Term Length and The Effort of Politicians

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Abstract

We evaluate the effects of a fundamental lever of constitutional design: the duration of public office terms. We present a simple model grounded in interviews with legislators and highlight three forces shaping incentives to exert legislative effort. We exploit two natural experiments in the Argentine Congress (where term lengths were assigned randomly) to ascertain which forces are empirically dominant. Results for separate measures as well as an aggregate index of legislative effort show that longer terms increase effort. Shorter terms appear to discourage effort not due to campaign distractions but due to an investment payback logic: when effort yields returns over multiple periods, longer terms yield a higher chance of capturing those returns. A broader implication is that job stability may promote effort despite making individuals less accountable.

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1 Introduction

A fundamental question in constitutional design is how long public officials should serve before they can be replaced. One advantage to keeping terms short is that more frequent accountability may allow tighter control over political agents and reduce shirking, a point made by Barro (1973, p. 30) in the context of electoral discipline. But, as we demonstrate with a simple model, a high frequency of elections may distract officials from their duties and likewise discourage efforts that take time to yield results. In order to determine an optimal term length for elected officials we must first examine the forces that dominate the incentives facing these agents.

We explore this matter empirically by exploiting two natural experiments in the history of the Argentine legislature. In each instance, politicians were assigned different term lengths through a well-documented and randomized procedure. The first case involves the Argentine House of Representatives in 1983, when 254 House members were randomly assigned to two- or four-year terms; and the second involves the Argentine Senate in 2001, when a constitutional reform led to the random allocation of 71 senators to terms lasting two, four, or six years.

We first analyze the 1983 case of the House of Representatives, for which we have more observations and more measures of effort (to be detailed shortly), and compare the level of legislative effort of the two-year representatives to that of their four-year counterparts. This comparison is made for the first two years of legislative activity, while both groups were working side by side. Our first step is to study the effects on an aggregate index constructed with the z-scores of individual measures of legislative effort (see Kling, Liebman and Katz 2007). The next step is to study the effects on individual measures, namely attendance in floor sessions, participation in floor debates (measured by number of speeches), committee activity (attendance to committee sessions and participation in the production of committee bills), the number of bills each member introduced, and how many of these bills were approved.[1] We also consider an alternative aggregate measure, namely the first principal

[1]Some of these measures have been used in the context of American politics (e.g., Schiller 1995, and Hamm, Harmel, and Thompson 1983).

component of the individual effort measures. We find a significant positive effect of longer terms on the effort index and on the principal component measure. The estimates of the effects of a longer term are positive for all six individual metrics of effort, and depending on the specification we use, they are statistically significant in three or four of the six measures. Our study of the Senate shows a similar pattern.

According to our theoretical model, incentives to perform are stronger in the shorter term as the prize of reelection lies closer in time (an *accountability* effect). But our model also highlights two contrary forces. First, if campaigning commitments clash with legislative duties, a shorter term may lower legislative effort (a *campaigning* effect). Second, if legislative efforts yield rewards that accrue over time, a shorter term lowers the expectation of reaping such rewards, which again discourages effort (a *payback horizon* effect).

Our first set of empirical findings suggests that the accountability effect is overridden by either the campaigning effects, the payback horizon effects, or both. The size of the effects is substantial. For example, longer terms increase senatorial bill submissions by almost 50 percent, and for representatives they almost double the number of bills that get approved. Consequently, longer terms might be a cost-effective way to promote legislative productivity: Ferraz and Finan (2008) estimate that it takes a 20 percent wage increase for local Brazilian legislators to increase the number of bills submitted by 25 percent and the number of bills approved by 22 percent.

Our next step is to investigate the presence of the campaigning and/or the payback horizon effects by empirically contrasting additional implications of the theory. We compare the efforts of the senators who were assigned four-year terms against those assigned terms of six years, and in each case we restrict attention to the first two years of their terms, when campaigning commitments are absent. Under the campaigning hypothesis, we should find no effort differences across legislators, while under the payback scenario effort differences should be apparent. In fact we find that the senators in the six-year track make a greater effort, which suggests that the payback horizon does matter. Similarly, under the campaigning hypothesis the effort of short-term House members should be closer to that of long-term members in the first year when campaigning commitments lie further in the future. Yet, in this case we fail again to find such evidence. An additional implication of our model is that if campaigning drives the effects of term length, then these effects should be stronger among legislators representing geographically remote districts. (These legislators face a stronger conflict between campaigning and legislating.) We again fail to find empirical support for the campaigning hypothesis. In the absence of campaigning-driven effects, our model predicts that payback effects imply an interaction between term length and electoral safety: safer legislators should be less sensitive to term length than those at risk. The data support this prediction. Our final empirical exercise shows that the effects of term length do not seem to vary with experience.

Our results underscore the fact that the advantages of more frequent instances of accountability may be reversed by other forces. Job stability could pay when returns to efforts accrue over time. In this respect, these efforts resemble investments and a longer guaranteed tenure allows additional payback. The legislators we interviewed rated this explanation as highly plausible.

The empirical investigation of the effects of term length faces several identification challenges that are overcome in our study. Consider for example the substantial cross-country variation in the length of legislative terms shown in Table 1.

[TABLE 1 ABOUT HERE]

One might think this variation could help identify the effects of term length on legislative effort; yet, term length may be endogenous to different incentive trade-offs, hindering identification.

An alternative approach is to focus on a single legislature with staggered terms and compare the behavior of legislators facing reelection at different times in the future. Amacher and Boyes (1978) employed this approach focusing on the 93rd Congress of the United States and found that senators closer to reelection voted more in line with House representatives. (In this case, the representatives presumably serve as proxies for constituency interests. Kalt and Zupan (1990) studied the effects of election proximity for senators in the 95th United States Congress without finding a strong effect. Likewise, Thomas (1985) tracked the voting pattern of United States senators in their third versus their sixth years between 1959 and

1976, finding a moderating tendency of election proximity. Lott and Davis (1992) provide further references in this area, and emphasize that most of the papers attempting to identify the effects of electoral proximity focused on voting patterns and suffer from measurement and specification problems.[2] Lott (1987) comes closer to our focus on shirking in his estimation of the negative association between retirement (itself an endogenous variable) and the frequency of voting. The work closest to ours is a recent paper by Titiunik (2008) who analyses the impact of a random assignment of term lengths in the state legislatures of Arkansas and Texas. Her findings corroborate ours in that she tends to find stronger efforts–e.g., lower absenteeism–among legislators with longer terms.

Some of the main problems in the existing literature relate to endogeneity bias as well as to time effects, tenure effects, or cohort effects which confound the effect of a shrinking term length.[3] The ideal setting would allow for the observation of legislators elected at the same time, who differ only in their assigned term length. Our study investigates just that scenario.

We analyzed two different natural experiments involving two different chambers at two different points in time, and we found similar patterns and partly addressed issues of external validity. But much remains to be done for a comprehensive understanding of the effects of term length. One limitation of our study is it cannot say at what length—8, 10, or more years—further term extensions will discourage effort. Also, term lengths may have different effects in different countries. Our study covers only a single country under a proportional representation system, and we do not address the important issue of the quality of policy. Lastly, natural experiments—including our own—still face challenges in terms of

[2] Lott and Davis (1992) reexamine data for United States senators and find that the proximity of elections does not significantly affect voting behavior in the United States Senate. The general consensus in the profession since then has converged on the idea that voting patterns in the Congress of the United States are largely independent of electoral pressure (see Poole and Rosenthal 1997 and Lee, Moretti, and Butler 2004).

[3] Crain and Tollison (1977) consider governorships as investment projects and compare campaign expenditures across races for two versus four year positions. They find that campaign expenditures are larger for one race for a four-year governorship than for two races for a two-year governorship. The main potential confounding factors here are state effects, and the possibility that different term lengths may attract different candidates and different campaign contributions.

the exogeneity and nature of treatment. An important threat to identification relates to effects arising from the lottery per se, be they psychological or organizational in nature. Fortunately, various features of our data help allay some of these reservations.

The structure of the paper is as follows. In Section 2 we present a simple theoretical model to frame the empirical investigation. In Section 3 we describe the natural experiment in the House and present the data. In Section 4 we lay out the econometric approach, report the main results from the House, and discuss threats to identification and robustness. In Section 5 we present data and additional evidence from the Senate, and in Section 6 we investigate possible mechanisms behind the effects of term length. Section 7 concludes, and our proofs are presented in an appendix.

2 The model

As mentioned earlier, Barro's classic model (1973) predicts that more frequent elections induce less shirking (see also Ferejohn 1986 and Schultz 2008). We postulate a very simple model of legislative effort which is better suited to match the structure of our data as well as to capture additional effects.

We consider two classes of legislators: short-term legislators who face reelection after one period, and long-term legislators who face reelection after two periods. After their first reelection, members of each cohort have two period terms (matching our House study) at which point their prospects contingent on being reelected are identical and have value V.[4] Our exposition abstracts from dynamic programming aspects to focus on the (empirically observed) first period effort choices.

Legislators choose legislative effort l upfront in their term, facing a quadratic cost l^2, and they must also exert campaigning effort. There is a basic campaigning effort made by the party on behalf of each legislator facing reelection. As a result, incumbents not facing reelection must still exert some effort by collaborating with their party effort on behalf of those facing reelection, and this basic level is normalized to zero. Furthermore, legislators facing reelection at the end of a period may face an extra, individual campaign effort outlay c,

[4] Once our model is fully specified it becomes obvious that $V > 0$.

at a quadratic cost c^2. However, if the term is short, legislative and campaigning effort must be deployed in the same period, creating an extra cost $\sigma\rho(l+c)$ due to agenda congestion.[5] In keeping with the legislative sources we interviewed, the cost of simultaneously engaging in the two activities increases with the distance, $\rho \geq 0$, between the capital and the legislator's province. The parameter $\sigma \in \{0,1\}$ tracks the presence of individual campaigning: when $\sigma = 0$ campaigning is an evenly spread team effort; and when $\sigma = 1$, those facing reelection meet additional campaign commitments.

