

Do Higher Salaries Lead to Higher Performance? Evidence from State Politicians*

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Abstract

We study the impact of politician salary on electoral competitiveness and political performance using new data on US state legislators and governors over the last sixty years. Higher salary is associated with statistically significant, but economically small, increases in electoral competitiveness and legislative productivity, the latter measured with bill-passing and missed roll call votes. Salary has no effect on politician quality, corruption, or fiscal policy. To address the possible concern of salary changes being correlated with politicians' outside options, we implement a spatial discontinuity design using legislative district pairs straddling state borders, and obtain qualitatively similar findings to our other research designs (fixed-effects, selection-on-observables, and a narrative analysis). Despite limited impacts on performance, higher salary is significantly correlated with behavior on another margin, namely time-use; time-use data suggests that politicians in higher wage states spend greater time on fund-raising and on constituent services, but no more time on legislative activities. Our results lend caution to common claims that increasing politician salary would significantly increase the quality of US state government.

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1 Introduction

Designing institutions that promote talented people to run for office and to govern well is a central issue in political economy (Barro, 1973). A large literature focuses on understanding how incentives regarding re-election (e.g., term limits) affect politician effort (e.g., Besley and Case, 1995; List and Sturm, 2006). Another potentially important, but much less studied institutional feature is politician salary. Although politicians, like most workers, generally do not receive explicit pay-for-performance contracts, higher salaries may reduce moral hazard, both by incentivizing effort due to fear of losing one’s job (Becker and Stigler, 1974; Shapiro and Stiglitz, 1984) and by drawing in higher ability job-seekers (Weiss, 1980). In this paper, we investigate how politician salary affects the productivity and quality of politicians, as well as the competitiveness of political markets.

In addition to importance for theory, how salary affects political performance is important for policymakers. In the US, this is particularly true in state politics, where there is wide variation in salaries, and there is significant concern about low performance by politicians. In discussion about the quality of US state government, a common concern is that politician salaries are too low. For example, the Council of State Governments, a national organization representing state governments, often argues that low legislative compensation has deleterious consequences, stating “If legislators are not paid adequately, then candidates are drawn from a smaller pool. ...You can’t expect to attract good candidates with pay that is lower when compared to other jobs and professions.”¹

We study the impact of politician salary on election competitiveness and politician performance by looking at two different types of politicians—governors and legislators—both of which perform critical roles but may respond differently to incentives. We study how salaries affect the number and types of individuals who run for election, their quality, and their effort and performance if elected. We do so exploiting 60 years of new US data. We take advantage of the large variation in politician salary across states and over time, and employing fixed effect, selection on observables, and spatial discontinuity research designs we find that salary has statistically significant, but economically very

¹Quotation from Keon Chi, editor-in-chief of the Council of State Government’s *Book of the States*. Quoted in “Legislators’ pay falling behind” by Eric Kelderman, February 13, 2007, article on Stateline.org. A more recent article from July 23, 2013 in USA Today, “Most governors paid less than state execs,” argues that state governors may be significantly under-paid relative to the private sector and other state officials, and thus that governorships may not be drawing in the best people. General popular discussion on whether politicians should be paid more is also very common. For example, business magnate Richard Branson, founder of Virgin Airlines, in a July 13, 2013 blog post entitled “Should politicians be paid more?” argues that increasing pay is critical for attracting high-quality politicians in the context of recent discussion about whether to raise pay for British members of parliament.

small positive impacts on election and performance outcomes.

We draw on the citizen candidate model of Besley and Coate (1997) and Osborne and Slivinski (1996) in a conceptual framework of how politician salary may affect performance. In this framework, citizens choose whether to run for office by weighing the gains from office against their outside option. Higher ability workers have higher outside options, so increasing salary makes higher ability workers want to work in politics instead of as lawyers and businesspeople. Thus, salary will increase the number of candidates running for office. In addition, since a politician’s chance of getting re-elected depends on them exerting effort to provide public goods, higher salaries mean that politicians become more concerned with getting re-elected, and thus exert more effort. Theory by itself, however, cannot determine the likely magnitude of these effects, nor assess how these magnitudes compare with alternative countervailing forces.

We study the predictions of the conceptual framework using the large variation across states and over time in salary for state legislator and governors. Our analysis focuses more on legislators, for whom we have richer data, but we have similar findings for both legislators and governors. Specifically, we analyze the impact of politician salary on election outcomes, including the number of candidates and re-election rates; politician selection, measured by quality proxies such as politician schooling and diversity; and political effort and performance, measured by bill-passing, fiscal policy, and roll call voting. For legislators, we find that increasing salary by 50% is associated with a one percentage point increase in the probability an election is contested and increases the number of candidates in the election by 0.025. It increases by share of bills approved by the legislature by 0.7 percentage points and decreases the probability of a missed roll call vote by 1 percentage point. These effects are statistically different from zero, precisely estimated, and are very small compared with variation over time or across states. We find no connection between legislator salary and having more diverse or educated legislators. For governors, we find that salary appears to have almost no impact on running for office, on selection, or on performance.

We lay out three main concerns related to whether salary could be correlated with unobservables and address them in turn, including by using a spatial discontinuity analysis. Absent random variation or a good instrument for politician salary in the US, that salary could be correlated with unobservables is the central empirical concern for our study. First, states may be likely to increase salaries when times are (unobservedly) economically “good” so as to match politicians’

outside options, which could potentially bias, for example, the relationship between salary and electoral competitiveness. Although we control for state GDP, what is relevant for potential political candidates may not be the overall labor market, but rather, say, the labor market for lawyers. While California and Nevada may have very different labor markets for lawyers in a given year, it may be that the market in a district in eastern California and the market in a bordering district in western Nevada are more similar. To address the possibility that increases in salary could correlate with unobserved local labor market conditions such as the market for lawyers, we use a spatial discontinuity design, comparing legislative districts on either side of state borders. Through our spatial discontinuity design, we can analyze the impact of salary on electoral competitiveness, comparing only between bordering districts in a given year, and we find very similar results as in the baseline.

Second, as we describe in Section 2, pay is set by the upper legislative house for governors and by one of several means (compensation committee, statute, or the legislators themselves) for legislators. One may worry that pay could be set to co-vary with unobservable information about performance or with unobserved dimensions of politician quality. Salaries may be increased when politicians expect to do well, though this would tend to upward bias the results away from our finding of small impacts of salary on performance. Alternatively, unobservedly higher quality politicians may be averse to raising salaries. However, based on observable measures of quality, we find no evidence that higher quality politicians are less likely to have their salaries raised. We also find that results are similar when we restrict the sample to states where politicians have little direct control over salary.

Third, it is possible politician salaries could be correlated with other institutions in a state. Although we collected a large amount of data on changes in state legislative institutions over time, it is possible that changes in salary may correlate with unobserved changes in institutions. To address this, we use a selection-on-observables strategy, where we gradually control for observable aspects of legislature professionalism. Our results are highly robust to this strategy, suggesting bias is limited.

In addition, in order to assuage any lingering concerns related to these issues, we implement a narrative analysis. We use the historical record to identify a set of 17 salary changes or non-changes which we believe are plausibly exogenous for our analysis (e.g., whether the salary change went through or not was determined by a close vote of a salary commission or the state legislature). Although the sample is much smaller, we find similar results as in our other analyses.

We find some suggestive support for two interesting mechanisms which could explain the mod-

est impact of politician salary: (1) That politicians are responding on other effort margins and (2) That strong political parties act as a barrier to electoral competition. We also fail to find support for other mechanisms. For one, politician salaries in many states are low relative to other corporate and professional jobs, and many politicians are independently wealthy. It could simply be that salaries are too small to affect politician incomes or behavior. We show, however, that legislator salaries comprise a significant portion of household earnings and have a significant impact on whether legislators work outside of politics. We also fail to find a correlation between legislator salary and legislators' desire to start a career in lobbying following their time in office. In support of the hypothesis that politicians respond on other effort margins, unique data on legislator time use show that when politicians receive higher salaries, they spend more time on fund-raising and on constituent services, as opposed to on developing legislation. This evidence seems inconsistent with an explanation based on politicians have strong intrinsic motivations, where they exert effort because of a "calling" for politics (Weber, 2004) and not because of a desire to get re-elected.

