ .

In each session, the basic game is as follows:

1. A random “recognition rule” is used to choose the member who will make a proposal to divide the dollar. Each member has probability  $1/N$  of being selected or “recognized”.
2. The recognized legislator,  $r$ , proposes  $p_r$ .
3. The proposal  $p_r$  is voted on, and  $V \leq N$  legislators vote for it.
4. If  $V \geq (N + 1)/2$  the proposal  $p_r$  is adopted. If  $V < (N + 1)/2$ , step 1 is repeated, with a legislator randomly selected to make an alternative proposal  $p_a$ . If, once again,  $V \geq (N + 1)/2$ ,  $p_a$  is not approved, the dollar is not allocated and everyone receives zero.

This game is played once in each session, and is followed by an election in which  $N/2$  legislators are up for reelection.

As opposed to Baron and Ferejohn (1989)’s model, legislators do not derive utility from direct consumption of the dollar. They are purely Downsian, in that they only derive utility from winning office, and the dollar is only desired insofar as it helps increase their probability of winning,  $\Pi_i$ . Legislator  $i$ ’s share of the dollar in session  $m$  is denoted by  $x_{im}$ , and the vector  $x_i = (x_{it}, x_{it-1})$ , referred to as legislator’s  $i$  legislative record, contains the share of the dollar received in each session. Legislator’s  $i$  probability of reelection is defined as  $\Pi_i = \Pi_i(x_{it}, x_{it-1})$ , which implies that each legislator’s reelection depends upon his legislative record only, this is, upon his ability to bring pork to his constituents during the course of his term.

The strategy of each legislator  $i$  is a proposal to divide the dollar in case of being recognized, and a decision about how to vote in each of the possible proposals by the other members.

Two assumptions are needed to reach the main theoretical results:

$$\frac{\partial \Pi_i}{\partial x_{ik}}(x_{it}, x_{it-1}) > 0 \quad \text{and} \quad \frac{\partial^2 \Pi_i}{\partial x_{ik}^2}(x_{it}, x_{it-1}) < 0 \quad \text{for } k = t, t-1 \quad (16)$$

$$\frac{\partial \Pi_i}{\partial x_{it}}(x_{it}, x_{it-1}) > \frac{\partial \Pi_i}{\partial x_{it-1}}(x_{it}, x_{it-1}) \quad (17)$$

Assumption 16 means that a legislator’s probability of reelection increases in the share of the dollar he receives in every session, but does so at a decreasing rate. Assumption 17, referred to as the “What have you done for me lately” (WHYDFML) principle, poses that the impact of a further incremental increase in payoff on the reelection probability is larger for later sessions than for earlier ones. This is, voters exhibit a *recency bias*: at election time, they place more weight on the pork received *recently* than on pork received further in the past.

Under these assumptions, the model yields two fundamental results. First, the legislative record  $(x_{it}, x_{it-1}) = (1, 0)$  is ex-ante optimal among all allocations in which legislator  $i$  receives his expected value. This is, each legislator prefers to “backload” the entire expected payoff in the second session, which is the session immediately before reelection. Second, the outcome  $(x_{it}, x_{it-1}) = (1, 0)$  for all  $i$  is a sustainable equilibrium, provided that an adequate punishment regime is implemented. In particular, this punishment regime must penalize all off-the-equilibrium-path behavior.

Thus, this model shows that in a staggered legislature, an equilibrium can be sustained in which the different generations of legislators engage in an intertemporal deal according to which in every period the amount to redistribute is shared *only* among those senators who are facing reelection in that period. In this equilibrium, legislators agree to forgo resources early in their terms to aid the generations that are seeking reelection, a behavior that is later reciprocated by their fellow legislators (given a credible punishment regime) and hence allows them to concentrate or backload all benefits in their last period, just before they face reelection.

Shepsle et al. (2007) provide empirical evidence that this intertemporal deal seems to be present in the U.S. Senate, and although there is to my knowledge no similar study for state legislatures, the relevance of this theoretical argument for the experiment considered here cannot be overemphasized. If the overlapping generations of state senators considered here do engage in an intertemporal distributional agreement, senators who are randomly assigned a four-year term may forgo some benefits that will be reciprocated by senators assigned a two-year term as part of the intertemporal

deal. It is then clear that if none or very few senators were assigned a two-year term, no intertemporal deal would be possible and hence the behavior of senators in the four-year term would be different. Thus, one could no longer define a *pair* of potential outcomes  $(Y_{i0}, Y_{i1})$  for each senator since individual outcomes would depend on the entire vector of treatment assignments. This is there would possibly be one  $Y_i^{\mathbf{T}}$  for every vector  $\mathbf{T}$  or, more formally,

$$Y_i^{\mathbf{T}} \neq Y_i^{\mathbf{T}'} \text{ with } T_i = T_i' \tag{18}$$

and SUTVA would be violated. It follows that under this kind of intertemporal deal the causal effect of term length on senators' behavior,  $\lambda_t$ , would *not* be identified because the control group (senators serving four years) would be adjusting their behavior not only according to their term length but also according to the terms received by other senators, this is, according to the staggered structure of terms.

The discussion so far illustrates that even when the treatment of interest is randomly assigned, modelling individual behavior and considering the incentives imposed by the institutional structure in which individuals interact is crucial to the correct interpretation of the parameters under study. But how plausible is this model? And how could one test whether senators are behaving according to this model in the state senates considered in this paper? For the purposes of evaluating how plausible the model is, the most important piece is the punishment mechanism. The theoretical results show that backloading gives legislators the highest expected utility, and that an equilibrium in which this optimal outcome occurs *can* be sustained. But this is not the only possible equilibrium, and therefore whether it will be the equilibrium that occurs depends entirely on whether the appropriate punishment regime can be implemented.

This punishment regime must essentially punish all off-the-equilibrium-path behavior; for example, if a legislator offers a proposal that does not allocate all the resources to the legislators running for reelection, all legislators must vote against it, and if a coalition of legislators manages to pass the proposal and obtain resources before their reelection session, the punishment regime must be such that this behavior is punished in the sessions to come. But note that this assumes that legislators are always the *same* people, this is, that they don not retire or lose elections. If this assumption is relaxed and the turnover rate is allowed to be greater than zero, implementing the

punishment mechanism becomes a very demanding requirement, since the agreement must supersede individual legislators and be applied to their seats. And there may be reasons to believe that freshmen legislators will not be willing to be punished for a deviation committed by the previous incumbent. Arguably, in an environment where political parties are strong, they can be the ones to enforce the “inheritance” of the punishment between consecutive incumbents of the same seat. Thus, one necessary condition for the achievement of this equilibrium appears to be an institution that can ensure the punishment regime even under positive turnover.