The legislative and campaigning efforts affect a legislator's probability of reelection $P(l, \sigma c, \pi) = \psi p(l, \sigma c, \pi) \in [0,1)$, which depends on a scalar parameter $\psi \geq 0$ and an electoral safety shifter π. We will typically obviate this latter argument to save on notation. The function $p(l, \sigma c)$ satisfies $p_l \geq 0, p_c \geq 0, p_{ll} \leq 0, p_{cc} \leq 0, p_{lc} \geq 0, p_\pi > 0$. In other words, we assume weakly decreasing marginal returns, and weak complementarity between the two forms of effort.[6]

Each unit of legislative effort yields a stream of unitary returns beginning in the current calendar period in the form of recognition, policy achievements, or legacy. If $h = 0$, we say the payback horizon is short because effort yields only a unit return in the current period. If $h = 1$, we say the horizon is long because the stream of returns lasts for two periods. (The stream under this long horizon can be made arbitrarily long at some extra notational burden). In this situation legislative effort embodies an investment, and its returns can also be interpreted as generating a lower cost of being active in the next period, as is the case when learning by doing occurs. Assuming legislators earn wages w per term, the respective programs for long and short-term legislators are,

$$\begin{aligned} L &: \underset{l_L, c_L}{Max} \left\{ V = -l_L^2 - \delta c_L^2 + w + l_L + \delta h l_L + \delta^2 P(l_L, \sigma c_L) V \right\} \\ S &: \underset{l_S, c_S}{Max} \left\{ V_S = -l_S^2 - c_S^2 - \sigma\rho(l_S + c_S) + w + l_S + \delta P(l_S, \sigma c_S)(V + h l_S) \right\}, \end{aligned} \tag{1}$$

where subscripts L and S denote respectively long and short-term legislators. These expres-

[5] A multiplicative formulation for the congestion costs is perhaps more natural. It yields similar results but imposes a heavier notational burden.

[6] This appears reasonable. If there is any interaction between the two forms of effort, it is more likely that campaigning might improve reelection chances if the legislator has good things to communicate about his legislative record.

sions capture the main ingredients of the model. Short-term legislators face reelection sooner, so the rewards from being reelected are discounted less heavily. But they also face higher costs from simultaneous legislating and campaigning, and when the stream of legislative returns is long they need to be reelected before they can capture the entire stream.

In order to isolate the effects of interest we characterize the solution under alternative parametric scenarios in the following,

Proposition 1 *i) (Accountability) If there is no role for individual campaigning ($\sigma = 0$), the returns to legislative effort are instantaneous ($h = 0$), and the more distant future is discounted more heavily ($\delta \in (0, 1)$), then short-term legislators exert more legislative effort than long-term legislators.*

ii-a) (Campaigning) If the returns to legislative effort are instantaneous ($h = 0$) but there is a role for individual campaigning ($\sigma = 1$) and the discount rate is either high or low enough, then long-term legislators exert more effort than short-term legislators.

ii-b) (Payback) If there is no role for individual campaigning ($\sigma = 0$), the returns to legislative effort extend into the future ($h = 1$), reelection is less than fully certain, and the marginal effect of legislative effort on reelection chances is low enough (ψ is low enough), then long-term legislators exert more effort than short-term legislators.

This proposition tells us that the comparison of legislative effort across short- and long-term legislators is ambiguous in principle. The discounting of more distant rewards (or punishments) discourages effort by legislators with the advantage of a long term. The role of discounting is similar both to the role of *accountability* pressure from election proximity which was highlighted by Barro (1973), and to the incentive effects of shortening the time between wage revisions noted by Cantor (1988). But two other forces may make short-term legislators work less on legislation. First, campaign commitments may crowd out legislative effort for short-term legislators (a *campaigning* effect); and second, short-term legislators may be less certain that they will be around to capture the full returns to their legislative effort (a *payback horizon* effect).[7]

[7] In the scenario which includes both campaigning and payback effects ($\sigma = 1, h = 1$), these effects

Two aspects of the payback effect are worth discussing at this point. The first is that a short term weakens incentives to exert legislative effort at the margin only if the marginal impact of that effort on the reelection probability is relatively low. This is because there are two forces at play. On the one hand, legislative effort raises the reelection probability. Thus, for a legislator with a short term an additional unit of effort increases his chances of capturing the future returns of the inframarginal units. This causes short terms to strengthen incentives to exert effort at the margin. On the other hand, when rewards accrue over time a short term makes it less likely that rewards will be captured in full, which weakens incentives. When the first, marginal effect of effort on the probability of reelection is relatively low, the second effect dominates.

The second aspect to emphasize is that in the payback parametric scenario $[\sigma = 0, h > 0]$ short-term legislators reduce the effort they put into an activity that is slow to pay. Because campaigning is zero in that scenario, it is not fully apparent that there are incentives to substitute one type of effort for another. In order to further develop intuition about the payback effect, it is instructive to rewrite the model with the parameter σ appearing only in the congestion costs expression. (This alternative formulation yields similar results throughout, but imposes a higher computational burden; so we return to the simpler version in the remainder of the paper). In the payback scenario $[\sigma = 0, h > 0]$ of this modified model, legislators engage in individual campaigning but there is no crowding out for short-term legislators. It is straightforward to obtain the expressions, $\frac{l_L}{c_L} = \frac{1+h+P_l(l_L,c_L)V}{P_c(l_L,c_L)V}$ and $\frac{l_S}{c_S} = \frac{1+P_l(l_S,c_S)(V+l_S h)+P(l_S,c_S)h}{P_c(l_S,c_S)(V+l_S h)}$, with the former being larger than the latter under the same conditions supporting $l_L > l_S$ in part *ii-b* of our last proposition. This means that when legislative effort yields returns over multiple periods short-term legislators substitute campaigning (a form of effort that pays quickly) for legislation (a form of effort that pays slowly). This form of the payback effect —obtained in a context of observable effort— resonates with the familiar substitution results in models of multi-tasking with unobservable effort

reinforce each other and depress legislative effort. The payback horizon effect bears resemblance to the impact of lengthening labor contracts when a firm must invest to develop the human capital of its employees and is able to monopolize its returns on such an investment (as opposed to sharing the returns with employees) only for the duration of the existing contract (see Cantor 1990).

(Holmstrom and Milgrom 1991, Dewatripont, Jewitt and Tirole 1999). In our model the driver is not the precision with which different types of effort can be observed, but the speed with which they pay. Term length alters the relative attractiveness of activities when their payback horizons differ.[8]

Whether the campaigning or payback effects can, in practice, overcome the accountability effect driven by discounting is an empirical question. The legislators we interviewed deemed plausible both the campaigning and the payback channels.

3 The natural experiment in the Argentine House of Representatives

3.1 Background

Argentina is a federal republic consisting of twenty four legislative districts: twenty three provinces and an autonomous federal district. The National Congress has two chambers, the Chamber of Deputies (i.e., the House of Representatives) and the Senate.[9] Argentina experienced a return to democracy in December of 1983, following a period of military rule. At that time, all 254 deputies were elected, and they began their terms on December 10, 1983. The Argentine Constitution stipulates four-year terms, no term limits, and the renewal of half the House every two years, so to stagger the renewal, half of the representatives elected in 1983 were allotted two-year terms. The allocation of two and four-year terms was by random assignment.

The procedure for the random allocation of terms set by the Comisión de Labor Parlamentaria (the equivalent of the Rules committee in the United States) involved dividing the representatives into two groups of equal size, Group 1 and Group 2. Each party-province delegation apportioned an equal number of its members to each group (see Table 2).

[8] Cantor (1988) examines incentive effects of contract length in a moral hazard context where wage revisions are periodic and costly. Cantor (1990) studies how contract length affects complementary investments in human capital by firms and employees. Effort is unobservable in both models.

[9] For descriptions of the Argentine Congress see Molinelli, Palanza, and Sin (1999) and Jones et al. (2007).

The procedure for the random allocation of terms, set by the *Comisión de Labor Parlamentaria* (the equivalent of the Rules committee in the United States) involved dividing the representatives into two groups of equal size, Group 1 and Group 2. Each party-province delegation apportioned an equal number of its members to each group (see Table 2).[10] During a public session on January 20 of 1984, the *Secretario Parlamentario* performed a lottery draw, which gave legislators in Group 1 a four-year term and legislators in Group 2 a two-year term.[11]

[TABLE 2 ABOUT HERE]

The party-province delegations did not know which group would get assigned the long term when apportioning legislators to groups 1 and 2. One might be concerned that if delegations were to systematically assign the better legislators to one particular group, then the assignment of term lengths would not be exogenous. But behind a veil of ignorance, there would be no reason for risk-averse legislators to introduce any imbalance, so we do not regard this aspect of the design as problematic. Moreover, the majority of party-province delegations (75 percent) did the assignment in a way that was essentially random. They assigned legislators occupying an odd-numbered position in the 1983 electoral party list to one group, and those occupying an even-numbered position to the other. Positions in the ticket (i.e., whether a legislator is close to the top or not) depend largely on the demographics of the province area to which the legislator belongs. A second factor affecting list positions is the perceived popularity of a candidate in her area. Thus, whether a legislator is first or tenth in her party list is not random, but whether she falls in an even- or odd-numbered position is. The remaining 25 percent of the delegations did not follow this odd vs. even slot procedure to assign legislators to groups, but by all observable measures their assignment also appears

[10] The two representatives from Tierra del Fuego (the smallest district) were allocated to the same group. In the case that a party had an odd number of representatives from one province the imbalance was corrected with the analogous surplus from another province where the party also had an odd number of representatives.

[11] Since the return to democracy in 1983, the two dominant political parties in Argentina have been the Unión Cívica Radical (UCR, or Radical party) and the Partido Justicialista (PJ, or Peronist party). In the period under analysis, the majority party was the Unión Cívica Radical.

random. As will be shown, our findings are robust to eliminating these delegations from the sample and to clustering standard errors at the party-province level.