We provide one of the first analyses (to our knowledge) of the impact of politician salary in the US.² We suspect this absence may be due to data limitations. There is no central database on politician salary, and a significant portion of our data is hand-collected. In addition, we assemble data on electoral competitiveness, partisan electoral advantage, politician quality (as measured through politician schooling and other characteristics), politician productivity (as measured by bill-passing), shirking (as measured by missed roll call votes), outside labor supply, and politician time use. In economics, the only work on US politician salary we are aware of is by Diermeier et al. (2005) and Keane and Merlo (2010). These two papers estimate structural models of career decisions for US congressmen. Their counterfactual simulations include an investigation of how changes in congressional wages affect congressional career decision-making. By analyzing the impact of politician salary using wage variation across states, our analysis complements their counterfactual results. Given that there is very little work on US politician salary, some of the most related work to our paper is (arguably) that on other institutions for US politicians, such as term limits (Besley and Case, 1995; List and Sturm, 2006), term length (e.g., Kalt and Zupan, 1990; Titiunik, 2014), and

²For the US, we know of very little prior empirical work on the impact of politician salary. In political science, we have found some papers that have included salary in some analyses related to state legislatures (Berkman, 1994; Fiorina, 1994; Squire, 1997), but the focus of these papers is not on the impact of politician salary. In economics, Besley (2004) shows that higher salary for governors is associated with greater ideological congruence between governors and their electorate. Although not directly related to our paper, two papers in political science, Groseclose and Krehbiel (1994) and Hall and Van Houweling (1995), show that US congressmen respond to financial incentives to retire.

district size (e.g., Baqir, 2002; Gilligan and Matsusaka, 1995).

Our study suggests that the impact of political salary may be context dependent. In economic theory, there is substantial work analyzing the impact of politician salary (e.g., Besley, 2004; Caselli and Morelli, 2004; Mattozzi and Merlo, 2008; Messner and Polborn, 2004; Poutvaara and Takalo, 2007); as we discuss in Section 3, this work points out that the impact of increasing salary is not always straightforward. Compared to several recent empirical papers finding predominantly large impacts of politician salary in other countries, the impacts we estimate are often an order of magnitude smaller. Ferraz and Finan (2010) and Gagliarducci and Nannicini (2010) study the impact of politician salary for Brazilian municipal legislators and Italian mayors, respectively. For Brazilian municipal legislators and Italian mayors, salaries change discontinuously with city population, allowing for regression discontinuity estimates of the impact of politician salary. Fisman et al. (2013) and Mocan and Altindag (2013) study the impact of salaries for Members of the European Parliament (MEPs), exploiting a recent pay equalization policy that significantly increased salaries for politicians from certain countries. Kotakorpi and Poutvaara (2011) analyze a recent pay increase for Finnish parliamentarians. These papers mostly tend to find large impacts on competitiveness, selection, and performance.³ While it is of course possible that politicians in the US may simply be “different” than politicians in other countries, or that differences in methods between the papers are important, we believe that it may be useful to consider differences in political institutions. For instance, in the Brazilian municipalities studied by Ferraz and Finan (2010), politics is highly decentralized and political parties are weak. In contrast, the two main parties in the US are relatively strong.⁴

Section 2 provides background on compensation and other institutions for US state legislators and governors. Section 3 provides a conceptual framework and describes the data. Section 4 shows results. Section 5 interprets our findings and considers possible explanations. Section 6 concludes.

³For electoral competitiveness, Ferraz and Finan (2010) and Fisman et al. (2013) find large positive impacts on whether politicians seek re-election. For selection, Ferraz and Finan (2010), Gagliarducci and Nannicini (2010), and Kotakorpi and Poutvaara (2011) find large positive impacts on politician quality, whereas Fisman et al. (2013) find negative impacts on politician quality. (For Kotakorpi and Poutvaara (2011), positive impacts are only observed for female candidates.) For political performance, Ferraz and Finan (2010) and Gagliarducci and Nannicini (2010) find large positive impacts on legislative productivity, whereas Fisman et al. (2013) find zero effect. We find modest impacts across electoral competitiveness, selection, and performance.

⁴Levels of power may also be important for interpreting results (Fisman et al., 2013). US state politicians occupy a different level of power than either EU parliamentarians or municipal politicians in Brazil and Italy.

2 Institutional Context

In this section, we give a brief overview of the key factors that characterize the pay structure and responsibilities of US governors and legislators in order to give context to our analysis. There is substantial variation in salaries across states and over time. Understanding the sources and implications of these differences is critical for careful investigation of our research question. While much more can be said about the institutional context beyond what is included in this section, our goal is to limit the focus to the points that are most relevant for our setting.

We study the impact of salary both for state legislators and for governors because both roles are important for the successful functioning of state governments. The two roles also may be affected by salary differently and they may have different implications for the effect of salary on outcomes more generally. In particular, the number of legislators per state has a range of approximately 50 to 400, whereas each state has only one governor. Legislators are responsible for state legislation and, importantly, for distributing state funds (e.g., Rehavi, 2007). Governors are chief administrators of the state and also play a key role in advancing legislation (e.g., Bernick, 1979).

While nominal salaries for legislators averaged across all states have been increasing at a steady clip, as seen in Figure D2, there is substantial variation both within and across states. As seen in Figure 1, in some states, salaries have increased a great deal over time, whereas in others, salary has remained somewhat flat. In states where salary has increased substantially over the sample frame, salary has been increased in large irregular discontinuous jumps, though in other states the increases have been more gradual.

How pay is determined for governors and legislators varies between the positions and across states, and has important implications for our research design. For governors, pay is determined by a state's upper assembly, and the amount is frequently adjusted.⁵ For legislators, how pay is determined varies across states. As of 1993, in 31% of the contiguous 48 states, pay for legislatures is determined by a state compensation commission. Commissions set salaries based on a number of criteria, including the pay of other government officials and bureaucrats. In a relatively small number of the contiguous states, 14.5%, pay is set by the constitution. New Mexico is an extreme example of this where the state constitution specifies that legislators will receive no pay (except for some expenses). In states where pay is constitutionally determined, it is changed only if the constitution

⁵For a rich analysis of how compensation is set for governors, see Di Tella and Fisman (2004).

is amended. Finally, in the remainder of the states, legislator pay is set by statute, either a law outside the constitution, or the legislators essentially set their own pay by passing compensation bills. In many cases, legislators vote on their own pay, but the increases only take effect for the next term.⁶ We later exploit this variation in how pay is set for legislators to assuage concerns that endogenous wage setting is driving our results.

What determines salary changes over time for legislators and governors? First, for legislators, salaries have increased as legislatures have become more “professionalized.” In the past, many legislatures would meet every other year or be in session for only several months. In the mid 20th century, however, a push began where legislatures began a shift toward more responsibilities. As of 2010, only five legislatures meet every other year: Montana, Nevada, North Dakota, Oregon, and Texas. The National Conference of State Legislatures classifies states into categories based on whether their legislatures are full-time or part-time. Ten states are categorized as full-time or almost full-time, and the rest are considered as part-time or almost part-time (National Conference of State Legislatures, 2009). In general, full-time legislators tend to receive higher pay. In all our analysis of state legislators, we control for length of legislative sessions in days, probably the most important measure of session length, and we also explore other measures of professionalization such as a legislator’s number of staff.⁷

Second, as documented by Di Tella and Fisman (2004), salaries have often been increased in response to economic growth. As the economy improves and there are greater tax revenues at both the national and state level, salaries for politicians tend to increase. To account for this in our regressions, we control for state GDP.