In light of this discussion, the results of this experiment must be interpreted with caution. If these intertemporal deals are occurring, the behavior of the four-year term senators is not the behavior that they would have exhibited if the *entire* senate had been elected for four years, and hence the estimated difference between two-year and four-year senators would reflect not only the effect of serving a shorter term, but would also include the compensating effect of the four-year senators, who would be “allowing” two-year senators to behave in the observed way, under the agreement that they will be reciprocated in the following session. If these intertemporal deals were not occurring and SUTVA were not violated, then the experiment would directly recover  $\lambda$ .

It follows that, if possible, one should establish whether these intertemporal strategic interactions are occurring in the experiments considered here. How could this be tested? I plan to exploit two “placebo” experiments that occur in Arkansas and Texas to test whether overlapping generations of senators are engaging in intertemporal logrolling. In 1993, in Arkansas, senators were assigned to two-year terms and four-year terms only at the end of the legislative session, which means that senators ignored whether they would be running for reelection until after the legislative session adjourned. Similarly, in 1993, in Texas, senators were randomly assigned to two-year and four-year terms in the middle of the legislative session, which means that for the first half of the session senators ignored who would be running for reelection at the end of the session. These periods during which *all* senators ignored when their reelection would be could be used as a baseline to estimate what their behavior is like when intertemporal deals are *not* occurring, under the assumption that when senators ignore who is serving two years and who is serving four years they can not implement an intertemporal deal because they do not know who should be receiving the resources. In other words, “backloading” is undefined when the reelection date is unknown, and hence no intertemporal deals is possible.

During these “blind” periods when senators ignore when they will run for reelection, their behavior will not be contaminated by intertemporal deals. Thus, one can observe whether after terms have been assigned both groups deviate from their previous behavior in the blind period. If one observed that, for example, abstention rates increased for the two-year group and simultaneously decreased for the four-year group relative to the blind period abstention rates, then this would provide some evidence in favor of the existence of intertemporal logrolling. If, on the other hand, the abstention rates of the two-year group increased but the abstention rates of the other group remained at the same blind-period levels, then perhaps this could be used as evidence that intertemporal deals are not likely to be occurring.<sup>12</sup> Considering this and possibly other ways to test whether intertemporal logrolling is a topic of current research.

## 5 Randomization in state senates

In this section, I describe the details of the random assignment of term lengths in Arkansas and Texas. This section also discusses the case of Illinois, the third state in the U.S. where term lengths are randomly assigned after reapportionment. The state of Illinois is not included in the analysis because (i) as explained below, the random assignment of term lengths was done at the level of district groups and not of individual senators, (ii) randomization is performed before the elections, and (iii) the availability of roll-call data is somehow limited.

Arkansas and Texas share a unique feature among all U.S. states: a new State Senate is chosen after every apportionment, and the senators elected after each apportionment are divided by lot into different groups or “classes” according to the length of their term immediately following reapportionment. The purpose of this provision is to allow all senate districts to have elections in the first election under the new district plan, to ensure that the senate is representative of the newly created districts. The following sections present the specific details of the allotment process in each of these states.

It is important to note that in all cases, the random assignment of term lengths is at the senate

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<sup>12</sup>Of course, this comparison requires strong assumptions, since the blind period behavior could be simultaneously affected by the uncertainty in the time horizon, and hence a difference in behavior between the blind period and the period when all reelection dates are known could be entirely caused by a time-horizon effect. It seems that in the proposed strategy, finding no difference between the two periods for *only one* of the groups would be the most promising piece of evidence, since it could be interpreted as evidence against intertemporal logrolling under some time-stationarity assumptions.

*district* level and not at the senator level, so that if a senator decides not to run for reelection his successor must respect the term length that was assigned to the seat after the last reapportionment.

## 5.1 Arkansas

The Arkansas State Senate has 35 members serving four-year terms. In all general elections that do not occur immediately after a redistricting plan has been implemented, half of the senate seats are up for election.<sup>13</sup> Senators are therefore staggered, with half of them running for election/reelection every two years.

The Arkansas Constitution requires that immediately following each Federal census senate districts be redrawn to ensure that they each district contain approximately the same population. In the election immediately after each reapportionment, all state senate seats must be up for election to ensure that the new constituencies be appropriately represented.

When all senate seats are up for election, the staggered structure of senators' terms is broken; the way in which it is restored is the random assignment of term lengths. Section 6, Amendment 23, of the Arkansas Constitution requires that senators be randomly divided into two roughly equally sized classes after each reapportionment. Senators in the class of size 18 serve a two-year term immediately following redistricting and a four-year term thereafter, while senators in the class of size 17 serve a four-year term immediately following redistricting and a two-year term at the end of the decade.

Senators draw lots to determine which districts will initially have two-year terms at the first legislative session of each legislature elected immediately after redistricting. Importantly, the Arkansas Constitution mandates that senators draw lots in the first legislative session immediately after reapportionment, but it does not specifically require that lots be drawn before or at the beginning of such session. However, lots are usually drawn before the first legislative day.

As shown in Table 8, however, in 1993 lots were not drawn until *after* the end of the regular legislative session. This was due to the fact that a law mandating term limits in the state senate and house was passed in 1992 and it was believed by state senate officials that the passage of this law invalidated the drawing of lots. However, it was later decided by the state courts that lots had

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<sup>13</sup>Technically, since the total number of senate districts is odd, the districts are divided into two groups of size 18 and 17 respectively.

nevertheless to be drawn, and senators were called in October of 1993, *after the 1993 legislative session had ended*, to randomly assign their districts to one of the two classes. I currently do not include the 1993 session in my analysis.<sup>14</sup>

As also shown in Table 8, the assignment of terms was performed just *before* the beginning of the 2003 regular session, ensuring that during the campaign senators *ignored* the duration of the term for which they would be elected.

As mentioned above, Arkansas state senators are term limited; as of 1992, they can serve a maximum of eight years in their lifetime. The Arkansas State Legislature is a medium sized legislature relative to other legislatures in the U.S. (see classification by the National Conference of State Legislatures). The regular session meets for approximately 60 days in every odd-numbered year, and thus members are generally part-time legislators.

## 5.2 Texas

The Texas state senate has 31 members serving four-year terms. As in Arkansas, in all general elections that do not occur immediately after redistricting, half of the senate seats are up for election.<sup>15</sup> Senators are therefore staggered, with half of them running for election/reelection every two years.