As shown in Table 3, there are no statistically significant differences in observables across the two groups of legislators according to a difference in means test, which again suggests that the randomization was successful.[12]

[TABLE 3 ABOUT HERE]

We perform two additional balancing tests. In the first place, we regress the probability of being assigned to Group 1 on the set of pre-assignment characteristics and find that these characteristics are not significant predictors of assignment. (The F statistic p-value is 0.54.) And secondly, we run a regression of the index of legislative effort on the set of observable characteristics, compute the predicted effort, and then regress this predicted effort on a dummy variable that is equal to one for those legislators who were assigned the long term, and zero otherwise. We find no significant link between term assignment and the part of effort driven by observable characteristics. (The coefficient on the long-term dummy is not significant; the p-value is equal to 0.14.) The results of these tests provide additional assurance that the allocation of terms was random.

3.2 Data and measures of effort

Our dataset contains yearly information on individual effort and legislator characteristics for the period February 1984 - December 1985. Of the 254 legislators who began their term in December 1983, three resigned and five died before December 1985. Thus the sample includes 492 observations corresponding to 246 legislators over two legislative years.

The Argentine Congress made available six objective measures of legislative effort by a legislator: floor attendance (as percentage of legislative floor sessions), committee atten-

[12] One may worry that the aggregate data masks a systematic unbalancing within parties which cancels out in the aggregate, and that perhaps one imbalanced party might spuriously drive results. We ran balancing tests for the two main parties separately and found no pattern of systematic difference. Only one observable is unbalanced for one party (the fraction of lawyers for the Partido Justicialista). As we show later, the effect of term length holds for each separate major party.

dance (as percentage of committee sessions), the number of committee bills in which the legislator participated (as reflected by the committee bills bearing the legislator's signature), the number of times the legislator spoke on the floor on a legislative topic, the number of bills introduced by the legislator, and finally, the number of those bills that were approved.

To complement our understanding of the functioning of the Argentine Congress, we interviewed the six legislators identified in our acknowledgements. These individuals were identified through personal contacts and in no way constitute a representative sample.[13] However, their opinions were highly consensual, and we believe it is worthwhile to share them.

The legislators interviewed believe the metrics that we obtained capture different types of legislative effort. They valued the diversity of measures because different legislators have different profiles. Some legislators may seek to capture the attention of constituents by introducing high numbers of bills, while others may care more strongly about policy and, as a result, may focus more on the approval of bills or on committee work. (The measure of bills approved is also valuable as a proxy for impact on legislation.) More generally, floor attendance will reflect involvement with the daily legislative business.[14] Table 4 shows the correlation matrix of the measures we use. Several correlations are weak and in some cases negative. In light of both this and the feedback from legislators, we believe the metrics we use, while noisy, do serve as proxies for different and relevant dimensions of legislative effort. One indication of their relevance is that these metrics are significant predictors of reelection.[15]

[13] We followed a predetermined interview plan. We first asked these legislators their assessment of the effort metrics made available by Congress, and later we discussed the issue of term length. In order not to bias their feedback, we reminded them of the randomization of term lengths and waited for their unprompted analyses of the intervening mechanisms and consequences.

[14] To illustrate how attendance may capture different forms of involvement, consider the case of César Jaroslavsky (UCR, Province of Entre Ríos). Representative Jaroslavsky was not involved in specific committee work and did not introduce many bills of his own. Yet he played a central political role as majority leader, was present in over 94% of all floor sessions, and placed second in the overall attendance ranking.

[15] We investigated probit regressions where the dependent variable is a dummy equal to one if the legislator is reelected, and the effort metrics are the main independent variables. Our preferred specification has the effort index (with collapsed data by legislator) as the main independent variable. The linear term of the

[TABLE 4 ABOUT HERE]

In order to draw general conclusions in a context of multiple outcomes, we construct an index of legislative effort that aggregates the six measures described above. The index is the equally weighted average of the yearly z-scores of its components (see Kling, Liebman, and Katz 2007). The z-scores are levels standardized using the mean and standard deviation for the two-year legislators. In all cases higher effort measures have higher z-scores.

The variable of interest is *Four-year term*, an indicator variable equal to one for legislators assigned to an initial four-year term and zero otherwise. Our data includes various legislator characteristics, such as age (as of November 1983), the distance in kilometers from the capital of the legislator's province to Buenos Aires, and a series of dummy variables equal to one when the legislator: is male, is a lawyer, is a first time national legislator, holds a university degree other than lawyer, occupies a leadership position (i.e., president of the chamber, chair of a committee, and majority or minority leader), belongs to the majority party, and belongs to a small block (i.e., less than four legislators).

Representatives in Argentina are elected through a closed party list at the province level. Under this system the degree of electoral safety depends on how high up on the party ticket a legislator ranks. For example, in 1983 the UCR had 19 candidates running for the seats corresponding to the province of Santa Fe. But given the party's vote share and the proportional representation system, only the top ten members were seated. Those legislators close to the tenth position in the ticket faced risk going forward, given that the party's electoral strength might erode, and that the legislator's ranking in a future party list depends largely on relatively permanent factors, such as the demographics of the legislator's home area. We develop a dummy variable (*Slackness*) to capture electoral safety. We say a legislator is safe if she entered Congress within the top half of her party-province delegation (in our example, in the top five slots), in which case *Slackness* is equal to one. We say she is relatively at risk otherwise, in which case *Slackness* is equal to zero. As we would expect,

index is strongly significant across all specifications we tried, and the quadratic term is not significant, which suggests that reelection is a roughly linear function of effort.

the legislators for whom the variable *Slackness* is positive have a significantly higher chance of reelection.

4 Econometric model and results

Given random assignment, the causal effect of assignment to an initial four-year term relative to serving an initial two-year term can be estimated by using the following regression model:

$$Y_{it} = \alpha + \beta \mathit{Four\text{-}year\ term}_i + \gamma X_i + \mu_t + \varepsilon_{it} \qquad (2)$$

where Y_{it} is any of the effort measures under study for legislator i in period t (where $t = 1984, 1985$, the two years following the assignment), β is the parameter of interest, X_i is a matrix of time-invariant legislator characteristics, μ_t is a year fixed effect, and ε_{it} is the error term.

Table 5 reports estimates of β when the dependent variable is the index of legislative effort.[16] Results with and without controls indicate that legislators serving a four-year term work harder than those serving a two-year term and that the difference is statistically significant. The size of the estimated effect is a third of a standard deviation of the effort distribution. The size of the effect appears considerable relative to the effects of other observable characteristics. For instance, the effort difference between the long and short-term legislators is more than one and a half times the effect of a university degree, almost one and a half times the impact of being a legislative leader, and roughly the same as the effect of being in the majority party. This underscores the strength of the term length effect.[17]

[TABLE 5 ABOUT HERE]

To determine whether the effects are wide-ranging or concentrated in just one or two outcomes, we estimate and report in Table 6 the effects on each separate effort metric.

[16] A typical concern when conducting inference for the estimated parameters of equation 2 is that the errors for the same legislator might not be independent. To address this concern we cluster standard errors at the legislator level, and we later report a specification using data that has been collapsed by legislator.

[17] For example, belonging to the majority party has been estimated to play a sizeable role in legislative effectiveness by Padró-i-Miquel and Snyder (2006).

The effect of term length appears quite general. The point estimates are positive for all six metrics and statistically significant for three of them. The p-value for a fourth metric (committee bills) in the controlled regression is 0.105.[18] [19] Figure 1 shows box-and-whiskers plots comparing the two groups on each measure of effort, which gives a view of the whole distribution of effects.

The differences in effort tend to be important in size. Focusing on mean effects in the controlled regressions, we see from Table 6 that getting a longer term significantly increases floor attendance by 3 percent (relative to the mean of the two-year legislators). This is a nontrivial effect; as a point of comparison, Lott (1987) estimated a 6 percent decrease in voting frequency for retiring legislators. Committee attendance is 12 percent higher for long-term legislators, and the number of committee bills bearing the legislator's signature goes up by 19 percent. Lastly, floor speaking appears to respond by 13 percent to a longer term although this result is far from significant.

The idea that longer terms increase effort also appears to be backed by the measures of "bill production." The point estimate in column (10) in Table 6 indicates that the number of bills introduced goes up by 20 percent. This estimate, however, like that for committee bills, loses significance when clustering the standard errors (p-value of 0.17). When we switch attention from the "volume" measure of bill production to the "legislative impact" measure, namely the number of bills that pass, the estimates become strongly significant. The point estimate in column (12) in Table 6 indicates that moving from a two- to a four-year term almost doubles the number of bills passed.[20] Ferraz and Finan (2008) find a 22

[18] The variables committee bills, floor speeches, bills introduced, and bills ratified take discrete values and are strongly skewed to the right with many observations at zero; thus, ordinary least squares estimation would be inappropriate. For all of these variables we were able to reject the hypothesis that the dispersion parameter is equal to zero according to a likelihood-ratio test, which suggests that a more appropriate specification, as adopted here, is a negative binomial model for count data.

[19] One may conjecture that the years 3 and 4 of four year legislators are similar to the years 1 and 2 of the two year legislators, so the former should display lower effort in their last two years. This conjecture is true (results available upon request), although one must keep in mind the result could be driven by time effects.

[20] We explore an alternative definition of bill production that considers not only the bills a legislator introduced but also those that she endorsed and obtain similar results.

percent increase in bills approved when Brazilian local legislators receive wages that are 20 percent higher. Thus, the productivity effect of a two-year term extension is comparable in magnitude to the effect of substantial wage increases. Overall, our results indicate a strong tendency for longer terms to increase legislative effort.