Third, and most important for our analysis, even conditional on professionalization and economic growth, as well other differences across states, there are a substantial number of large, abrupt, and seemingly irregular salary changes in the historical record. As an example for legislator compensation, consider the nearby states of Michigan and Minnesota. In the 1980s and 1990s, legislator salaries were higher in Michigan than in Minnesota, but both were growing at a slow, steady rate. In 2000, Michigan legislators made \$55,054 and Minnesota legislators made \$31,440. However, due to a vote of the Michigan Compensation Commission, salaries were increased roughly 40% from

⁶Per the 27th amendment to the US constitution passed in 1992, this is the method use by the US congress in setting its compensation.

⁷Governorships have historically been full-time jobs with professional staff, so the issue of professionalization is somewhat less relevant than for legislators.

\$55,054 to \$77,400, where it remained at a roughly similar level throughout the 2000s. In contrast, the Minnesota salary stayed flat during this time period. Or in another example, legislator salary in Oklahoma increased by 60% in 1990, from \$20,000 to \$32,000, changes which were not observed in nearby states. There are also examples of seemingly irregular changes in governor salary.⁸

Looking deeper into the historical record, these seemingly irregular changes appear to stem from a number of different sources. The membership of compensation committees changes over time, and their votes on proposed changes are frequently divided. When salary changes are passed by the legislature itself, votes are sometimes close and sometimes divided between the upper and lower house. In a few instances that we have located, salary increases that have been passed by compensation commissions or legislatures have later been undone by courts or governor vetoes. We believe that such changes (and non-changes) are likely to be driven by forces external to those affect electoral competitiveness and productivity. In Section 4.7, we use these events to create a sample of salary change events which are likely to be exogenous for our regressions.

Other factors that may contribute to both politician salary and election outcomes are whether a seat is term-limited and the length of a term. Some states have term-limited seats for legislators and governors and some do not. In addition, in some states, legislator terms are four years and in some they are two years. The form of legislative compensation also differs across states. In some states, legislators are paid a yearly annual salary. For example, in California legislators made \$125,000 per year in 2008. This amount is paid irrespective of the number of days the legislature meets. In other states, legislators are paid a daily wage.⁹

Conditioning on important institutional features, there remains a great deal of variation in legislator and governors. In a regression of real salary on session length, frequency of meeting, term length, and term limits, the R^2 is only 0.213. Figures D3 and D5 plot real salaries over time for legislators and governors, respectively. For legislators, pay in some states has been persistently very low, whereas in other states there has been much more variation.

The differences across states in session lengths, meeting frequencies, modes of pay determination and pay frequency are useful for testing mechanisms for our results, but they also imply that

⁸For example, in Tennessee, governor salary was increased to \$160,000 in 2008, up from \$85,000 where it had been for over 15 years. Bordering states made no changes or relatively small changes to governor salaries. In a much older example, governor salary in Alabama shot up in the early 1950s, while surrounding states again kept governor salaries relatively constant.

⁹In Vermont, legislators receive a weekly wage.

we need to be especially careful in our analysis of the effect of salary on electoral outcomes in the US. To control for this variation, whenever possible, we use state and year fixed effects, as well as controls for important institutional features (particularly features such as session length, frequency of legislative sessions, term limits, and term length).¹⁰ Our estimation methodology is described in more detail in Sections 3 and 4.

3 Conceptual Framework and Data

This section provides theoretical motivation for our analysis and information on our data. We begin by laying out a conceptual framework on how a politician’s salary would affect their performance and electoral competitiveness. A formal model accompanying the discussion is given in Appendix C. Following this framework, we describe the sources and structure of our data, and delineate how we measure our key variables.

3.1 Conceptual Framework

Paying politicians a higher salary serves to incentivize them to exert more effort once they are in office. As in a standard efficiency wage model, the cost of shirking for politicians is the cost of getting fired (or in their case, not re-elected). Raising the salary increases the return to getting re-elected, even when a significant part of being in office may be benefits outside of the formal salary.¹¹

In addition, paying politicians a higher salary may affect the quality of people who are willing to run for office. Suppose that perspective politicians differ on a single dimension of quality, which could represent their ability to get legislation done or to act in the interest of their constituents. It is likely that higher ability politicians also have higher outside options (Gagliarducci et al., 2010). Thus, by raising the return for running for office, a higher salary could draw in higher quality politicians.

Although this logic may seem relatively straightforward, it is not theoretically obvious that raising salary should increase politician quality. Mattozzi and Merlo (2008) present a dynamic equilibrium model of politician careers. If higher quality politicians receive higher financial payoffs after serving in office, increasing the wage may *reduce* politician quality, since doing so reduces the

¹⁰We do not have observations over time for some of our dependent variables, including our measure of politician effort. For these cases, we do not include state and year fixed effects.

¹¹Such benefits include utility from public service (Weber, 2004), the opportunity to be a lobbyist after politics (Blanes i Vidal et al., 2012), or the opportunity to accumulate political rents (Querubin and Snyder, 2011).

comparative advantage of being a high quality politician. It is thus primarily an empirical question whether politician salary will increase political effort and politician quality.

By increasing politician salary, more citizen-candidates may be willing to run for office, so elections have more candidates and are more likely to be contested. As emphasized in Ferraz and Finan (2010), impacts on re-elections may be more theoretically ambiguous. On the one hand, by spurring more candidates and/or higher quality candidates to run, re-election rates for incumbents should decrease. On the other hand, increasing salary increases the value of a seat for incumbents, so they may exert more effort in providing public goods and/or other efforts to get re-elected.

In Appendix C, we also address the issue of political parties. To formalize how political parties interact with the impact of politician salary, we conceptualize a strong political party as a tax that politicians pay. When the benefits of public office increase, it is not the case that anyone can receive them. Rather, only those with tight connections to one of the major political parties are likely to have a reasonable chance of running for office and winning. This may limit the impact of politician salary on electoral competition and performance.¹²

3.2 Data Sources

Our paper combines data from several sources, and includes a substantial portion of hand-collected data. A complete list of data sources is given in Appendix B. We discuss the most important elements here, including how they relate to our identification strategies.

Salaries. A sizeable portion of the data was hand-collected from the *Book of the States*, a periodical providing extensive information about policies and programs in different states. The most important data here are the politician salary data.¹³ Governors all receive an annual salary. For legislators, we create a single annual salary figure for each state by annualizing the relevant time frame. Salaries that are given for biennial legislative sessions are divided by two to obtain an annual

¹²As described in Mayhew’s (1986) seminal political science book on parties in US politics, political parties have often play a central role in state politics, but the role has differed by state. In “strong party” states such as New York, famous for the Tammany Hall political machine, or Indiana, with strong local party organizations across the state, political parties traditionally have had a major say in which candidate is chosen to run for office, as well as the interests of politicians once they enter office. However, in “weak party” states like Massachusetts, Michigan, and California, politics has been much more individualistic. Beyond the way that political parties are modeled in our conceptual framework, we note that this is not the only way by which parties could affect electoral competition and performance. For example, Mayhew (2008) describes how parties may make it easier for incumbents to get re-elected via access to the resources of political machines, helping with canvassing and voter outreach.

¹³We hand-collected legislator salary data for 1967 to 2008. Gubernatorial salary data were collected for 1950-1994 by Di Tella and Fisman (2004) and were graciously provided by Ray Fisman. We hand-collected gubernatorial salary data for 1995 - 2008.

salary figure. Daily salaries are multiplied by the average number of days a legislature is in session over time. For legislators, salary data are from 1967 to 2008. For governors, salary data are from 1950 - 2008.