However, Section 3, Article *III*, of the Texas Constitution (reproduced in Appendix (A)) mandates that an entirely new senate must be chosen following each reapportionment, which means that after state senate districts are redistricted, all senate seats must be up for election. Naturally, this breaks the staggered structure of senators' terms which is also mandated by the Constitution; the mechanism by which terms are staggered again is the random assignment of senate seats to two classes of size 15 and 16 respectively. Senators whose seats belong to the class of size 15 serve a two-year term immediately following redistricting and a four-year term thereafter, while senators whose seats belong to the class of size 16 serve a four-year term immediately following redistricting and a two-year term at the end of the decade.

The Texas state senate convenes in January of every odd year for a regular session that is

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<sup>14</sup>As a matter of fact, this session could be used as a placebo test, as I do for Texas 73rd session. Unfortunately, these data are not readily available, and for this reason is not currently included in the analysis.

<sup>15</sup>Again, since the total number of state senate districts is odd, the districts are divided into two groups of size 15 and 16 respectively.

approximately sixty days long. After the regular session adjourns, a special session may be called by the governor to consider additional legislation.<sup>16</sup>

The Texas Constitution requires that state senate districts be redrawn following publication of the decennial census. Normally, new districts are redrawn in the two years following the beginning of each decade, so that in the general elections year ending in 2 (e.g., 1992, 2002, etc.), all state senate seats are up for election. In the three Texas randomizations considered here, lengths were randomly assigned the year *after* the election, when the respective regular session convened. This implies that, as in Arkansas, during the campaign, senators *ignored* the duration of the term for which they would be elected.

As shown in Table 8, there were three random assignment of senators' terms between 1992 and 2003: once after the redistricting plans that followed each decennial census, and one additional time in 1995 due to the fact that senate districts were again redrawn between the 1992 and the 1994 general elections.

As also shown in this table, in 1993 term lengths were assigned at the end of March while the senate convened at the beginning of January. Thus, for approximately two months senators *ignored* whether they would have to run for reelection in 1994. Below I will use this feature as a crucial placebo test, since the assignment of different term lengths should have *no* effect in the behavior of senators before this assignment was actually performed.

The Texas State Legislature is also a medium sized legislature relative to other legislatures in the U.S. (see classification by the National Conference of State Legislatures). As in Arkansas, the regular session meets for approximately 60 days in every odd-numbered year, and thus members are generally part-time legislators. Members are not term-limited.

### 5.3 Illinois

The Illinois State Senate is significantly different from both the Arkansas State Senate and the Texas State Senate. It has 59 members, almost twice the size of the senates in the other two states, and its regular session meets every year for approximately 160 days. Because members must devote a significant part of their time to the legislature every year, they have larger staffs and most of

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<sup>16</sup>This is also the case in Arkansas and Illinois.

them are full-time legislators.

State senators generally serve four-years, and they are staggered so that every two years a third of the senate seats is up for election.<sup>17</sup> Once again, after each reapportionment all senate seats are up for election to ensure that the new electorate in each district is adequately represented, and the staggered structure is broken.

Section 2.a, Article IV, of the Illinois Constitution (reproduced in Appendix (A)) mandates that after each reapportionment senate districts be divided into three classes. Senators whose districts belong to the first class are elected for terms of four years, four years and two years, respectively, during the ten years that follow each redistricting plan. Senators whose districts belong to the second class, are elected for terms of four years, two years and four years, respectively. Finally, Senators whose districts fall into the third class are elected for terms of two years, four years and four years, respectively. While the Illinois Constitution does not explicitly require that senators be assigned to classes randomly, Article 29c in Illinois Election Code does require that assignment be made random.

The random assignment mechanism is different from that of Arkansas and Texas, since it is stratified by groups, which are predetermined. The first group includes districts 1, 4, 7, 10,  $\dots$ , 58, the second group includes districts 2, 5, 8, 11,  $\dots$ , 59 and the third group includes districts 3, 6, 9, 12,  $\dots$ , 57 (see Appendix (A) for further details). After each reapportionment, each of these three groups is randomly assigned to exactly one of the classes mentioned above:

- Class 1: serves 4 years, 4 years, 2 years
- Class 2: serves 4 years, 2 years, 4 years
- Class 3: serves 2 years, 4 years, 4 years

Thus, the assignment of term lengths is done at the group-level and not at the senate-district level.

Another key difference between Illinois and Arkansas and Texas is that the random assignment of districts groups to the different classes is done after redistricting but *before* the election, so that senators do know whether they will be initially serving a 2-year term or a 4-year term.

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<sup>17</sup>Again, senators are only roughly divided in thirds, since the total number of seats is not a multiple of three. There are three groups, one of size 19 and two of size 20.

## 6 Randomization inference

Inference procedures for treatment effects under random assignment usually rely on large sample approximations for the statistics of interest. For example, a widespread approach for the estimation of the average treatment effect (ATE) is to compute the difference in means between groups (possibly using parametric models for covariate adjustment) and then to make inferences assuming its distribution is (approximately) Gaussian. A major concern with this approach when sample sizes are relatively small is that it imposes unnecessary strong assumptions that are not guaranteed by the randomization of treatment status and, when violated, may lead to incorrect inferences.

An alternative fully non-parametric, robust approach when treatment is randomly assigned is to base all statistical inferences on the randomization process itself. The randomization model was first proposed by Fisher (1935) and has been recently used to analyze field experiments, natural experiments and observational studies (e.g. Bowers and Hansen 2008; Rosenbaum 2002a; Ho and Imai 2006; Imbens and Rosenbaum 2005). The basic characteristics of the randomization model are the following (Lehmann 1998): (i) treatment is randomly assigned among subjects, (ii) the randomization procedure is *known*, and (iii) subjects are fixed, i.e. they are not assumed to be a random sample from a population. The randomization of term lengths in Arkansas, and Texas satisfies these three conditions and hence is an ideal setting to obtain statistical inferences via a randomization inference approach.

The most attractive methodological feature of a randomization model is that the hypothesis of no treatment effect can be tested with no assumptions of any kind (Rosenbaum 2002b). Let  $\mathbf{Y}$  be an  $N$ -dimensional column vector which contains the responses for the  $N$  subjects to whom treatment is randomly assigned, let  $\mathbf{T}$  be the  $N$ -dimensional vector of treatment assignments, and let  $N_t$  be the number of treated subjects and  $N_c = N - N_t$  the number of control subjects.