[TABLE 6 ABOUT HERE]

[FIGURE 1 ABOUT HERE]

4.1 Robustness checks

To further address the issue that observations are not independently drawn, we create a single observation for each legislator by averaging the two yearly observations and then cluster the standard errors at the province level. As reported in columns (1) to (7) in Table 7, our conclusions remain unchanged for the index and the individual metrics of effort. The magnitude of the effects is similar and the estimates are strongly significant for four of the six metrics of effort. The estimated effect on the index of effort is preserved when we compute the index by dropping one effort measure at a time (results unreported and available upon request). In column (8) in Table 7 we show estimates of the principal component (which accounts for 33 percent of the total variance). Again, the estimated coefficient of the *Long term* dummy variable is positive and significant at the one percent level.

[TABLE 7 ABOUT HERE]

In Table 8 we report additional estimations under a wide range of alternative specifications and samples. First, the significance of the term length variable is not affected when we cluster standard errors at the province level, or according to party-province combinations. Second, our conclusions remain unchanged when we restrict the sample to those party-province delegations that used the even/odd rule to assign legislators into the two groups, and when, additionally, we also exclude the first two legislators in the party list. (We take this extra step because the difference between occupying an even vs. odd position is generally random but one could argue this may not apply to the top two positions in the party list). Third, we run separate regressions for the two main parties in order to explore possible heterogeneity in the effect of term length according to political party. Despite the smaller sample size, we still find a positive and significant association between term length and legislative effort for legislators of both parties. While the point estimate is larger for Peronists, the difference between the two estimates is not statistically significant. Finally, the value and significance of the coefficient of interest remains unchanged when we exclude from the sample legislators that were leaders or those few who changed leader status during the sample period.[21]

[TABLE 8 ABOUT HERE]

4.2 Potential concerns

Even when our study relies on a well documented randomization, one can still harbor some potential concerns regarding the exogeneity and nature of the treatment. First, it could be the case that re-optimizations took place after the random assignment, which might have affected effort for reasons other than the change in term length. For example, legislators given a four-year term could have obtained better committee assignments. Thus, in the presence of hierarchical re-optimization, the conclusion that lengthening terms is a good idea would not follow if such an extension were to benefit all legislators. Our experiment is quite unique because, as is well documented, all committee assignments, leadership positions, and placement along the internal hierarchy of the chamber were decided before the assignment of terms was done. Very few re-allocations are observed after the random assignment, and they appear unrelated to term length.[22] The results remain unchanged when we exclude from the sample those legislators who changed status as chamber leaders or moved in or out of the most important committees (see the last column in Table 8).

[21] We experimented with different definitions of leadership and always found similar results.

[22] Only seven legislators left the most important committees after the random allocation of terms (four two year and three four year legislators). Of the seven substitutes, three legislators ended their term in 1985 and four in 1987. Of all legislators who are considered leaders, only two left their position before December 1985 (one two year and one four year legislator). One of the substitutes ended his term in 1985 and the other in 1987.

A second story is one where parties may direct opportunities and responsibilities to the long-term legislators, discriminating against the others. This story also posits that collective arrangements may confound (even replace) the role of individual incentives, and that the effects of the treatment are not unrelated to the relative size of the treated group. Such collective arrangements might be especially prevalent in a proportional representation system. While our data do not allow a direct test for such a possibility, several observations are called for. First, the most likely way that such a centralized process would work is through a manipulation of the positions within the internal hierarchy of the House. However, as discussed above, this hierarchy was determined before terms were assigned. Second, there is a strong indication that there is a link between individual incentives, individual actions, and individual career outcomes because individual effort is a significant predictor of reelection.[23] Third, if long-term legislators enjoyed advantages that make them more powerful or well known, they should enjoy higher reelection chances conditional on effort. However, after controlling for effort the long-term legislators do not enjoy higher reelection rates (a caveat on this result is that differences in reelection rates are affected by time effects). Fourth, we will show below that the data matches further predictions of our model driven by an individual logic that is independent of collective arrangements or the treatment status of others. And fifth, our interviews failed to substantiate the notion that such centralized processes play a role in the Argentine Congress. The view of legislators is that effort choices are essentially an individual decision.

The views of legislators are of interest given Argentina's proportional representation system. There are additional reasons for us to believe that the results are not dependent on this proportional representation factor. One of these reasons is covered in the following section, where we study evidence from the Senate, where races are much closer to uninominal.

[23] As stated before, the effort index is a significant regressor in the reelection probit. In a specification with controls, the coefficient is 0.45 (p-value 0.016), which translates into a marginal effect of 0.14. A move from 0 to 1 in the effort index buys 14 percentage points in reelection probability. The mean difference in the effort index between long- and short-term legislators is 0.2, so the term length effect is roughly an increase of 2.8 percentage points in the reelection probability. Although not a large effect, this is not trivial when considering the low baseline reelection probabilities in Argentina.

The other is that we would expect proportional representation features to be stronger in larger province delegations, where individual politicians are less well known and potentially more dependent on the party. However, having experimented with various caps on delegation size, we have found the effects to be independent of delegation size.

A third, and perhaps more important, threat to the interpretation of the effects is that the outcome of the lottery may directly affect the morale of legislators, boosting the spirits of those who received four-year terms, and depressing the rest. In this case, the lottery instrument would not be affecting behavior through its effect on term length, but directly through a "win" or "loss" connotation. According to the literature in experimental psychology (see for instance Amsel 1992), an implication of the "altered morale" hypothesis is that we should observe the effort of legislators given two-year terms lagging in the early months, then quickly recovering as spirits revert to normal. As shown later in column 3 of Table 11, the strength of the effects is statistically indistinguishable across the two sample years. In addition, in our working paper (Dal Bó and Rossi 2008) we examine the effort differential across groups on a monthly basis and cannot support the idea that four-year legislators do better only in the first few months. This suggests that the effect of the lottery-based assignment of terms is linked to incentives rather than to the lottery having affected morale.

5 Data and additional evidence from the Senate

As a result of a constitutional reform in 1994, the whole Senate needed to be renewed in 2001, when all of the body's 71 members were elected and began their terms on December 10.[24] The modification of term lengths and renewal rates required that some senators be assigned two-year terms, others four-year terms, and others six-year terms. The allocation was done through a well documented random assignment during a public legislative session on December 12 of 2001. All three senators from each province were jointly and randomly placed on a two-, four-, or six-year track. One implication of this design is that we cannot

[24] There were 71 senators (instead of 72) because one of the three seats belonging to the federal district was left vacant until 2003. As with House representatives, senators are eligible for reelection and face no term limits.

use province dummies, although we can control for distance.

Examining the Senate episode is important for external validity reasons. It allows us to focus on a different chamber at a different historical juncture. Moreover, the Senate offers a setting where the idiosyncracies of the party-list cum proportional representation system are more diluted. There are only three senators per province; they are very well known and represent the entire province. The majority party gains two of the seats and the minority the third. The high recognition together with the extremely short list make senatorial races resemble those in a uninominal system.

In keeping with the House experiment where the short-term legislators had a two-year track, we will begin by comparing senators in the two-year track against the rest; accordingly, we will define the *Long term* variable as a dummy equal to 1 if the senator got a four- or six-year term, and zero otherwise. Table 9 exhibits the summary statistics, showing that the predetermined observables are largely balanced. The only observable that shows a significant difference between the short- and long-term legislators is distance, which is not an individual but a delegation characteristic. This is not incompatible with chance when measuring nearly ten observables over two different experiments. Moreover, we ran further balancing tests as was done with the House data and found no evidence that either observable characteristics or predicted effort could predict assignment.

[TABLE 9 ABOUT HERE]

Our dataset for the Senate contains yearly information on legislative effort and legislator characteristics for the two-year period starting in December 2001 and ending in December 2003. We could obtain only three objective measures of legislative effort: floor attendance, the number of bills introduced by each legislator, and the number of bills approved. Of the 71 legislators that started their term in December 2001, six resigned before December 2003. Thus the sample includes 130 observations corresponding to 65 legislators for two years.

Again, in order to draw general conclusions in a context of multiple outcomes we use an index of legislative effort. In column (1) in Table 10 we show results showing effects that are about twice as large than those in the House.[25] Columns (3) and (4) show a similar picture when the data are collapsed at the legislator level; the model reported in column (4) has as dependent variable the first principal component of the individual effort measures (which accounts for 57 percent of the variance). When we include a full set of controls the point estimates from these specifications remain broadly unchanged but with our sample size they lose significance, as exemplified in column (2). Exploring the individual metrics of effort (columns (5)-(7)), we find that the change caused by a longer term over the mean effort of the two-year senators is of 2 percent for floor attendance (an effect similar to that in the House), 49 percent for the number of bills introduced, and 27 percent for the number of bills passed. As shown in Table 10, only one of these differences is significant. However, the differences in all three metrics favor of the long-term legislators, which is reassuring. The effect on bills introduced is statistically significant and its magnitude is large. To provide an idea of the potential money value of the effect, note that Ferraz and Finan (2008) estimate that a 20 percent wage raise for local legislators in Brazil increases bills submitted by 25 percent. The effect of extending terms for senators in Argentina appears to be twice that of a 20 percent increase in wages in Brazil.

Overall, the picture from the Senate obtained so far corroborates the one we obtained from the House.

[TABLE 10 ABOUT HERE]

6 Investigating mechanisms

6.1 Campaigning and payback

Legislators with longer terms appear on average to exert more effort both in the House and the Senate. According to our model in Section 2 that effect could arise under two very

[25] When we consider three treatments categories (two, four, and six year terms) we find that the point estimate of being assigned to a four year term is positive but smaller than the one associated to being assigned to a six year term. In other words, effects appear to get stronger the longer the term assigned. We return to the comparison between four and six year senators later.

different parametric scenarios. In one, campaigning may keep short-term legislators busier (in terms of the model, $\sigma = 1$) and in the other legislative effort may take time to yield returns (in terms of the model, $h = 1$). To investigate whether these forces could be at play, in this section we rely further on our model and our data.