We use real salaries deflating with the national CPI (2009 level). In Section 4.6, we discuss how we obtain similar results deflating with separate state level price indices over time. As in Di Tella and Fisman (2004), we ignore non-salary benefits that politicians receive (e.g., transportation expenses, living *per diems*, etc.). States compensate non-salary expenses in different ways (e.g., some provide reimbursement on actual expenses, which are unobserved to the econometrician, whereas other states give fixed amounts), thereby precluding the creation of a consistent measure across states. We discuss this further in Appendix B. We do not believe that the inability to measure non-salary benefits is important for our paper’s findings.¹⁴

Other Characteristics of Legislatures and Legislative Productivity. From the *Book of the States*, we also hand-collected data on methods used to set salaries, session length over time, bill-passing over time, and on legislative support staff over time. We use the data on methods of compensation to help assuage concerns about endogenous wage-setting, examining how the impact of politician salary varies by method of compensation. Session length is an important control variable. Bill-passing is a standard measure of legislative productivity. We use data on legislative support staff as a control, one that is particularly useful in proxying a legislature’s degree of professionalisation over time.

Border Districts. To implement our border discontinuity design, we obtained data on electoral border districts from the website of the US census. The website provides coordinates for all state lower-house and upper-house districts, which were then read into ArcGIS. Using ArcGIS, a map was created of all districts which lay on a state boundary, based on the 2006 cartographic boundary files. These districts were then paired with the legislative districts bordering these districts in other states. See Appendix B.2 for details, including discussion of issues surrounding re-districting.

Politician Characteristics. To examine the impact of salary on political selection, we gath-

¹⁴Non-salary benefits represent an unobserved component of compensation for our analysis. Fixed differences across states will be accounted for in our state fixed effects, and trends in benefits over time within a state will be accounted for in our state-specific time trends. Even given state fixed effects and state time trends, one may worry that increases in salary could be correlated with increases in non-salary benefits. Such a possibility would be likely to bias upward the estimates (provided that non-salary benefits are also valued by potential challengers or cause incumbents to work harder), going in the opposite direction of our conclusion that salaries seem to have small effects. If salaries and benefits are negatively correlated, the bias would go toward biasing down our estimates.

ered data on politician characteristics. Data on legislator characteristics were kindly provided by Kathleen Bratton and are an extension of the characteristics given in Bratton and Haynie (1999). We have data on lower-house legislators in 11 states. For governors, we hand-collected data on gubernatorial primary candidates using Google searches. Details are provided in Appendix B.6.

4 Results

We begin this section by describing our estimation equation and follow by presenting our estimation results. The first set of results are on the impact of salary on electoral competitiveness, measured as the likelihood that an election is contested, the number of candidates running for office, the likelihood that the incumbent wins the election, and the margin of victory. We find an economically small but statistically significant positive relationship between politician salary and election competitiveness. We run several robustness checks on these results to try and reduce concerns associated with the endogeneity of our findings. We follow this by presenting our results on the link between salary and politician characteristics which suggest a small and statistically insignificant relationship between the two. Subsequently, we discuss our estimates of the relationship between salary and bill-passing and between salary and state fiscal policy (i.e. taxes and spending). We then turn to the relationship between salary and roll-call voting, a measure of politician shirking, and find a small, statistically significant, negative relationship. The rest of the section addresses additional threats to identification.

Our baseline specification for the empirical analysis is a fixed effects model:

$$y_{est} = \alpha_0 + \alpha_1 w_{st} + X_{st}\beta + f_s + f_t + \epsilon_{est} \quad (1)$$

where y_{est} is an outcome variable for election e in state s in year t ; w_{st} is the salary in state s in year t ; X_{st} is a vector of covariates (such as state population and state GDP); f_s and f_t are state and year fixed effects, respectively; and ϵ_{est} is an error.¹⁵ We estimate equations of this form for both state legislators and governors. The outcome variable, y , will include electoral competitiveness; candidate quality; legislative productivity; shirking; public goods provision; measures of outside-of-politics labor supply; and time allocation toward law-making, constituent services, and fundraising. We cluster standard errors at the state level (Bertrand et al., 2004).

¹⁵Our baseline estimates regarding electoral competitiveness use fixed effects for states instead of for electoral districts; results are similar with district fixed effects.

4.1 Effects on Electoral Competitiveness and Partisan Advantage

Legislators. Table 2 shows mostly modest impacts of salary on different measures of electoral competitiveness. The effect of Log Salary on an election being contested and on the number of candidates is positive and statistically significant, though the effect is small. The coefficient of 0.025 in column 1 means that a 100% increase in the salary is roughly associated with a 2.5 percentage point increase in the chance that the election is contested (73% of elections are contested overall).¹⁶ A 100% increase in salary is also associated with an increase in the number of candidates by 0.05 (the average number of candidates is 1.93). In addition, a higher salary for politicians is associated with a smaller margin of victory and a lower chance the incumbent is re-elected, but the effects are not statistically significant.¹⁷

Our estimates related to number of candidates are precise, and are small in comparison to those in other countries. For Brazil, Ferraz and Finan (2010) find that an increase of 100% in politician salary is associated with 2.7 more candidates per election (where the baseline number of candidates per seat is 6.1). For the EU, Fisman et al. (2013) find that a 100% increase in MEP salary increases the likelihood of running for re-election by 23 percentage points and increases the number of parties that field a candidate by a economically large amount.

Table D1 separately examines the effects of politician salary for upper and lower house elections. Politician salary only has a statistically significant effect in lower house elections.¹⁸

Three other results in Table 2 are also of interest. First, column 5 shows that increased salary is associated with a greater probability that Democrats will win the election. This result may reflect that Republican candidates are often businesspeople with significant independent wealth or income, whereas Democratic candidates are often lawyers or public servants who may be more responsive to political salary in choosing whether to run for office. Indeed, Table D3 shows that Republican legislators tend to have higher family income than Democratic legislators and are less likely to have

¹⁶Interpreting log changes in percentage changes is imperfect for larger changes. Given that $\log(2) \approx 0.7$, we are actually finding that a 70% increase in salary is associated with a 2.5 percentage point increase in the chance that the election is contested. These numbers are still generally an order of magnitude below what others have found.

¹⁷The interpretation of results related to margin of victory and whether the incumbent is re-elected are less clear than those related to number of candidates. On one hand, non-competitive elections are often associated with incumbents who are repeatedly re-elected by wide margins. On the other hand, by working hard and providing high quality governance, incumbents may make themselves more likely to be re-elected and to win by a larger margin.

¹⁸This could occur for several reasons. For example, political parties may exercise more control over upper house election, and better be able to regulate which candidates run. In addition, there may be greater non-salary returns for running for the upper house of a state legislature compared to the lower house; thus, the calculus for citizens of whether to run or not hinges less on the official salary.

prior governmental experience, using a large survey of state legislators that we make more use of later.¹⁹ Second, there are the expected (and somewhat mechanical) effects from term limits; term limits increase the likelihood of a contested election and the number of candidates running for election, and decrease margins of victory. Third, term length does not have a significant impact.

Legislator Robustness. To address the concern that our results could be biased by state-specific trends, Table D4 repeats Table 2 while adding state-specific time trends. Results are qualitatively similar.²⁰ We also split the sample by full-time vs. part-time legislatures in Table D6 to test whether the results vary with the demands of the job. We find that the effect of salary is more pronounced for part-time than full-time legislatures, and, in particular, the coefficients using the full-time legislature sample are not statistically different from zero for any outcome except margin of victory.²¹ Finally, in all these regressions, we use log wage as the independent variable of interest. We have also run the regressions in un-logged form, using instead the salary in terms of tens of thousands of dollars. The effects of politician salary on electoral competitiveness decline in significance in their un-logged form, reinforcing our general conclusion that effects are modest (if present).