When the randomization procedure is known, one can define the set  $\Omega$  of all possible values of the vector  $\mathbf{T}$  in which the number of treated subjects is fixed to be  $N_t$  (and hence the number of controls is fixed to be  $N_c$ ). In the randomization of term lengths that occurs in Arkansas and Texas, the number of elements in the set  $\Omega$  is all possible values of the vector  $\mathbf{T}$  in which there are  $N_t$  ones and  $N - N_t$  zeros. Thus, the cardinality of  $\Omega$  is  $\text{card}(\Omega) \equiv \binom{N}{N_t}$  and each of these possible

assignments has an equal probability of occurring, given by

$$\mathbb{P}(\mathbf{T} = \mathbf{t}) = \frac{1}{\text{card}(\Omega)} \quad (19)$$

To test the hypothesis that the treatment is without effect, one defines a test-statistic  $W(\mathbf{T}, \mathbf{Y})$  which depends on the treatment assignment  $\mathbf{T}$  and the vector of outcomes  $\mathbf{Y}$ . Under the null hypothesis that the treatment is without effect, the potential outcomes are fixed, and they can be considered to be assigned to treatment or control together with the subjects. Thus, under the null hypothesis of no treatment effect the *only* random variable is the treatment assignment, and therefore the distribution of  $W(\mathbf{T}, \mathbf{Y})$  is completely determined by the randomization distribution of  $\mathbf{T}$ . To emphasize that the vector of potential outcomes is not a random variable, I write  $W(\mathbf{T}, \mathbf{y})$ .

In a two-sided test, the null hypothesis of no treatment effect is rejected for large values of the absolute value of the observed test-statistic  $W(\mathbf{t}, \mathbf{y})$ . In this randomized experiment, the two-sided significance level for a test that rejects the null hypothesis of no treatment effect is given by

$$p = \frac{\text{card}(\mathbf{T} \in \Omega : |W(\mathbf{T}, \mathbf{y})| \geq |W(\mathbf{t}, \mathbf{y})|)}{\binom{N}{N_t}} \quad (20)$$

If  $N$  were sufficiently small, the p-value defined by equation (20) could be calculated exactly. In the experiments considered in this paper, however, exact calculation is not possible because there are  $\binom{31}{15}$  possible treatment assignments in Texas and  $\binom{35}{18}$  possible treatment assignments in Arkansas, which makes the computation of all possible values of the test statistic cumbersome.

An alternative approach is to approximate the distribution of  $W(\mathbf{T}, \mathbf{y})$  via simulations (see, for example, Ho and Imai 2006). The algorithm I use is the following:

1. Calculate the observed value of the test statistic in the data,  $W(\mathbf{t}, \mathbf{y})$ .
2. Take a random sample without replacement of the treatment assignment,  $\mathbf{T}^s$  (ie, draw  $\mathbf{T}^s$  from the known distribution of  $\mathbf{T}$ )
3. Calculate the simulated value of the test-statistic,  $W(\mathbf{T}^s, \mathbf{y})$ .
4. Repeat steps (2) and (3)  $S$  times

5. Calculate the approximated, two-tailed p-value as

$$p^* = \frac{1}{S} \sum_{s=1}^S \mathbb{1} \{|W(\mathbf{T}^s, \mathbf{y})| \geq |W(\mathbf{t}, \mathbf{y})|\} \quad (21)$$

where  $\mathbb{1}\{\cdot\}$  is the indicator function.

A crucial feature of randomization inference is that one can derive from equation (20) the randomization distribution of *any* test statistic  $W(\mathbf{T}, \mathbf{y})$ . Thus, the hypothesis of no treatment effect can be tested using different test-statistics to ensure that inferences are not driven by the particular choice of  $W(\mathbf{T}, \mathbf{y})$ . In the analysis below, I base my inferences on four different test-statistics: (i) the difference in means, (ii) the difference in medians, (iii) the rank-sum statistic, and (iv) the the maximum absolute difference between the treated and control distributions (the  $D$  statistic). These test-statistics are respectively defined as follows:

$$W_1(\mathbf{T}, \mathbf{y}) = \frac{\sum_{i=1}^N T_i y_i}{N_t} - \frac{\sum_{i=1}^N (1 - T_i) y_i}{N_c} \quad (22)$$

$$W_2(\mathbf{T}, \mathbf{y}) = y_t^{med} - y_c^{med} \quad (23)$$

$$W_3(\mathbf{T}, \mathbf{y}) = \sum_{i=1}^N \mathbb{1}\{T_i = 1\} r(y_i) - \frac{1}{2} N_t (N_t + N_c + 1) \quad (24)$$

$$W_4(\mathbf{T}, \mathbf{y}) = \sup_x \left| \widehat{F}_t(x) - \widehat{F}_c(x) \right| \quad (25)$$

where  $r(\cdot)$  is the rank function,  $y_t^{med}$  and  $y_c^{med}$  are the median of the treated and control outcomes, respectively, and  $\widehat{F}_t$  and  $\widehat{F}_c$  are the empirical cumulative distribution functions of the treated and control outcomes, respectively.

## 7 Results

### 7.1 Balance tests

In large samples, the random assignment of treatment implies that the distributions of all variables not affected by treatment (including baseline covariates) are identical between treatment and control groups. In finite samples, due to (random) sampling variability, the empirical distribution of baseline covariates may be significantly different between both groups. Since covariate imbalance

may lead to biased inferences, testing whether randomization produced similar average empirical distributions across groups is crucial.

I collected a large number of baseline covariates for all state senators in the states of Arkansas and Texas and tested whether the probability distributions of a large number of baseline characteristics were statistically equal between both groups. All hypothesis tests were conducted using the randomization inference procedure described in Section 6 for the four different test-statistics when appropriate<sup>18</sup>.

Figure 4 presents the results for Arkansas, where the covariates refer to the senator level. As can be seen, the hypothesis that the covariate distribution is the same between groups is not rejected at standard significance levels for any of the characteristics presented.

Figures 1, 2 and 3 present the results for Texas, for the years 1993, 1995, and 2003 respectively. Again, the covariates are extremely well-balanced between both groups. Senators are similar in terms of their demographics, and districts are similar in terms of their partisanship, as measured by the Democratic vote share for several state-wide and national offices.

In all figures, the columns show the mean of each covariate in each group to provide the reader with some information about the distributions. The graph shows the p-values corresponding to the test of the null hypothesis that the treatment is without effect. Note that for all non-dichotomous variables the hypothesis that there is no difference between the groups is tested using not only the difference in means but also three other test-statistics that concentrate in different characteristics of the distributions. These findings provide strong statistical evidence that the entire probability distributions of these baseline characteristics are equal, and hence that randomization was successful in ensuring the orthogonality between treatment assignment and all potential confounders.

## 7.2 The causal effect of term length on the behavior of senators

Do senators whose reelection is two years away behave differently from senators whose reelection is four years away? In this section, I consider whether senators whose reelection is closer have an incentive to take position more often, as measured by their abstention rates. The evidence suggests that the positions incumbents take when in office may have electoral consequences (e.g Erikson

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<sup>18</sup>For binary variables, only the difference-in-means statistic is used.