Test 1 - Campaigning vs. payback - Senate

A straightforward test is possible in the context of the Senate. We can compare four- vs six-year senators during their first two years in office, when none of them are campaigning. In terms of the model, that amounts to $\sigma = 0$. It follows from Proposition 1 that i) If campaigning is solely responsible for the effects identified in sections 4 and 5, then the six-year senators should not work harder than four-year senators, and ii) For the six-year senators to work harder than the four-year ones, it is necessary that the payback channel be present, i.e., that $h = 1$.

We now define the *Long term* variable to be equal to 1 for six-year senators and zero for the four-year senators. Columns (1) and (2) of Table 11 report estimates obtained through the exclusion of all senators in the two-year track from the sample; column (1) corresponds to yearly observations and shows a point estimate that is substantial in size and with a p-value of 0.057. Column (2) corresponds to observations that have been collapsed by legislator. With lower power, the p-value becomes 0.101. Taken together these results favor the view that during the first two years of their terms six-year legislators exert more effort than four-year legislators. This suggests that long-term legislators work harder because of a concern with payback and not merely due to campaigning distractions.

Test 2 - Campaigning vs. payback - House

Given the lack of six-year terms, the preceding test is not feasible in the House. But a related test is feasible if we exploit the intra-term variation. If campaigns crowd out legislative effort during the months immediately preceding the election, it should be true that the effort differences between representatives arise mainly in the second of the two sample years. Thus, an interaction term between the *Long term* variable and a dummy for the second year should be positive and significant. Column (3) in Table 11 reports estimates of the effect of the long-term assignment and the interaction effect in the House. We fail to find evidence that the effort differential in favor of the long-term legislators widens in the second year.[26] This again speaks in favor of the payback hypothesis and against the campaigning story.

Test 3 - Campaigning - House

We now explore the campaigning hypothesis in the House in an alternative way. The legislators we interviewed indicated that campaigning commitments arrive as the election nears; they also told us that campaigning, which requires presence on the legislator's home turf within the province, poses a larger conflict with legislative effort for those representing geographically remote provinces. Consequently, according to the legislators we interviewed, if campaigning distractions drive the effect of term length, this effect should be stronger among representatives from more distant provinces.

We can use our model to study the impact of increases in distance ρ on the effects of a longer term. We consider the model under the parametric scenario $[\sigma = 1, h = 0]$ where campaigning drives results and study the comparative statics of ρ on l_S and l_L.

Proposition 2 *If campaigning poses additional commitments for those facing reelection ($\sigma = 1$) and legislative effort does not yield rewards in the future ($h = 0$), the effect of a longer term on legislative effort (i.e., the difference $l_L - l_S$) increases with geographic distance ρ.*

According to this proposition, if campaigning drives term length effects, the coefficient of the interaction between the *Long term* dummy and distance should be positive. Columns (4) and (5) of Table 11 report results that correspond respectively to regressions using yearly observations and collapsed data, and reveal that the interaction has the wrong sign and is not significant (the p-values are 0.53 and 0.51, respectively). We again fail to find support for the campaigning hypothesis in the House, but this does not imply we can completely rule it out. Given the coefficients and the standard errors reported in columns (4) and (5), it is conceivable that an effect at the top of the confidence interval would produce some campaigning-driven effects. But it is still noteworthy that three different implications of the

[26] When the sample is restricted to the first year of the term, the estimated coefficient for *Long Term* is 0.227 with a standard error of 0.069. (This coefficient is statistically significant at the one percent level and equivalent to that obtained in the unrestricted sample.)

campaigning hypothesis, one in the Senate and two in the House, fail to find support in the data.

Because campaigning is an activity that demands time, it seems *prima facie* counter-intuitive that it would not appear to differentially damage the short-term legislators. One possibility is that campaigning does not really pose a stronger conflict with legislation for those representing more remote districts—in which case the distance-based test would be misguided. But the legislators we interviewed appear to be correct when they assert that campaigning is costlier for representatives from remote provinces.[27] As indicated by the legislators we interviewed, another possibility is that campaigning in Argentina is to a great extent a team effort at the party level. Legislators who are not running for office often campaign alongside those who are, which in the language of our model would yield $\sigma = 0$.

[TABLE 11 ABOUT HERE]

Test 4 - Payback - House

We now investigate the presence of the payback effect in a different way. We analyze the consequences of variation in the measure of electoral safety π, under the null $\sigma = 0$ which we have been unable to reject. We focus on the payback scenario (where $[\sigma = 0, h > 0]$), and study the comparative statics of the electoral shifter π on the two groups of legislators. Comparative statics effects involve both direct and indirect effects, and the exact magnitude of the latter depends on curvature features. So for this exercise we impose an additional assumption, namely that the function $P(l, \pi)$ should not be too concave in l.[28] We can now state,

Proposition 3 *If campaigning does not pose additional commitments to those facing reelec-*

[27] For example, to take a metric directly related to physical location, floor attendance diminishes more in the second year for legislators from remote districts (e.g., 1,500 kilometers from the capital), which indicates that that the toll of campaigning is statistically detectable. But campaigning in more remote districts does not significantly affect the size of the term length effect on floor attendance, and this suggests that the term length effect is not driven by campaigning. We thank a referee for suggesting this check.

[28] This is compatible with the fact reported earlier: the probability of the reelection of the representatives in our sample increases with legislative effort, and the coefficient of the quadratic term is not significant.

tion ($\sigma = 0$), legislative effort yields rewards in the future ($h = 1$), and our assumptions on the function $P(l, \pi)$ are satisfied, the effect of a longer term on legislative effort (i.e., the difference $l_L - l_S$) is decreasing in electoral safety π.

This proposition captures the intuition that if the payback horizon hypothesis is true, legislators who are electorally safer should care less about term length than do those at greater risk. In the extreme case of a legislator whose reelection is guaranteed, term length does not affect the expected time in office. Thus, a two-year and a four-year term yield the same expectation of capturing the returns on effort investments. At the other extreme, in the case of a legislator who is certain not to be reelected, the term length extension does in fact dramatically affect the expectation of payback from partial to complete. Thus, if our main findings are driven by the payback horizon channel, an interaction variable between having greater safety and being assigned to a longer term should be negative.

The variable capturing electoral safety, *Slackness,* was introduced in Section 3. In columns (6) and (7) of Table 11 we report results supporting the idea that the interaction of the *Long term* variable and the electoral safety measure for House members does indeed lower the impact of a longer term. The interaction term is negative with p-value of 0.102 in the specification with yearly data and standard errors clustered at the legislator level, and 0.028 in the specification with collapsed data and standard errors clustered at the province level. In other words, the effects of term length appear stronger among "at risk" legislators. Moving from the bottom half to the top half of one's party-province delegation undoes two thirds of the effect of being dealt a longer term. These results suggest that the payback horizon plays a role in the term length effects found in the House, and that legislative effort embodies an investment. This further backs the view that the effects of term length are driven by an individual calculus rather than by collective arrangements.

We believe the investment logic is plausible given the time structure of legislative activity. In our sample period, even abstracting from the time spent preparing the bill, the mean time lag from the introduction of a bill to its approval was 327 days. Likewise, a legislator will often have to decide whether to spend time absorbing information that will be useful while a policy issue remains current.[29] Alternatively, a legislator may buy an apartment close to the legislature in order to lower the future costs of attending meetings, or shut down a private law firm to more fully focus time and attention on legislation. These costs are slow to amortize and legislators with shorter terms may decide not to incur them. Furthermore, a change from a two-year to a four-year term should significantly affect the effective payback horizon facing Argentine legislators: reelection rates in Argentina have been traditionally low (around 25 percent for our sample).[30]

We should make an important caveat at this point: the maintained assumption in this exercise is that the slackness variable is exogenously driven by the demographic characteristics of the part of a province from which a legislator originates. Although legislators maintain that such demographics are an important exogenous determinant of list placement, legislators in different parts of the party-province list are not necessarily comparable, so identification in this exercise is less reliable than in our main results.

6.2 Does experience matter?

Lastly, we inquire about the nature of the potential investments involved in legislative activity. This is useful from the perspective of the optimal design of terms. One possibility is that investments, once accumulated, render legislators unresponsive to term lengths. This would be the case for instance if longer terms affected effort because they foster learning by doing about general legislative procedures which does not depreciate. Another possibility is that investments depreciate and new investments must be made. If the first possibility were true, the investment logic would only be relevant early on in a legislative career. Then it might be optimal to allow inexperienced legislators a long first term in order to incentivize initial

[29] Some policy issues may have a long life-cycle and an effort investment may pay over a long period of time. As one legislator put it to us (our translation from the Spanish), "The library of Congress is vast...you can do a doctorate in here, as some of the committees deal with really complex issues. Obviously, if you are going to be around for longer, you get into it more..."

[30] The average reelection rate for the 1983-2001 period was 20 percent (see Jones et al. 2002). The reelection rates for the years 1985 and 1987 were 30 percent and 22 percent respectively, and are not statistically different.

investments, while having more senior legislators face shorter terms in order to benefit from stronger accountability. If the second possibility were true, term lengths could be determined as they are now, without regard to seniority.

In order to explore what type of investment predominates, we ask whether the effects of term lengths are stronger for inexperienced senators. We rely on the 2001 Senate data because, given the dictatorship during the period from 1976 through 1983, in 1983 there were only a handful of House members with previous legislative experience. In columns (8) and (9) of Table 11 we estimate specifications including an interaction between the *Long term* variable (four- and six-year terms) and the *Freshman* variable for senators. If once-and-for-all investments do in fact drive the investment logic, we would expect experienced senators to care less about what term length they get. In other words, we would expect the interaction between the *Freshman* and the *Long term* variables to be positive and significant. We find that interaction is not significant and that it has the incorrect sign in the model with controls. We conclude that investments either depreciate after a few years or that they are related to varied and continuing opportunities which are also valuable for experienced legislators. As a result, we find no support for determining term lengths with regard to seniority.