Addressing Local Shocks. In these regressions, the identifying assumption is that politician salary is uncorrelated with the error term conditional on the spatial fixed effects, the time fixed effects, and other controls. In Section 4.6, we address a number of ways by which this assumption could potentially be violated. In the context of salary and electoral competitiveness for state legislators, one important confound we address here is the possibility of local economic shocks. As discussed in Sections 1 and 3.1, a key component of political competition is a prospective politician's outside offer, e.g., the economic opportunities for lawyers in his local area. One might worry that state legislative salaries could be raised or lowered in step with shocks to prospective politicians' outside option. To address this, we exploit that local economic shocks need not respect state borders. While the market for lawyers in California may be a poor comparison to that in Nevada, the markets for lawyers in two bordering legislative districts in eastern California and western Nevada may be similar.

To address shocks of this type, we implement a spatial discontinuity design, analyzing pairs of

¹⁹The argument that salaries could benefit Democratic legislators has been made previously in the political science literature, and a basic correlation (though without any time or state controls) was pointed out by Fiorina (1994).

²⁰When time trends are added, salary no longer significantly predicts the number of candidates, but unlike before, higher salary is significantly associated with a lower margin of victory.

²¹We do not have strong evidence as to why we see a modest positive effect of salary on competitiveness for part-time legislators, but not for full-time legislators. That the coefficients are only significant for part-time legislators highlights our general conclusion that impacts of salary seem to be modest (if they are present).

electoral districts straddling state borders. We consider regressions of the form:

$$y_{p(d)dst} = \alpha_0 + \alpha_1 w_{st} + X_{st}\beta + f_s + f_{p(d)} + f_t + \epsilon_{p(d)dst} \quad (2)$$

where $p(d)$ refers to a border district pairing, $f_{p(d)}$ is a border district pair fixed effect, and the other regressors are the same as in equation (1). We perform the above regression in a sample restricted to electoral districts that lie on state borders, including all pairs to which a given district belongs.²²

To highlight the logic of the spatial discontinuity strategy, consider a year where politician salary is raised in California, but not raised in Nevada. We wish to examine whether the increase in salary leads to greater competition. In estimating (2), the only electoral districts that will factor in are those along the state border. By including border pair fixed effects, our results are identified by changes in salary within a pair of electoral districts within a border pair, e.g., by an electoral district in Eastern California bordering one in Western Nevada. Such a design is not perfect, and could potentially be biased if there are time-varying shocks to given border district pairs. To address this, we can include border pair-year fixed effects (beyond including border pair fixed effects and year fixed effects), thereby making comparisons of one district to another in a given year. For equation (2) to be biased by a local economic shock once border pair-year fixed effects are included, there needs to be a shock to the market for lawyers in 1976 in western Nevada, but not in eastern California.^{23,24} A limitation of using border pair-year fixed effects is that it requires estimating thousands of fixed effects, thereby limiting our power.²⁵

In addition to helping with shocks to legislator outside options, our border district methodology is also useful for addressing other local shocks that could affect voting. For example, it could be that

²²Districts that belong to multiple pairings will be included multiple times. For discussion on standard errors, see Appendix B.

²³Of course, there are broader differences between California and Nevada, such as there is more gambling in Nevada, but time-invariant differences will be picked up in the state fixed effects. Our identifying assumption is that changes in unobserved conditions affecting one district in a border district pair are uncorrelated with changes in salary, conditional on all of our controls.

²⁴In the US, because of restrictions placed on lawyers by state bar associations, it may be difficult for lawyers to easily cross state borders to practice law. However, what matters for us is not that the lawyers can cross the border but merely that the relative demand for lawyers is similar in neighboring districts in a given year. We use lawyers as an example because they are a common occupational group for state legislators. However, lawyers actually only comprise about 20% of state legislators, and are outnumbered by state legislators with a business occupation, as tabulated in the survey of Carey et al. (2002). Businesspeople are generally able to freely move across state borders. One concern could be that businesspeople would not move across state borders if tax rates were much higher in one state than in the other, however, our results are similar when we include controls for state corporate tax rates.

²⁵For some border district pairs, the states have elections in different years. Thus, including border district pair-year fixed effects is the most conservative approach, but doing so may also lead to over-controlling. We have, thus, also estimated border pair-two year fixed effects and border pair-four year fixed effects, and obtain similar results to using border pair-year fixed effects.

voters' preferences change depending on the economic climate. By focusing on two nearby districts, we help generate conditions with a similar economic climate.²⁶

Table 3 implements the border district methodology and finds similar qualitatively similar results to those in Table 2. Panel A uses border pair fixed effects and year effects. The salary estimates are not statistically significant from zero (except for whether the Democrat candidate wins). However, based on the overlapping 95% confidence intervals between the two tables, we cannot reject that the estimates with the border pair design in Panel A of Table 3 are statistically different from the ones in Table 2. Panel B uses border pair-year fixed effects. The standard errors are larger (reflecting the large number of fixed effects being controlled for), but the estimates are broadly similar. Thus, like Table 2, Table 3 shows that increased legislator salary does not lead to economically significant increases in electoral competitiveness.

For our strategy to be effective in addressing concerns about salaries being increased in response to changes in the outside option, we need that the outside option of legislators be comparable on both sides of the border. However, some border legislative districts may be not good comparisons with one another. For example, in the western US, legislative districts are big and bordering districts may encompass wide geographies. However, our results are similar if we eliminate western states from the analysis. This does not completely address the issue, as even when districts are small, they may still not be good comparisons. Consider, for example, the cities of Philadelphia, PA and Camden, NJ, lying on opposite sides of a state border, but differing in their level of economic activity (Camden is more disadvantaged). To address this, we would like to run our analysis restricting to districts that are “similar” to one another. Unfortunately, we do not have historical data on the demographics of state legislative districts. Instead, we re-did our analysis restricting to districts where the two states are similar; doing this yielded qualitatively similar results.²⁷

Governors. In Table 4, we analyze the effect of politician salary in gubernatorial primaries. Since the general elections for governors usually have exactly two major candidates (one Democrat

²⁶For example, voter labor market shocks could affect the degree of accountability pressures for politicians or affect voter preferences.

²⁷To measure whether two states are similar to one another, we calculate the squared distance between two states across several covariates (share Black, share between age 5-17, share over age 65, unemployment, GDP per capita, and population). We average the covariates by state, normalize the average, and compute the sum of square difference over the normalized covariates. We then rank border districts by comparability and restrict to border districts spanning comparable states. We experimented with different thresholds of being “similar” (e.g., top 50% of border district pairs with most similar states, top 25%). In doing this, we reached the same qualitative conclusions as in the baseline border district analysis.

and one Republican), we focus on the results for gubernatorial primaries. The data shows no systematic relationship between gubernatorial salary and electoral competitiveness. The estimates are close to zero, though the standard errors are larger than in the legislator estimates. We are able, though, to rule out large effects. For the impact on number of candidates, a 50% increase in salary would have a 95% confidence interval of $[-0.18, 0.24]$. Given a mean of 2.56 candidates and a standard deviation of 1.43, we can rule out that the 50% salary increase would increase the number of candidates by more than 0.24 candidates or 0.17 standard deviations (σ) in candidates.²⁸ It is important to keep in mind, however, that there is far less variation in salaries for governors than there is for state legislators. An increase in 100% for gubernatorial salary is thus a very large amount.

4.2 Effects on Political Selection

Table 5 turns to analyzing the impact of politician salary on various politician characteristics including candidate diversity, candidate education and occupation, and past political experience. As discussed above, the measures are used simply because they are what can be observed and clearly do not represent all relevant measures of quality. The data are only available for some states, so there are fewer observations in Table 5 than in Table 2.

Panel A shows the effect of log salary on the characteristics of state legislators. There are no statistically significant correlations. Politician salary is actually negatively (though insignificantly) associated with the probability of a politician having a college degree. The coefficient of -0.014 , and the standard error of 0.025 means that we can rule out an effect of greater than 3.6% at the 95% confidence level (in the baseline, the share of college graduates is 77%). In contrast, the estimates of Gagliarducci and Nannicini (2010) indicate that a 100% increase in salary to be associated with an Italian mayor having an additional 2.7 years of schooling. Kotakorpi and Poutvaara (2011) find that a 35% increase in the salary of Finnish MPs increased the number of female candidates with higher education by 5 percentage points.²⁹

²⁸Table D2 estimates these effects separately for Republican and Democratic primaries. In general, for both groups, we find no relation between salary and electoral competitiveness, with an interesting exception for margin of victory in Republican primaries. In Republican primaries, we observe that higher salary is associated with a greater margin of victory. We do not have a clear explanation for this difference.