1971; Canes Wrone, Brady, and Cogan 2002). In turn, this suggests that electoral pressures may make legislators more cautious with respect to the positions they take, since taking (or not) a particular position may entail some political risks.

I study the impact of term length on the abstention rate, defined as the proportion of roll-call votes in which the senator voted neither “Yea” nor “Nay”.<sup>19</sup> Table 3 presents the results for the 73rd (1993), 74th (1995) and 78th (2003) Texas Legislature Regular Sessions. In general, the results show that senators whose reelection is closer in time have a *higher* abstention rate. As mentioned in Section 5, in 1993 the assignment of term lengths occurred in the middle of the legislative session, effectively providing a placebo test. Thus, I divide the sample in two, according to whether voting occurred before or after randomization. As shown in Table 3, while before randomization there is no significant difference between the groups which will be later assigned to different term lengths, after the assignment senators whose reelection is two years away present a significantly *higher* abstention rate. In 1995 the results are not statistically significant, even though the estimated ATE is consistent with the findings in the other years. In 2003, the results again show that senators whose reelection is closer present significantly higher abstention rates.

The results for Arkansas’ 84th Legislature are presented in Table 2. In this state, a shorter term length seems to have no effect in abstention rates. Of course, there are many differences between the political environment of Texas and Arkansas that may explain the results. One possible explanation may be that in Texas, the State Legislature has been more salient (for example, the Texas Legislature was involved in controversial redistricting cases during the 90s and the first years of the 2000 decade, which received considerable media attention, sometimes at a national level).

This set of results suggest that, in Texas, when elections are closer, senators take position less often. Although in Arkansas shorter term lengths do not seem to increase abstention rates, these results are consistent with the results for Texas in showing that when elections come near incumbents do *not* seem to have an incentive to abstain less often. These findings are consistent with some of the evidence presented by Thomas (1991), according to which U.S. senators increase their rate of nonvoting to avoid taking a position during the last year in office.

But the interpretation of increased abstention rates as avoiding position-taking requires some extra analysis, since senators could be abstaining more often for reasons unrelated to position-

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<sup>19</sup>This measure is commonly used in the literature; see, for example, Rothenberg and Sanders (2000).

taking. In particular, senators who are serving a shorter term may be devoting a higher proportion of their time to campaign activities in their districts, which would also cause increased abstentions. In an attempt to separate both effects, Table 4 shows the effect of term length on the proportion of the total legislative days in which senators abstained in *all votes of the day*. As seen in this table, there is no significance difference between senators serving two years and senators serving four years when daily abstentions are considered. This gives evidence against the campaigning hypothesis, since if senators were traveling more to their districts, one would expect to see that they are absent for entire days. The results in Table 4 show that senators whose reelection is closer are present in the chamber the same average number of days than senators whose reelection is further away, providing evidence that their higher abstention rates are caused by their abstaining in *some but not all* of the votes in any given day.

Table 5 presents the effects of term length on bill sponsorship. The variables analyzed are the number of bills introduced by a given senator as a proportion of the total number of bills introduced during the legislative session, and the number of bills introduced by a given senator and passed in the chamber as a proportion of the total bills passed during the legislative session. The second variable is included to establish whether the two groups of senators coordinate to pass disproportionately more bills sponsored by 2-year senators, providing one possible test of SUTVA.

As seen in this table, in Texas, there is no difference in either of these variables in any of the three sessions considered. Senators serving two years introduce the same proportion of bills as senators serving four years, and the same holds for the proportion of bills passed. In Arkansas, however, senators serving two years introduce a significantly lower number of bills than senators serving four years, and this is not compensated by passing disproportionately more bills introduced by 2-year senators. As seen in the last row of Table 5, the share of bills introduced and passed by 2-year senators is also significantly lower than the corresponding share for 4-year senators. The results for Arkansas suggest that during a reelection session senators may decide to allocate their resources away from bill sponsorship and towards campaign activities and may also choose to be more selective in the type of bills that they decide to sponsor. Finally, the evidence for both Arkansas and Texas is consistent with that presented by Schiller (1995), who studied bill sponsorship in the U.S. senate and found that reelection was not significantly associated with bill introduction.

## 8 Final remarks and directions of future research

The length of terms for which legislators are elected were carefully designed based on the premise that the frequency of elections had a direct relationship to the behavior of legislators while in office. Studying this premise empirically has been difficult, given the extraordinary methodological challenges that are involved in isolating the effect of term length from all other factors that affect the behavior of legislators.

In this paper, I use a unique experiment that assigned term lengths at random after redistricting in the state senates of Arkansas and Texas to identify the causal effect of term length on senators' behavior. In Texas, senators whose reelection is two years away have a higher abstention rate than senators whose reelection is four years away. In Arkansas, senators serving a shorter term introduce a significantly lower number of bills during the legislative session. Taken together, these results suggest that legislators modify their behavior according to their remaining time in office and, in particular, that shorter term lengths seem to provide an incentive to engage in less formal legislative activities. The results are likely the combined effect of a reallocation of limited resources towards campaign activities, and an avoidance of position-taking on bills and votes that may be politically costly.

Future research will examine the relative importance of the campaign activities hypothesis and the avoidance of position-taking hypothesis in more detail. For abstention rates, the effect of term length will be analyzed for salient and non-salient bills separately. This will provide crucial evidence, since if short-termed senators are indeed avoiding position-taking, their higher abstentions rates must be caused by abstentions in salient bills. The analysis of bill sponsorship is more challenging, as by definition the bills chosen not to be introduced are not observed. Still, a comparison of the type of bills introduced by senators in both group may provide important evidence.

Finally, a crucial issue which is currently under study is the largely analyzed question of responsiveness, that is, whether senators who are closer to their reelection are more responsive to their constituents' preferences. This question is being analyzed by scaling legislators' roll-call votes and ranking senate districts according to a weighted average of presidential, governor, U.S. senate, U.S. House, and state house vote shares.