7 Conclusion

Term length is a fundamental aspect of constitutional design. We frame our empirical investigation with a theoretical model that emphasizes competing forces. On the one hand, longer terms push the reward of reelection further into the future (a weakened accountability effect). On the other hand, longer terms may free legislators from campaigning as well as increase the chance that they will be around to benefit from their past legislative involvement (the campaigning and payback effects, respectively). Results from two natural experiments in the Argentine House and Senate, where the length of terms was randomized, suggest that the accountability effect is dominated by one or both of the contrary effects, namely campaigning and payback.

In this paper we take steps not just to investigate whether term lengths matter, but also to gain insight on how and why they matter. With this aim, we perform a series of tests. We do not expect a single exercise to provide a final interpretation, but taken together our results indicate that the effects of term length are unlikely to come as a result of campaigning crowding out legislative effort. Instead, our data supports the idea that the payback horizon matters to legislators. In the context we study, incentives seem to be strengthened by job stability, which our model predicts can happen when effort embodies an investment yielding its return over multiple periods.

The issue of term lengths, and more generally the benefits of job stability, is relevant not only to public office but also to the private sector, where both incentives and the accumulation of firm-specific human capital are important. Surprisingly, there is a dearth of empirical work at the micro level which might neatly identify the effects of a guaranteed longer tenure on employee incentives. Presumably, this is the result of the identification difficulties that to date have hindered analogous studies of politics. We hope that our approach can provide a blueprint for studies of other political settings and of labor relations and organizational behavior more generally.

8 Appendix

Proof of Proposition 1: The first order conditions for the problems in (1) are,[31]

$$\begin{aligned}
l_L &: -2l_L + 1 + \delta h + \delta^2 P_l\left(l_L, \sigma c_L\right) V = 0 \\
c_L &: -2c_L + \delta P_c\left(l_L, \sigma c_L\right) V \leq 0, c_L \geq 0, \text{ with complementary slackness} \\
l_S &: -2l_S - \sigma\rho + 1 + \delta P_l\left(l_S, \sigma c_S\right)\left(V + l_S h\right) + \delta P\left(l_S, \sigma c_S\right) h = 0 \\
c_S &: -2c_S - \sigma\rho + \delta P_c\left(l_S, \sigma c_S\right)\left(V + l_S h\right) \leq 0, c_S \geq 0, \text{ with complementary slackness.}
\end{aligned}$$

The assumptions made on the cost and reelection probability functions guarantee that $l_S, l_L > 0$ in all scenarios.

i) Under $\sigma = 0$ there is no role for individual campaigning, so $c_S = c_L = 0$; given $h = 0$ the first order conditions for l are $-2l_L + 1 + \delta^2 P_l\left(l_L, c'\right) V = 0$ for long-term legislators and

[31] The second order conditions are straightforward and require that the sensitivity of reelection ψ and the complementarity between c and l not be too high.

$-2l_S + 1 + \delta P_l(l_S, c')V = 0$ for short-term legislators. The extra discounting in the former expression implies $l_S > l_L$.

ii-a) Under $[\sigma = 1, h = 0]$ the first order conditions become $-2l_L + 1 + \delta^2 P_l(l_L, c_L)V = 0$ (for l_L), $-2c_L + \delta P_c(l_L, c_L)V = 0$ (for c_L), $-2l_S - \rho + 1 + \delta P_l(l_S, c_S)V = 0$ (for l_S), and $-2c_S - \rho + \delta P_c(l_S, c_S)V = 0$ (for c_S). Note that for δ close enough to 0 or 1 discounting is the same for both types of legislator. The only difference in the first order conditions is the higher marginal cost facing short-term legislators. Thus, as long as δ is close enough to 0 or 1 weak complementarity becomes a sufficient condition for $l_S < l_L$.

ii-b) Under $[\sigma = 0, h > 0]$, we have $c = 0$ for both long and short-term legislator and the respective first order conditions become $-2l_L + 1 + \delta h + \delta^2 P_l(l_L)V = 0$ and $-2l_S + 1 + \delta P_l(l_S)(V + l_S h) + \delta P(l_S)h = 0$. Under $\delta > 0$, the marginal cost of legislative effort is the same for long and short-term legislators, but the marginal benefit is higher for the former if $\frac{\delta}{\psi}h + \delta^2 p_l(l_L)V > p_l(l_S)(V + l_S h) + p(l_S)h$, which holds if ψ is low enough.■

Proof of Proposition 2: Note the first order conditions for the long-term legislator are invariant in ρ, so we only need to show that $\frac{dl_S}{d\rho} < 0$. Differentiating the first order conditions of the short-term legislator with respect to ρ, while considering l_S and c_S as implicit functions of ρ, and then solving for $\frac{dl_S}{d\rho}$ we get $\frac{dl_S}{d\rho} = \frac{-2+P_{cc}(l_S c_S)V - P_{lc}(l_S,c_S)V}{(-2+P_{ll}(l_S,c_S)V)(-2+P_{cc}(l_S,c_S)V)-(P_{lc}(l_S,c_S)V)^2} < 0$, where the sign follows from the denominator being positive from $P_{cc} \leq 0, P_{lc} \geq 0$ and from the second order conditions.■

Proof of Proposition 3: Differentiating the first order conditions for the legislative effort of short- and long-term legislators with respect to π, considering l_S and l_L as implicit functions of π (and using the assumption $P_{l\pi} = 0$), we obtain, $\frac{dl_L}{d\pi} = -\frac{P_l(l_L)\frac{dV}{d\pi}}{-\frac{2}{\delta^2}+P_{ll}(l_L)V} > 0$, and $\frac{dl_S}{d\pi} = -\frac{P_l(l_S)\frac{dV}{d\pi}+P_\pi(l_S)h}{-\frac{2}{\delta}+P_{ll}(l_S)(V+l_S h)+2P_l(l_S)h} > 0$. The signs follow from the assumptions on $P(.)$, the second order conditions, and the (easy to demonstrate) fact that $\frac{dV}{d\pi} > 0$. A comparison of numerators shows that the increase in l_S is larger due to a direct effect that is larger by $P_\pi h$ (this term captures the marginal effect of electoral safety higher expectation that electorally safer legislators have of capturing the future returns to effort), unless the denominator drives the result. For this not to happen it suffices that $\frac{2}{\delta} - P_{ll}(l_S)(V + l_S h) - 2P_l(l_S)h < \frac{2}{\delta^2} - P_{ll}(l_L)V$, which is true if P is approximately linear.■

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Table 1. Duration of terms in selected legislatures

Term duration (years)	Countries, and states in the United States of America
2	United States House of Representatives US states: Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Maine, Massachusetts, Michigan, Minnesota, Missouri, Montana, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and Wyoming
3	Australia, Bhutan, El Salvador, Mexico, Nauru, New Zealand, and Philippines
4	Albania, Andorra, Angola, Armenia, Argentina, Austria, Belgium, Bosnia and Herzegovina, Brazil, Bulgaria, Chad, Chile, Colombia, Costa Rica, Croatia, Denmark, Dominican Republic, Germany, Ghana, Greece, Guatemala, Haiti, Honduras, Hungary, Iran, Iraq, Japan, Jordan, Kazakhstan, Kiribati, Lebanon, Liechtenstein, Lithuania, Macedonia, Madagascar, Mauritius, Moldova, Mongolia, Montenegro, Netherlands, Nigeria, Poland, Portugal, Romania, Russia, Slovakia, Solomon Islands, South Korea, Spain, Syria, Tuvalu, and Vanuatu US states: Alabama, Louisiana, Maryland, Mississippi, Nebraska, and North Dakota
5	Afghanistan, Antigua and Barbuda, Azerbaijan, Bahamas, Bangladesh, Barbados, Benin, Bolivia, Botswana, Burkina Faso, Burundi, Cambodia, Cameroon, Canada, Cape Verde, Central African Republic, China, Comoros, Cuba, Cyprus, Czech Republic, Democratic Republic of the Congo, Djibouti, Dominica, Egypt, Ethiopia, Fiji, France, Gabon, Gambia, Grenada, Guinea, Guyana, India, Ivory Coast, Jamaica, Kyrgyzstan, Laos, Lesotho, Luxembourg, Malawi, Malaysia, Mali, Malta, Mauritania, Monaco, Morocco, Mozambique, Namibia, Nicaragua, Niger, North Korea, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Republic of the Congo, Saint Lucia, Samoa, San Marino, Senegal, Seychelles, Sierra Leone, Singapore, South Africa, Suriname, Tajikistan, Tanzania, Togo, Tunisia, Turkey, United Kingdom, Uruguay, Uzbekistan, Vietnam, Zambia, and Zimbabwe
6	Liberia, Sri Lanka, Sudan, and Yemen

Note: when the legislature consists of a lower and an upper house, we consider the lower house.
Source: Online portals of each country or state legislature.