²⁹The reform did not affect the educational background of male candidates.

4.3 Effects on Passing Legislation and on Fiscal Outcomes

Table 6 shows modest impacts of politician salary on legislative productivity, which we measure using the introduction and passing of bills, as well as with income, expenditure, and taxes per capita. The results are presented both with and without session length, the issue being whether one wishes to interpret session length itself as possibly reflecting productivity.³⁰ In columns 1-4 of Panel A, we see positive, but statistically insignificant impacts of salary on bill introduction and approval. For example, the elasticity of 0.03 estimated in column 4 indicates that a 100% increase in salary is associated with a 3 percentage point increase in the number of bills approved. Given the standard error of 0.04, we can rule out that a 100% increase in salary would increase the share of bills approved by more than 11%. In columns 5 and 6, the impact on the share of bills approved is positively statistically significant (and is not affected much by including session length), but is economically small. The coefficient of 0.013 means that a 100% increase in salary increases the share of bills passed by roughly 1%, which is about 3% of the mean. Results are robust to including state-specific year trends (Table D5).

Panel B shows no significant impacts of salary on expenditure or taxes per capita, which are often used as measures of fiscal discipline (e.g., Besley and Case, 1995; Peltzman, 1992), or on income per capita. The impacts on income per capita are negative and insignificant, whereas the impacts on taxes and expenditure are positive and insignificant. Thus, there is no evidence that paying higher salary helps improve fiscal discipline.

In Panel C, we observe that salary for *governors* has no robust significant impact on fiscal outcomes. Increasing salary is associated with slightly lower income per capita and slightly higher spending and taxes per capita. There is no evidence that increasing salary would lead to greater fiscal discipline in terms of lower taxes and spending.³¹

Our findings differ substantially from others in the literature. Gagliarducci and Nannicini (2010) find that a 33% increase in Italian mayoral salary decreases expenditure and revenue per

³⁰For example, one might imagine that very unmotivated legislators might choose to have short session lengths. Thus, controlling for session length, there may be no effect of log salary on session length, even though the salary is affecting their productivity.

³¹For example, using a 95% confidence interval for expenditure per capita, we find that increasing salary by 50% would not decrease spending per capita by any more than 1.6%. Although we find it more plausible that governors influence fiscal policy (Besley and Case, 1995) as opposed to whether bills are introduced and passed, we also looked at the relationship between governor salary and bills introduced, bills passed, and share of bills passed. We found no significant impacts.

capita by about 18% and increases the speed of revenue collection by 7%. Ferraz and Finan (2010) show that a 20% increase in salary for Brazilian municipal legislators increases the share of bills submitted by 25%, and also substantially increases the share of bills approved and whether there is a local commission. Ferraz and Finan (2010) also show that raising salary raises spending on education and health. Our estimates are sufficiently precise that we can rule out magnitudes of this level in US data. In terms of bills introduced, our preferred estimate in column 2 corresponds to a confidence interval of [-2.2%, +2.6%] for a 33% increase in salary, meaning we can rule out effect sizes 6 times smaller than those in Gagliarducci and Nannicini (2010). In terms of bills approved, our preferred estimate in column 4 corresponds to a 95% confidence interval of [-0.9%, +2.2%] for a 20% increase in salary, meaning we can rule out effects over 10 times smaller than those in Ferraz and Finan (2010).

4.4 Effects on Shirking: Roll-Call Voting

Table 7 examines the effect of salary on a performance outcome that legislators have individual control over, namely, whether or not they miss a roll-call vote during 1999-2000. Missing roll-call votes is considered a form of shirking in the political science literature (e.g., Baughman and Nokken, 2011; Rothenberg and Sanders, 2000; Wright, 2007). Economics studies (e.g., Gagliarducci et al., 2010; Lott, 1990; Bender and Lott, 1996) have also examined whether politicians miss roll call votes or have examined similar measures of basic effort (e.g., whether they attend sessions of the legislature).

Column 1 of Table 7 indicates a statistically significant negative association of salary with whether legislators miss a vote. The coefficient of about -0.02 means that a 100% increase in salary is associated with a two percentage decrease in the probability that a legislator will miss their roll call vote (compared to a baseline missed vote percentage of 9 percent). This relatively small economic magnitude is broadly consistent with Fisman et al. (2013) who find no impact of salary on attendance or shirking.³² Because our data on roll-call voting is essentially from a cross-section, we do not include time effects, and because salary varies at the state level, there are no state fixed effects. Lacking state and time fixed effects, our conclusions here should be regarded as more tentative.³³

³²Fisman et al. (2013) define shirking as the fraction of times an MEP attended a session without casting any roll-call votes.

³³In a previous version of the paper, we showed that our broad conclusion that salary has a limited impact on missed roll call votes was also robust to using a border-pair research design.

4.5 Effects on Corruption

Table 8 shows that politician salary has no significant impact on corruption, our final measure of politician performance. Corruption is frequently studied as a critical measure of governmental performance both in developing countries (e.g., Ferraz and Finan, 2011; Olken, 2007) and in the United States (e.g., Glaeser and Saks, 2006). We measure corruption using two standard measures from political science: the annual number of corruption convictions per 1,000 elected state governmental officials (Maxwell and Winters, 2005) and the level of perceived corruption in state government based on a survey of State House news reporters (Boylan and Long, 2003). It should be noted that data on perceptions of corruption is cross-sectional.

Looking at convictions, column 1 shows that politician salary five years prior has no statistically significant impact on the number of convictions.³⁴ Convictions has been used to measure corruption in a number of papers, both in political science (e.g., Meier and Holbrook, 1992; Goel and Nelson, 1998) and in economics (e.g., Alt and Lassen, 2014; Campante and Do, 2013), and has the advantage of representing corruption validated in court. However, it is possible that the number of convictions could also reflect factors unrelated to corruption, such as the diligence of prosecutors, so we also investigate corruption perceptions. Using the cross-sectional survey of corruption perceptions in column 2, we also see no statistically significant impact.³⁵

4.6 Additional Threats to Identification

We discuss three additional important threats to identification and why we believe them to be unlikely to affect our main results.

Reverse causality: politician quality and wage-setting. One broad concern for the analysis is whether politician salary is set anticipating future performance or set differently depending on the unobserved quality of the politicians. As discussed in Section 2, governor salaries are set by

³⁴Recognizing that there may be significant variation across cases in time from crime commission to conviction, we experimented with a number of different lengths of leads in sensitivity checks. The estimates vary by specification. However, we found no significant evidence that higher salaries lead to less corruption. One important caveat to keep in mind regarding the convictions measure of corruption is that it measures all of a state's corruption convictions, including those from both elected and non-elected officials. At the moment, separate data on legislator corruption convictions are not available (see Data Appendix for details). Dincer and Johnston (2014) note, however, that corruption is perceived to be significantly higher in state legislatures relative to other branches of government, suggesting that measuring overall convictions may be a reasonable proxy for convictions by state legislators.

³⁵Measuring corruption through survey perceptions has been used in many economics papers (e.g., Mauro, 1995; Treisman, 2000).

the upper legislative house, whereas legislator salaries are set by different methods in different states. For example, salaries may be set to conform to voters' beliefs about appropriate compensation given politicians' quality and behavior. For example, salary-setters may be reluctant to raise politician salary if they have unobservable (to the econometrician) information that politicians will shirk and have an unproductive year due to fear of popular backlash against higher salaries for low performance. If salaries are set to equal politicians' *expected* marginal products, this would lead us to *overestimate* the positive impact of salary, with higher salary being driven by higher performance instead of the other way around. Such a bias would work against us finding very modest impacts of politician salary on outcomes (compared to other papers in the literature).