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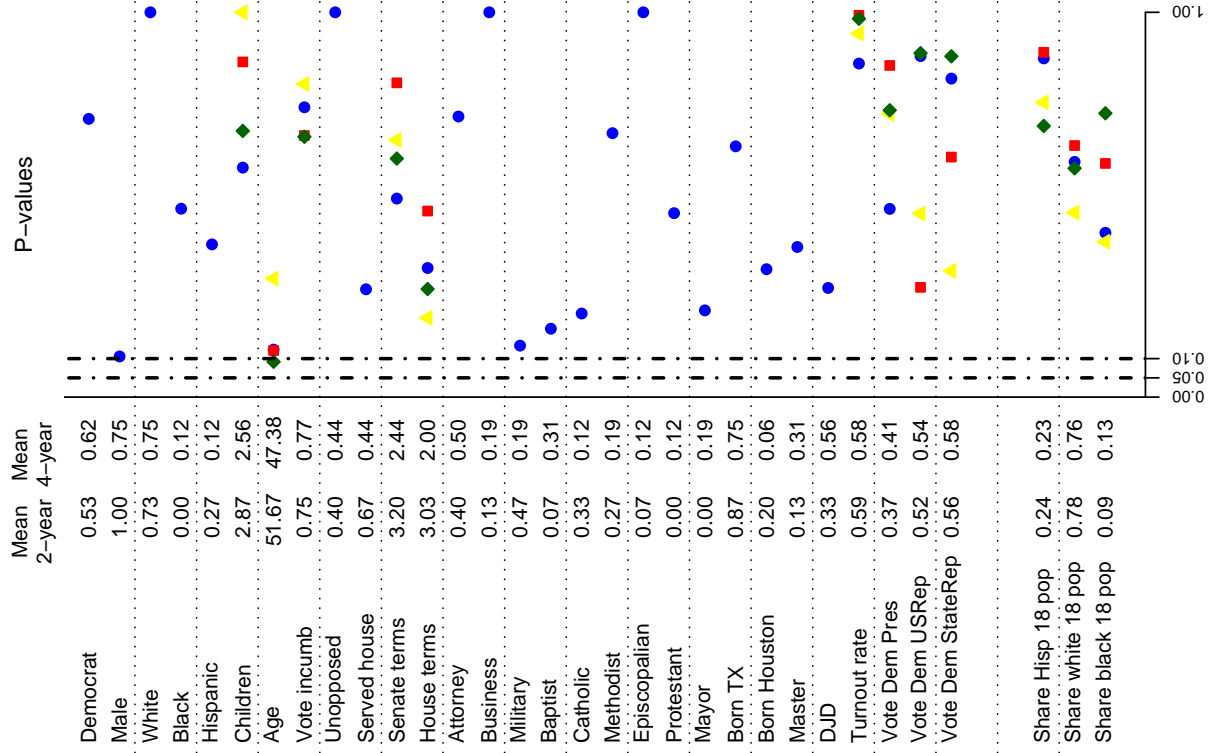
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Table 1: State Senate sessions in which terms were randomly assigned

		Arkansas	Texas	Illinois
1993	Regular session convened		Jan 12	Jan 13
	Lots drawn	Oct 01	Mar 25	
	Regular session adjourned		May 31	Jan 10, 1995
1995	Regular session convened	–	Jan 10	–
	Lots drawn	–	Jan 11	–
	Regular session adjourned	–	May 29	–
2003	Regular session convened	–	Jan 14	Jan 08
	Lots drawn	Dec 03, 2002	Jan 14	
	Regular session adjourned	–	Jun 02	Jan 11, 2005