Table 2. Distribution of legislators by province and political party for the random allocation of terms

		Group 1 (later assigned a four year term)										Group 2 (later assigned a two year term)								
Province	Total	UCR	PJ	PI	UCD	DC	AUT	MPJ	MPN	PB	Total	UCR	PJ	PI	UCD	LIB	MFP	MPN	PB	Total
Capital	25	7	3	-	1	1	-	-	-	-	12	7	4	1	1	-	-	-	-	13
Buenos Aires	70	18	16	1	-	-	-	-	-	-	35	19	15	1	-	-	-	-	-	35
Catamarca	5	1	1	-	-	-	-	-	-	-	2	1	2	-	-	-	-	-	-	3
Córdoba	18	6	3	-	-	-	-	-	-	-	9	5	4	-	-	-	-	-	-	9
Corrientes	7	2	1	-	-	-	1	-	-	-	4	1	1	-	-	1	-	-	-	3
Chaco	7	1	2	-	-	-	-	-	-	-	3	2	2	-	-	-	-	-	-	4
Chubut	5	2	1	-	-	-	-	-	-	-	3	1	1	-	-	-	-	-	-	2
Entre Ríos	9	2	2	-	-	-	-	-	-	-	4	3	2	-	-	-	-	-	-	5
Formosa	5	1	2	-	-	-	-	-	-	-	3	1	1	-	-	-	-	-	-	2
Jujuy	6	1	1	-	-	-	-	1	-	-	3	1	2	-	-	-	-	-	-	3
La Pampa	5	1	1	-	-	-	-	-	-	-	2	1	1	-	-	-	1	-	-	3
La Rioja	5	1	2	-	-	-	-	-	-	-	3	1	1	-	-	-	-	-	-	2
Mendoza	10	3	2	-	-	-	-	-	-	-	5	3	2	-	-	-	-	-	-	5
Misiones	7	2	2	-	-	-	-	-	-	-	4	2	1	-	-	-	-	-	-	3
Neuquén	5	1	-	-	-	-	-	-	1	-	2	1	1	-	-	-	-	1	-	3
Río Negro	5	2	1	-	-	-	-	-	-	-	3	1	1	-	-	-	-	-	-	2
Salta	7	2	2	-	-	-	-	-	-	-	4	1	2	-	-	-	-	-	-	3
San Juan	6	1	1	-	-	-	-	-	-	1	3	1	1	-	-	-	-	-	1	3
San Luis	5	1	1	-	-	-	-	-	-	-	2	2	1	-	-	-	-	-	-	3
Santa Cruz	5	1	1	-	-	-	-	-	-	-	2	1	2	-	-	-	-	-	-	3
Santa Fe	19	5	5	-	-	-	-	-	-	-	10	5	4	-	-	-	-	-	-	9
S. del Estero	7	2	2	-	-	-	-	-	-	-	4	1	2	-	-	-	-	-	-	3
Tucumán	9	2	3	-	-	-	-	-	-	-	5	2	2	-	-	-	-	-	-	4
T. del Fuego	2	-	-	-	-	-	-	-	-	-	-	1	1	-	-	-	-	-	-	2
TOTAL	254	65	55	1	1	1	1	1	1	1	127	64	56	2	1	1	1	1	1	127

Notes: UCR is Unión Cívica Radical; PJ is Partido Justicialista; PI is Partido Intransigente; UCD is Unión del Centro Democrático; DC is Democracia Cristiana; AUT is Partido Autonomista; MPJ is Movimiento Popular Jujeño; MFP is Movimiento Federalista Pampeano; MPN is Movimiento Popular Neuquino; PB is Partido Bloquista de San Juan; LIB is Partido Liberal.

Table 3. Summary statistics - House

	Long track	Short track	Difference of means
Floor attendance (in %)	82.346	79.833	2.513**
	(0.733)	(0.726)	(1.032)
Committee attendance (in %)	56.507	50.872	5.635**
	(1.543)	(1.552)	(2.188)
Number of committee bills	47.336	41.397	5.939*
	(2.366)	(2.224)	(3.247)
Number of floor speeches	5.616	4.339	1.277
	(0.561)	(0.569)	(0.800)
Number of bills introduced	6.224	5.496	0.728
	(0.655)	(0.587)	(0.879)
Number of bills ratified	0.276	0.128	0.148***
	(0.042)	(0.025)	(0.049)
Index of legislative effort	0.212	0	0.212***
	(0.039)	(0.033)	(0.050)
Age	50.168	50.868	-0.700
	(0.959)	(0.926)	(1.333)
Male	0.944	0.967	-0.023
	(0.021)	(0.016)	(0.026)
Freshman	0.944	0.934	0.010
	(0.021)	(0.023)	(0.031)
Lawyer	0.368	0.273	0.095
	(0.043)	(0.041)	(0.059)
University degree	0.184	0.157	0.027
	(0.035)	(0.033)	(0.048)
Leader	0.136	0.083	0.053
	(0.031)	(0.025)	(0.040)
Slackness	0.600	0.521	0.079
	(0.044)	(0.046)	(0.063)
Majority party	0.504	0.488	0.016
	(0.045)	(0.046)	(0.064)
Small block	0.056	0.058	-0.002
	(0.021)	(0.021)	(0.030)
Distance	6.598	6.817	-0.220
	(0.515)	(0.572)	(0.769)

Note: Standard errors are in parentheses. The long track corresponds to a four year term. The short track corresponds to a two year term. *Leader* is a dummy variable equal to 1 when the legislator is the president of the chamber, a majority or minority leader, or a committee chair. *Freshman* is a dummy equal to 1 for representatives without any previous legislative experience at the national level. *University degree* is a dummy equal to 1 for representatives with a non-law degree. *Slackness* is a dummy equal to 1 if, given the party-province list in the 1983 elections, the legislator placed in the top half of the elected delegation. *Small block* is a dummy equal to 1 when the legislator belongs to a party holding three or fewer seats. *Distance* is the distance (in hundreds of kilometers) from the capital of the legislator's province to Buenos Aires (the seat of the national legislature). The number of observations is 492. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level, based on a t-test of equality of means.

Table 4. Correlations among measures of legislative effort

	Floor Attendance	Committee attendance	Committee bills	Floor speeches	Bills introduced	Bills ratified
Floor attendance	1					
Committee attendance	0.39	1				
Committee bills	0.27	0.49	1			
Floor speeches	0.05	-0.09	-0.09	1		
Bills introduced	-0.02	0.06	0.17	0.04	1	
Bills ratified	0.16	0.03	0.07	0.15	0.13	1

Note: correlations computed on raw data observed on yearly basis. Collapsing the data by individual yields similar results.

Table 5. The effects of term length on legislative effort

	Index of legislative effort		
	(1)	(2)	(3)
Four year term	0.212***	0.195***	0.193***
	(0.062)	(0.063)	(0.064)
Age		0.001	0.001
		(0.003)	(0.003)
Male		0.104	0.018
		(0.165)	(0.135)
Freshman		0.097	0.137
		(0.185)	(0.177)
Lawyer		0.035	0.058
		(0.076)	(0.075)
University degree		0.119*	0.119
		(0.071)	(0.076)
Leader		0.215*	0.225*
		(0.113)	(0.115)
Slackness		-0.024	-0.027
		(0.059)	(0.061)
Majority party		0.145**	0.139**
		(0.066)	(0.066)
Small block		0.176	0.186
		(0.161)	(0.154)
Distance		-0.007	
		(0.005)	
Province dummies	No	No	Yes

Notes: Standard errors clustered at the legislator level are in parentheses. All models include a time dummy and are estimated by OLS. The number of observations is 492. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 6. The effects of term length on legislative effort by outcome

	Floor attendance		Committee attendance		Committee bills	
	(1)	(2)	(3)	(4)	(5)	(6)
Four year term	2.513**	2.548***	5.635**	6.259**	0.133	0.175
	(1.076)	(0.956)	(2.764)	(2.498)	(0.100)	(0.108)
Change	3%	3%	11%	12%	14%	19%
Controls	No	Yes	No	Yes	No	Yes
Method	OLS	OLS	OLS	OLS	Neg. Bin.	Neg. Bin.
	Floor speeches		Bills introduced		Bills ratified	
	(7)	(8)	(9)	(10)	(11)	(12)
Four year term	0.264	0.122	0.133	0.184	0.753***	0.669***
	(0.198)	(0.172)	(0.184)	(0.135)	(0.256)	(0.249)
Change	30%	13%	14%	20%	112%	95%
Controls	No	Yes	No	Yes	No	Yes
Method	Neg. Bin.	Neg. Bin.	Neg. Bin.	Neg. Bin.	Neg. Bin.	Neg. Bin.

Notes: Standard errors clustered at the legislator level are in parentheses. For OLS models, Change is calculated as 100*Estimate/mean of the respective output for legislators in a two year track. For Neg. Bin. (Negative Binomial) models, Change is calculated as 100*[exp(Estimate)-1]. All models include a time dummy. Controls include *Age*, *Male*, *Freshman*, *Lawyer*, *University degree*, *Leader*, *Slackness*, *Majority party*, *Small block*, and the set of province dummies. The number of observations is 492. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 7. Robustness check: data collapsed at the legislator level and standard errors clustered at the province level

	Index	Floor attendance	Committee attendance	Committee bills	Floor speeches	Bills introduced	Bills ratified	Principal component
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Four year term	0.234***	2.548**	6.259**	0.175**	0.104	0.172	0.698**	0.478***
	(0.075)	(0.908)	(2.283)	(0.089)	(0.198)	(0.168)	(0.280)	(0.141)
Change		3%	12%	19%	11%	19%	101%	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	Neg. Bin.	Neg. Bin.	Neg. Bin.	Neg. Bin.	OLS

Notes: Standard errors clustered at the province level are in parentheses. The principal component accounts for 33 percent of the total variance. For OLS models, Change is calculated as 100*Estimate/mean of the respective output for legislators in a two year track. For Neg. Bin. (Negative Binomial) models, Change is calculated as 100*[exp(Estimate)-1]. Change is not calculated for the Index since this variable is normalized to zero for legislators in a two year track. Controls include *Age*, *Male*, *Freshman*, *Lawyer*, *University degree*, *Leader*, *Slackness*, *Majority party*, *Small block*, and the set of province dummies. The number of observations is 246. **Significant at the 5% level; ***Significant at the 1% level.

Table 8. Additional robustness checks

	Index of legislative effort			
	(1)	(2)	(3)	(4)
Four year term	0.193***	0.193***	0.161***	0.267***
	(0.055)	(0.059)	(0.059)	(0.067)
Controls	Yes	Yes	Yes	Yes
Observations	492	492	316	168
	(5)	(6)	(7)	(8)
Four year term	0.168**	0.283**	0.214***	0.187***
	(0.066)	(0.123)	(0.067)	(0.064)
Controls	Yes	Yes	Yes	Yes
Observations	244	220	438	472

Notes: Standard errors clustered at party/province combinations are shown in parentheses in column (1). Standard errors clustered at the province level are shown in parentheses in column (2). Standard errors clustered at the legislator level are in parentheses in columns (3)-(8). Regression (3) includes only those party-province delegations that used the even/odd rule in order to assign legislators into the two groups, whereas regression (4) also excludes the first two legislators in the party list. Regression (5) includes only majority party (Unión Cívica Radical) legislators, whereas regression (6) includes only minority party (Partido Justicialista) legislators. Regression (7) excludes legislators that are leaders, and regression (8) excludes those legislators that change leader status during the sample period. All models include a time dummy and are estimated by OLS. Controls include *Age*, *Male*, *Freshman*, *Lawyer*, *University degree*, *Leader*, *Slackness*, *Majority party*, *Small block*, and the set of province dummies. **Significant at the 5% level; ***Significant at the 1% level.