However, it is possible that the bias could go in the other direction. As noted by Gagliarducci and Nannicini (2010), if politicians are setting salaries for themselves, it may be that higher-quality politicians do not wish to set themselves higher salaries, either because they are honest or they do not need the money. Such a bias could confound our main results.

We attempt to address this concern with two robustness checks. First, we would imagine that the bias from unobservedly higher quality politicians not wishing to raise their salaries would differ by the method of compensation. The bias might presumably be stronger when politicians are voting on setting salaries for themselves by voting on compensation bills. Tables D7 and D8 repeat our main analysis of politician salary on electoral outcomes and performance, respectively, restricting the sample to states where politicians do not essentially set their own salary. Results are similar.³⁶

Second, we examine whether observedly better performing politicians are less likely to receive increased salaries. We measure observable quality in several different ways. First, we measure quality using performance in year $t - 1$, regressing salary in period t on performance in $t - 1$ in Table D9.³⁷ Second, we measure quality using average performance to date; Table D10 shows no significant relationship between average performance to date for legislators and salary. Third, we measure quality in terms of observables, asking whether more educated politicians are less likely to receive a salary increase. To the extent that observable measures of politician quality are correlated with unobservable measures, we find little evidence that higher quality politicians are less likely to receive increased salaries.

³⁶Note that we can only do this robustness check legislators and not for governors because gubernatorial salary are set in all states by the upper house (Di Tella and Fisman, 2004).

³⁷Di Tella and Fisman (2004) show that when governors perform well, their salaries are often increased in the future, so there is no certainly no evidence there that higher performance is associated with lower salaries.

Unobserved changes in legislature characteristics or political institutions. Another broad concern is that changes in politician salary may be accompanied by unobserved changes in legislature characteristics or political institutions that affect performance. The late 1960s and 1970s saw a large increase in the “professionalization” of state legislatures, where wages were increased, along with other changes such as lengthening legislative sessions and providing increased legislative staff (Fiorina, 1994). These changes have continued to the present, with gradually longer sessions and annual instead of bi-annual meetings. Although we collected data on staff and session length and control for these variables, it is possible that wage changes may be accompanied by unobserved changes in legislature characteristics, e.g., the capital buildings or the facilities. While we cannot eliminate this concern altogether, we attempt a selection on observables design (Altonji et al., 2005, 2008) as a robustness test.³⁸ Because our concern is about changes in state-year legislative unobservables, we use measures of legislative staff and aides, the number of meetings per year, and the length of meetings to estimate the model. As seen in Table D11, our estimates are stable as observable proxies of professionalization are gradually added. As long as these proxies of professionalization also correlate with unobservable measures of professionalization, there would need to be substantial selection on unobservables to explain the result if it were spurious.

Differences in cost of living over time. So far, we have deflated politician salaries using the CPI, and have accounted for time-invariant differences across states by using state fixed effects. However, as detailed in a growing body of work (e.g., Moretti, 2013), the cost of living has changed very differently in different states over the last several decades. Although legislators in California receive high salaries compared to those in other states, the cost of living has increased substantially in California over time compared to many other states. To account for this, we created a state-level CPI using housing and grocery store price data, closely following the methodology in Moretti (2013). Our results are similar when we deflate salaries using state-level CPIs (Tables D12 and D13).³⁹

Additional time-varying state characteristics. Our regressions control for state GDP and population over time, as they seemed the most important characteristics to control for for our analysis. We re-did Table 2 with additional controls (share Black, share between age 5-17, share over

³⁸This approach is similar to an approach used in Gentzkow et al. (2012) in that we assume that the correlation between state-year unobservable shocks and election competition and outcomes is the same as the correlation between state-year observable shocks and our outcome variables.

³⁹Pricing data used to calculate the state level CPIs is available starting in 1980. For years prior to 1980, we use the national level CPI.

age 65, and state unemployment rate) and obtained similar estimates.

4.7 Narrative Analysis of Salary Changes and Non-Changes

The identifying assumption for our analysis is that politician salary is uncorrelated with the error, conditional on controls. To address the issue of local economic shocks biasing the relationship between salary and competitiveness in legislative elections, we used a spatial discontinuity methodology. We tried to rule out additional threats to identification in Section 4.6. To try to address any lingering concerns about endogeneity, we perform a narrative analysis.

Using old newspapers, we searched the historical record for salary changes or non-changes that are plausibly exogenous with respect to various concerns. We looked for proposed salary increases whose passages were contested. For example, we located cases where the salary change passed or not according to a vote of a divided state salary commission, based off of a close vote of the electorate, or because of some other event (e.g., the bill was vetoed, but then the veto declared unconstitutional; an unknown citizen led a drive to defeat the bill, etc.). A full explanation of our methodology and all the salary changes are given in Appendix B.13.⁴⁰

The purpose of the narrative analysis is to identify changes and non-changes which are plausibly exogenous for our analysis. That is, the events are plausibly exogenous with respect to factors such as shocks to politicians' outside options, expectations about future performance, and unobserved changes in political institutions. A disadvantage of the narrative approach is that it is based on a much smaller sample size, based off of 17 salary change or non-change events. As such, we believe that our full analysis and our narrative approach are complementary.⁴¹

Table D14 shows very limited impact of salary electoral competitiveness using our narrative sample. We analyze elections in states in the 8 years before and after the salary event.⁴² The estimates are relatively precise, e.g., in column 1, looking at whether an election was contested as

⁴⁰Our methodology is similar in spirit to Romer and Romer (2010), who identify exogenous tax changes using a narrative approach.

⁴¹The full analysis has the strength of a larger sample, whereas the narrative analysis has the virtue of using events which we are more confident are plausibly exogenous. Both approaches deliver similar results. Beyond the limitation of only having 17 event observations, we also only have 11 states. Because we are clustering by state, this means we have relatively few clusters and asymptotic inference may not be valid (Bertrand et al., 2004). Cameron et al. (2008) show that one approach to address this is to perform inference using $G - 2$ degrees of freedom, where G is the number of clusters. Using $G - 2$ degrees of freedom decreases the statistical significance of the log salary coefficient, meaning the coefficients on log salary remain statistically insignificant.

⁴²Thus, elections will be counted multiple times if they are within 8 years of multiple events. Our results are similar if we look at 4 year windows instead of 8 year windows.

the outcome, the 95% confidence interval on “Log Salary” is $[-0.05, 0.08]$. Repeating the regressions in Table 6 with our narrative sample, we also see limited impact of salary on bill introduction and bill passing (results omitted for brevity).

5 Interpretation

We discuss several explanations and interpretations of our results:

- That salaries are too small to be meaningful to politicians.
- That politicians are driven by intrinsic motivations.
- That salary impacts are small because salary is not a good measure of the pecuniary value of serving in office. Specifically, state politicians could have many future opportunities to become lobbyists and consultants, particularly in low salary states.
- That the small impacts of salary on entry and performance are driven by political parties serving as a barrier to entry.

Although not dispositive by any means, additional evidence fails to support the first three explanations, and seems more supportive of the fourth explanation.

Politician Salaries are too Small to be Meaningful. One explanation for our results is that politician salaries are too small to have much of an influence on outcomes. Potential politicians are reasonably well-educated, high-ability people, so differences in several hundred or thousand dollars may be too small to significantly affect household finances or politician behavior. However, Table D15 shows that politician salary is significantly and positively associated politician family’s self-reported income and decreases outside labor supply (i.e., “moonlighting”).⁴³ In Panel A, the dependent variable is family income, which has a mean of \$101,394. Thus, formal politician salaries comprise about 24% of household income, on average. Each \$1 increase in politician salary is associated with a 50 cent increase in reported family income.