**Figure 1: Balance tests – Texas 1993**



**Figure 2: Balance tests – Texas 1995**

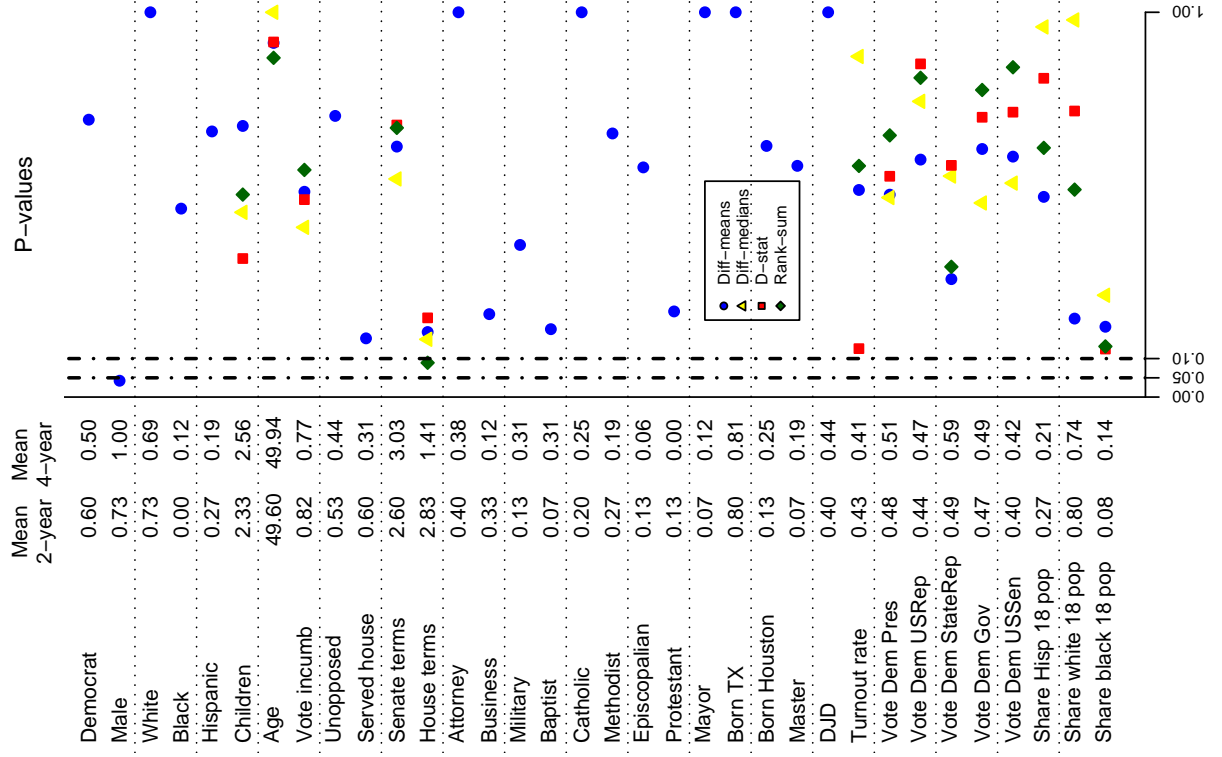


Figure 3: Balance tests – Texas 2003

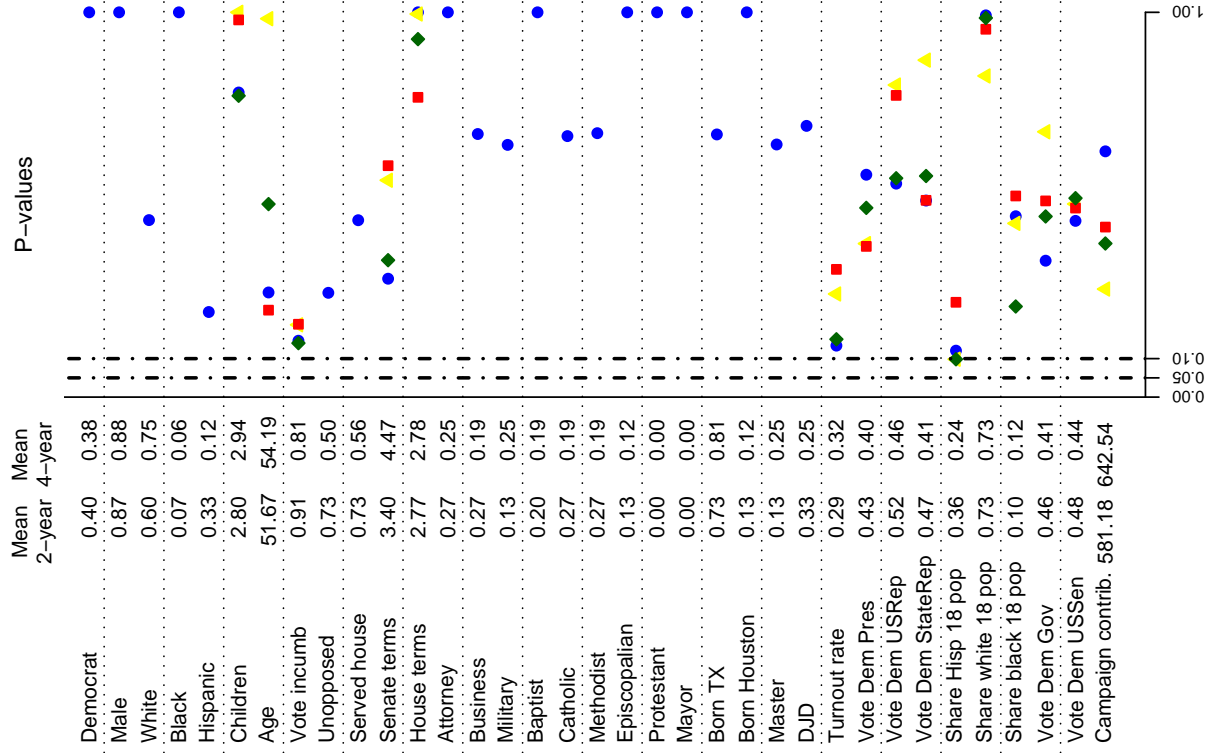


Figure 4: Balance tests – Arkansas 2003

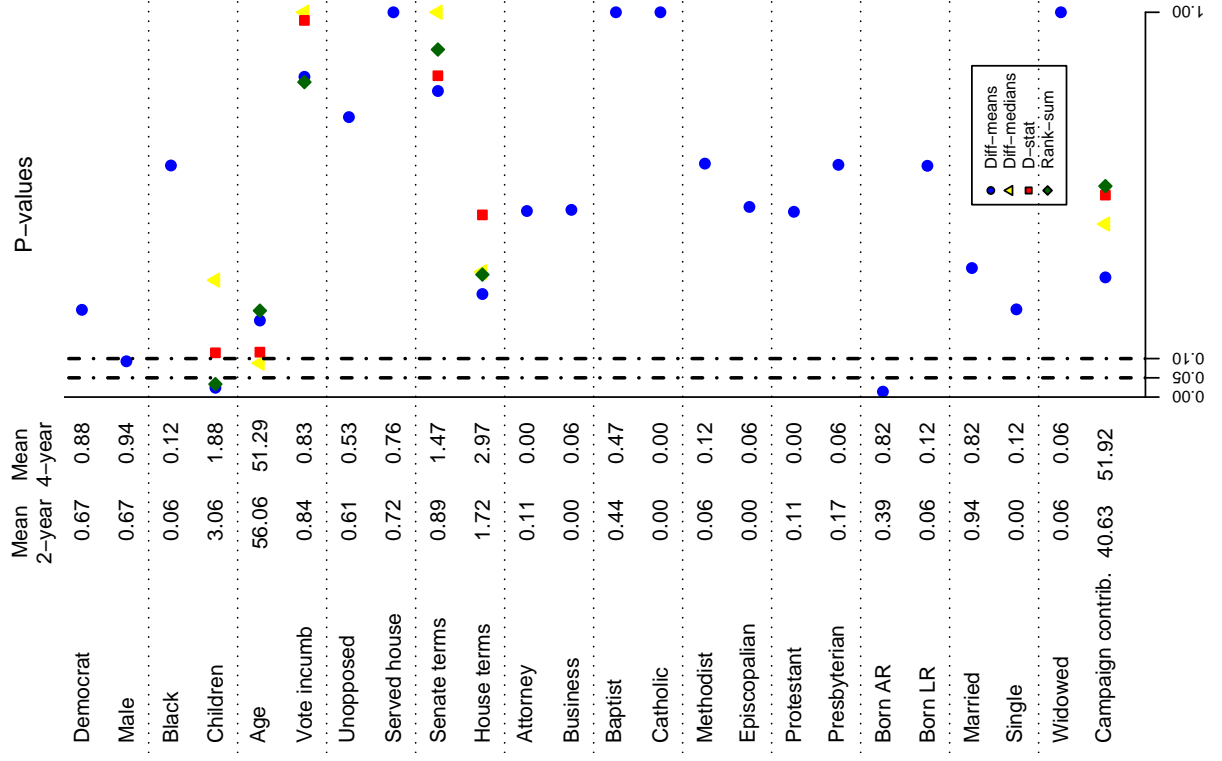


Table 2: Abstention Rates in Arkansas State Senate – 84th Session (2003)

Proportion	Mean 2yr	Mean 4yr	Med 2yr	Med 4yr	P-val (mn)	P-val (md)	P-val (d)	P-val (rs)	HL
Votes neither Y nor N	0.0294	0.0355	0.0246	0.0215	0.5895	0.7605	0.5372	0.9277	0.0007

Note: Total sample size is 35: 18 senators in the 2-year group and 17 senators in the 4-year group. Column labeled HL presents Hodghes-Lehmann estimate of location.

Table 3: Abstention Rates in Texas State Senate – 73rd (1993), 74th (1995), 78th(2003) Sessions

Year	Proportion	Mean 2yr	Mean 4yr	Med 2yr	Med 4yr	P-val (mn)	P-val (md)	P-val (d)	P-val (rs)	HL
1993 – pre ran	Votes neither Y nor N	0.021	0.026	0.008	0.006	0.629	0.996	0.730	0.767	0.000
1993 – pos ran	Votes neither Y nor N	0.044	0.026	0.047	0.023	0.049	0.043	0.103	0.068	0.016
1995	Votes neither Y nor N	0.046	0.020	0.020	0.020	0.243	1.000	0.756	0.649	0.002
2003	Votes neither Y nor N	0.029	0.011	0.024	0.009	0.004	0.015	0.071	0.007	0.014

Note: Total sample size is 31: 15 senators in the 2-year group and 16 senators in the 4-year group. Column labeled HL presents Hodghes-Lehmann estimate of location.