Table 9. Summary statistics - Senate

	Long track	Short track	Difference of means
Floor attendance (in %)	83.345	80.975	2.370
	(1.267)	(2.090)	(2.444)
Number of bills introduced	33.591	23.476	10.115**
	(3.134)	(2.869)	(4.249)
Number of bills ratified	2.318	1.667	0.652
	(0.361)	(0.294)	(0.466)
Age	50.750	52.238	-1.488
	(1.276)	(1.801)	(2.207)
Male	0.591	0.714	-0.123
	(0.075)	(0.101)	(0.126)
Freshman	0.545	0.571	-0.026
	(0.076)	(0.111)	(0.134)
Lawyer	0.455	0.333	0.121
	(0.076)	(0.105)	(0.130)
University degree	0.273	0.476	-0.203
	(0.068)	(0.112)	(0.131)
Leader	0.705	0.619	0.085
	(0.070)	(0.109)	(0.129)
Majority party	0.341	0.286	0.055
	(0.072)	(0.101)	(0.124)
Small block	0.091	0.190	-0.010
	(0.044)	(0.088)	(0.098)
Distance	1284.432	983.714	300.718**
	(108.176)	(71.909)	(129.896)

Note: Standard errors are in parentheses. The long track corresponds to four and six year terms. The short track corresponds to a two year term. *Leader* is a dummy variable equal to 1 when the legislator is the president of the chamber, a majority or minority leader, or a committee chair. *Freshman* is a dummy equal to 1 for senators without any previous legislative experience at the national level. *University degree* is a dummy equal to 1 for senators with a non-law degree. *Small block* is a dummy equal to 1 when the legislator belongs to a party holding three or fewer seats. *Distance* is the distance (in hundreds of kilometers) from the capital of the legislator's province to Buenos Aires (the seat of the national legislature). The number of observations is 130. **Significant at the 5% level, based on a t-test on equality of means.

Table 10. Evidence from the Senate

	Index of legislative effort			Principal component	Floor attendance	Bills introduced	Bills ratified
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Long term	0.459*	0.340	0.458*	0.577*	1.299	0.402**	0.241
	(0.232)	(0.272)	(0.251)	(0.323)	(2.822)	(0.166)	(0.221)
Change					2%	49%	27%
Method	OLS	OLS	OLS	OLS	OLS	Neg. Bin.	Neg. Bin.
Controls	No	Yes	No	No	Yes	Yes	Yes
Observations	130	130	65	65	130	130	130

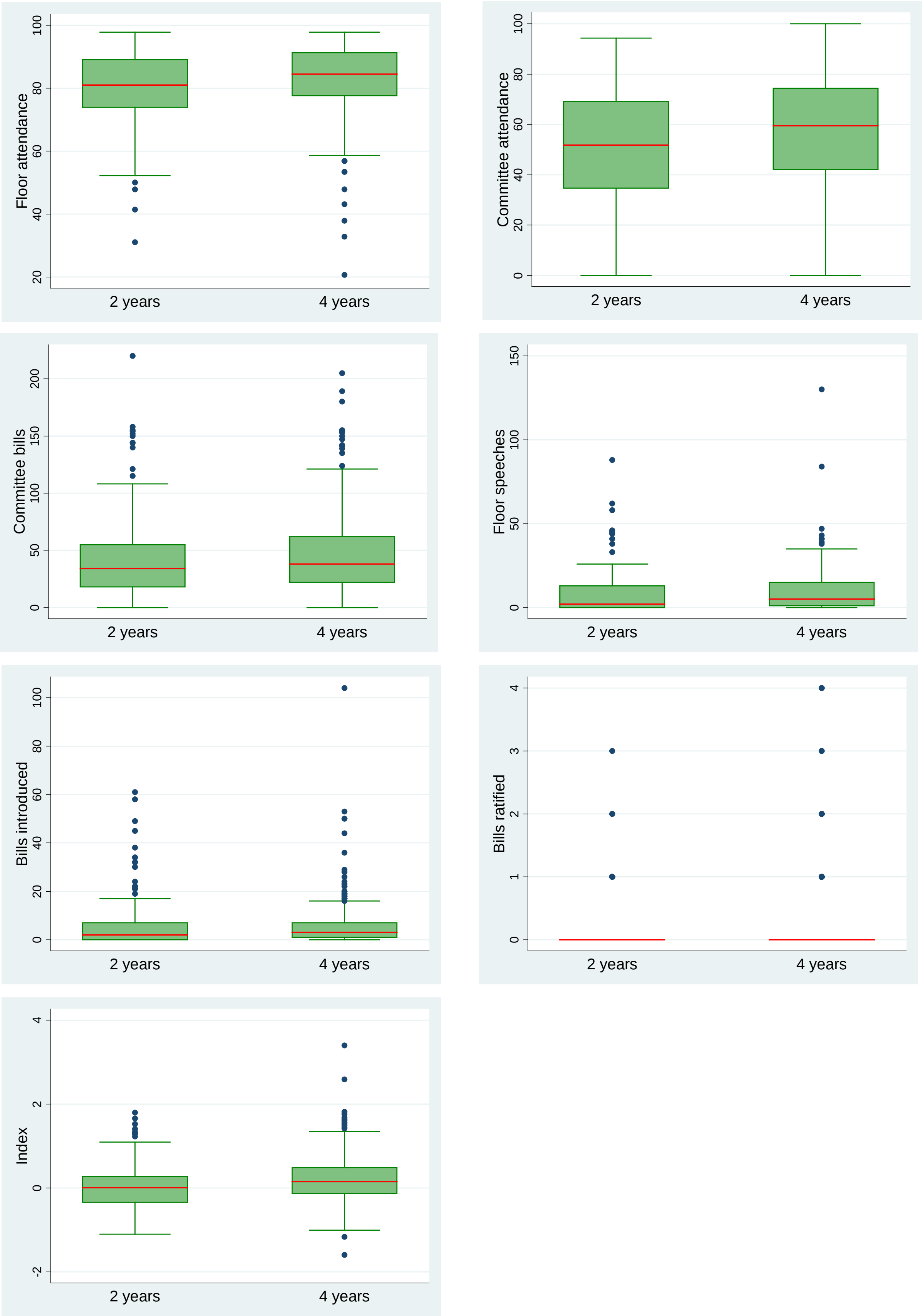
Note: Standard errors clustered at the legislator level are in parentheses (for collapsed data these are equivalent to robust standard errors). The principal component accounts for 56 percent of the total variance. For OLS models, Change is calculated as 100*Estimate/mean of the respective output for legislators in a short track. For Neg. Bin. (Negative Binomial) models, Change is calculated as 100*[exp(Estimate)-1]. Change is not calculated for the Index since this variable is normalized to zero for legislators in a short track. All specifications include a time dummy. In models (3) and (4) the data are collapsed at the legislator level. Controls include *Age*, *Male*, *Freshman*, *Lawyer*, *University degree*, *Leader*, *Majority party*, *Small block,* and *Distance*. *Significant at the 10% level; ***Significant at the 10% level.

Table 11. Investigating mechanisms

	Index of legislative effort								
	Senate	Senate	House	House	House	House	House	Senate	Senate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Long term	0.580*	0.585	0.228***	0.238**	0.298**	0.310***	0.379***	0.459	0.448
	(0.296)	(0.349)	(0.077)	(0.104)	(0.124)	(0.092)	(0.105)	(0.404)	(0.463)
Long term x			-0.069						
Second year			(0.072)						
Long term x				-0.007	-0.009				
Distance				(0.010)	(0.014)				
Long term x						-0.212	-0.264**		
Slackness						(0.129)	(0.113)		
Long term x								-0.209	-0.099
Freshman								(0.538)	(0.632)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province dummies	No	No	Yes	No	No	Yes	Yes	No	No
Observations	88	44	492	492	246	492	246	130	65

Notes: Standard errors clustered at the legislator level are in parentheses in columns (1)-(4), (6), and (8)-(9) (for collapsed data in the Senate these are equivalent to robust standard errors). Standard errors clustered at the province level are in parentheses in columns (5) and (7). In regressions (1) and (2), which exclude two year senators, *Long term* is a dummy variable equal to 1 for senators in a six year track and zero for senators in a four year track. In regressions (3) to (7) *Long term* is a dummy equal to 1 for representatives in a four year track and zero otherwise. In regressions (8) and (9) *Long term* is a dummy equal to 1 for senators in either a four year or a six year track and zero for senators in the two year track. *Second year* is a dummy variable that takes the value of one for the second year of the data. Models (1), (3), (4), (6), and (8) include a time dummy. In models (2), (5), (7), and (9) the data are collapsed at the legislator level. All models are estimated by OLS. Controls include *Age*, *Male*, *Freshman*, *Lawyer*, *University degree*, *Leader*, *Slackness*, *Majority party*, *Small block*, and *Distance*. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Figure 1. Box-and-whiskers plots by type of outcome (4-year vs. 2-year tracks)



Notes: The median is shown as a line across the box. The box plot stretches from the lower hinge (defined as the 25th percentile) to the upper hinge (the 75th percentile). The whiskers plot the lower and higher adjacent values respectively (equal to the respective hinge plus 1.5 times the interquartile range). The values outside the whiskers ("outside values") are plotted as dots.