Table 4: Abstention rates at the day level in Texas State Senate – 73rd (1993), 74th (1995), 78th(2003) Sessions

	Mean 2 yr	Mean 4 yr	Pval (mn)	Pval (md)	Pval (rs)	HL	
1993 – pos ran	Prop. days abstained all day	0.019	0.015	0.683	0.611	0.523	0.000
1995	Prop. days abstained all day	0.053	0.012	0.189	0.411	0.631	0.000
2003	Prop. days abstained all day	0.029	0.017	0.210	0.196	0.120	0.014

Note: Total sample size is 31: 15 senators in the 2-year group and 16 senators in the 4-year group. Column labeled HL presents Hodghes-Lehmann estimate of location.

Table 5: Bill Sponsorship in Texas and Arkansas Senates

Year	Proportion	Mean 2yr	Mean 4yr	Med 2yr	Med 4yr	P-val (mn)	P-val (md)	P-val (d)	P-val (rs)	HL
TEXAS										
1993	Share Introduced Bills	0.033	0.031	0.030	0.031	0.663	0.788	0.819	0.945	0.000
	Share Total Bills Passed	0.033	0.031	0.027	0.031	0.755	0.551	0.815	0.948	0.000
1995	Share Introduced Bills	0.031	0.033	0.030	0.027	0.738	0.479	0.682	0.944	0.001
	Share Total Bills Passed	0.035	0.030	0.037	0.027	0.388	0.393	0.649	0.482	0.004
2003	Share Introduced Bills	0.021	0.033	0.025	0.036	0.027	0.063	0.019	0.025	-0.013
	Share Total Bills Passed	0.021	0.033	0.025	0.036	0.022	0.052	0.027	0.015	-0.012
ARKANSAS										
2003	Introduced Bills	0.0196	0.0292	0.0186	0.0295	0.0041	0.0066	0.0149	0.0046	-0.0090
	Share Total Bills Passed	0.0185	0.0294	0.0169	0.0270	0.0017	0.0089	0.0066	0.0039	-0.0095

Note: In Texas, total sample size is 31: 15 senators in the 2-year group and 16 senators in the 4-year group. In Arkansas, total sample size is 35: 18 senators in the 2-year group and 17 senators in the 4-year group. Column labeled HL presents Hodghes-Lehmann estimate of location.

## **A Extract from states constitutions**

### **Arkansas: Amendment No. 23, Section 6**

At the next general election for State and County officers ensuing after any such apportionment, senators and representatives shall be elected in accordance therewith and their respective terms of office shall begin on January 1 next following. At the first regular session succeeding any apportionment so made, the Senate shall be divided into two classes by lot, eighteen of whom shall serve for a period of two years and the remaining seventeen for four years, after which all shall be elected for four years until the next reapportionment hereunder.

### **Illinois: Article IV, Section 2.a**

One Senator shall be elected from each Legislative District. Immediately following each decennial redistricting, the General Assembly by law shall divide the Legislative Districts as equally as possible into three groups. Senators from one group shall be elected for terms of four years, four years and two years; Senators from the second group, for terms of four years, two years and four years; and Senators from the third group, for terms of two years, four years and four years. The Legislative Districts in each group shall be distributed substantially equally over the State.

### **Illinois Election Code: Article 29C, *Division of Legislative Districts***

Sec. 29C. Three Groups of Legislative Districts.

The 59 legislative districts of this State are divided into 3 groups for the purpose of establishing the terms of Senators elected from each group. The districts in each group are distributed substantially equally over the State. The 3 groups shall consist of 20, 20 and 19 legislative districts, respectively, with each group having as its first district number, one of the numbers 1, 2 or 3, and shall be comprised of other district numbers,

as follows:

1	4	7	10	13	16	19	22	25	28	31	34	37	40	43	46	49	52	55	58
2	5	8	11	14	17	20	23	26	29	32	35	38	41	44	47	50	53	56	59
3	6	9	12	15	18	21	24	27	30	33	36	39	42	45	48	51	54	57	

(Source: P.A. 87-1052.)

Sec. 29C-10. Terms of Senators in each group.

Senators shall be elected from districts in each group of legislative districts on the dates and for terms as follows: First group - 2002 and 2006 for 4 years each, and in 2010 for 2 years; Second group - 2002 for 4 years, 2006 for 2 years, and in 2008 for 4 years; and Third group - 2002 for 2 years, and in 2004 and 2008 for 4 years each. All 59 Senators, one from each of the 59 districts, shall be elected at the first general election of representatives next occurring after each decennial redistricting.

(Source: P.A. 92-535, eff. 5-31-02.)

Sec. 29C-15. Determination of groups.

To determine which group of Legislative Districts shall be the "First group", "Second group", or "Third group" for the purpose of establishing the terms for which Senators shall be elected in each group until the next decennial redistricting, as provided in Section 3 of Article IV of the Illinois Constitution of 1970, the Secretary of State, in the presence of the majority and minority leaders of the Senate, after due notice to them, shall, as soon as practicable after each redistricting finalized according to law, draw one card at random, from 3 cards bearing the numbers 1, 2 and 3, and then draw one card at random from the 2 remaining cards. The first number so drawn shall be the first number of the "First group" and Senators shall be elected from districts in that group for terms as provided in Section 29C-10 for that group. The second number so drawn shall be the first number of the "Second group" and Senators shall be elected from districts in that group for terms as provided in Section 29C-10 for that group.

The number on the remaining card shall be the first number of the "Third group" and Senators shall be elected from districts in that group for terms as provided in Section 29C-10 for that group.

(Source: P.A. 87-1052.)

**Texas: Article 3, Section 3**

The Senators shall be chosen by the qualified voters for the term of four years; but a new Senate shall be chosen after every apportionment, and the Senators elected after each apportionment shall be divided by lot into two classes. The seats of the Senators of the first class shall be vacated at the expiration of the first two years, and those of the second class at the expiration of four years, so that one half of the Senators shall be chosen biennially thereafter. Senators shall take office following their election, on the day set by law for the convening of the Regular Session of the Legislature, and shall serve thereafter for the full term of years to which elected. (Amended Nov. 8, 1966, and Nov. 2, 1999.)