Of course, that politician salaries are a substantial portion of family income does not mean that they would actually affect behavior. For example, it may be that post-office returns (e.g., lobbying) are much larger than compensation received in office (we address this further below). However, Panel B suggests that an increased salary does affect behavior, namely by decreasing the probability the politician has an outside job. An increase in politician salary by \$10,000 is associated with a four

⁴³For an analysis of moonlighting by Italian politicians, see Gagliarducci et al. (2010).

to six percentage point decrease in the likelihood that a politician has an outside job (corresponding to a change of 6-9 percent).⁴⁴ The effect holds conditional on session length, suggesting the effect is not driven simply by legislatures with short sessions having politicians who are both more likely to earn lower salaries and have outside jobs. Given that politicians' decisions about whether to work outside of politics change significantly with changes in salary, it does not seem likely that salaries are too small to potentially meaningfully shift performance.

Intrinsic Motivations. An important body of work suggests that workers are often driven by intrinsic motivations, thereby blunting or reversing the impact of extrinsic rewards via crowd-out (Gneezy and Rustichini, 2000; Ariely et al., 2009). Though we are dealing with salary instead of performance pay, the relatively small impact of salary on behaviors is consistent with this view. We show, however, that salary is correlated with the way that politicians spend their time, suggesting that politicians are significantly motivated by extrinsic incentives. Higher-paid politicians spend much more time on fund-raising, and no more or less time on legislative activities.

We use self-reported time use data from Carey et al. (2002). State legislators in 2002 were asked to report how much time they actually spent on one of several activities going from 1='Hardly Any' to 5='A Great Deal.' We normalize these variables to use as outcomes in Table 9.⁴⁵

Panel A of Table 9 shows that when politicians receive higher pay, they do not seem to report spending more time on legislative activities. They report spending significantly *less* time on building coalitions across parties. Panel B shows that higher paid politicians do report spending significantly more time on constituent services, such as keeping in touch with constituents and helping constituents with problems. Panel C shows a significant positive relationship between politician salary and fundraising. In states where the wage is higher, politicians spend more time fundraising. Column (8) implies that an increase in salary of \$30,000 is associated with 0.16σ more time spent fund-raising. It is noteworthy that a higher salary is associated with spending more time fundraising

⁴⁴It is important to caveat that the analysis in Tables D15 is cross-sectional. While we control for various state characteristics that seem relevant for the analysis, such as state population, GDP per capita, session length, session frequency, and legislative staff controls, it is possible that salary could correlate with unobserved state characteristics. Although we speculate that unobserved state characteristics are not a significant source of bias here, the results should be treated as more tentative than those with state fixed effects. Our table on time-use, Table 9, is also based on cross-sectional comparisons.

⁴⁵While we use data from 2002, legislator time use data are also available for 1995 (Carey et al., 1995). However, in 1995, fewer questions were asked, and one of our main variables of interest (time spent fundraising for oneself) is not available. As a robustness check, we ran pooled analyses combining the 1995 and 2002 surveys for variables available for both surveys. Our results are robust.

for oneself (a \$30,000 salary increase is associated with 0.20σ more time fundraising in column (9)), but that it is not associated with time spent fundraising for others.

Rather than either significantly increasing performance or having no impact on behavior, higher salaries seem to increase time spent on fund-raising and constituent services. This suggests that politicians are not non-responsive to external incentives due to strong intrinsic motivations, but rather that they may respond by changing other behaviors. Although we are dealing with salary instead of performance, these results have parallels to the multi-tasking literature, where incentives on one dimension affect behavior on other dimensions (Holmstrom and Milgrom, 1991).⁴⁶

Political Office Leads to Lobbying Opportunities. One explanation for the limited relationship between salary and performance is that salary may not be a good measure of the pecuniary value of serving in office. It could be that being a legislator allows one to later work as a lobbyist or public policy consultant. However, in the state legislator surveys of Carey et al. (1995, 2002), only 15% of legislators said that it was likely that they would later pursue lobbying or consulting after leaving office. Furthermore, there is no correlation between legislative salary and whether legislators say it is likely they will later pursue lobbying or consulting (see Table D16). While it is possible that legislators could deliberately mis-report this information, the survey was anonymous and reported via mail; thus, there was little incentive to mis-report.⁴⁷

Strong Political Parties. Political scientists have long emphasized the importance of parties for understanding differences across states, arguing that parties play a role in determining who runs for office, in maintaining patronage networks, and in regulating whether incumbents face challengers (e.g., Mayhew, 1986, 2008; Primo and Snyder, 2010). Thus, it was natural for us to look to differences in party strength in helping understand why we find modest impacts of salary on electoral competition and performance relative to several other papers on the impact of politician salary.⁴⁸

To measure the strength of political parties, we turn to Mayhew (1986). He performs a detailed state-by-state analysis on whether political parties have historically been powerful, focusing on the

⁴⁶Although it seems (to us) that having politicians spend more time on fundraising, but no more time on legislative activities may not be beneficial, we do not need to take a stand here on whether the data patterns found here are “good” or “bad.” Spending lots of time on constituent services may be thought clientelistic or it may be thought socially efficient (compared to, say, drafting a bill that will never pass). Our point is merely that salary is correlated with time use, suggesting that intrinsic motivations do not make politicians impervious to extrinsic incentives.

⁴⁷See Carey et al. (1995, 2002) for further discussion and justification of the survey methodology.

⁴⁸In theoretical work in economics, there is also growing interest in understanding how party strength affects the political process and the impact of political institutions (e.g., Buisseret and Prato, 2014).

degree to which local politics are run by organized party machines. All states are given a ‘Total Party Organization’ score from 1 to 5, and states are divided into Organization States and Non-Organization states. For our analysis, we focus on a simple binary comparison of strong and weak party states. Following Primo and Snyder (2010), as well as the discussion in Mayhew (1986), strong party states are ones with a Total Party Organization score of 4 or 5, whereas weak party states have a Total Party Organization score of 1, 2, or 3.

In states with strong parties, potential candidates may not be able to respond to salary increases because, for instance, the party might decide who runs for office, in addition to driving political effort and other outcomes. As a result, while individual politicians may be responsive to salary changes, the decisions of a strong party may override these incentives. Table D17 repeats our main regressions on electoral competitiveness while adding an interaction between log salary and an indicator for whether a state has strong political parties. For all five regressions, the interaction term of Strong Party x Log Salary goes in the opposite direction of the coefficient on Log Salary. That is, while salary is modestly associated with greater competitiveness in weak party states, the association is muted (or even overturned) in strong party states. The interaction term is statistically significant in column 3, and narrowly misses significance at the 10% level in columns 1 and 4. These results are consistent with strong parties being a barrier to entry.⁴⁹

6 Conclusion

We examine the impact of salaries for state legislators and governors on political competition, politician quality, and political performance in the US. Our main finding is that effects are modest. Some effects are positive and statistically significant, some are statistical zeroes, but almost all are economically insignificant. Most estimates are relatively precise, with enough precision to reject moderate-sized effects. Although mostly inconsistent with reduced-form international evidence on the impact of politician salary, our reduced-form results are consistent with simulations from structural models (Diermeier et al., 2005; Keane and Merlo, 2010), which also find that the effect of

⁴⁹In unreported results, we also studied the interaction effect of salary and party strength on politician characteristics, bill-passing, fiscal policy, missed roll call votes, and time use. In these regressions, we do not generally significantly different impacts of salary for weak vs. strong party states. One interpretation of this is that party strength is only a mediating factor for electoral competition outcomes, but not for performance or politician quality outcomes. This is consistent with some of the discussion in Mayhew (2008), which emphasizes the role of parties in their impact on electoral competition rather than other outcomes.

increasing politician salary on selection and performance is likely be small for the US. Our results are of interest both for agency theory and for policy debates about politician pay.

The central concern for our paper is whether results may be biased due to correlation of wages with unobservables. We examine several possible sources of bias. To address the concern that estimates of salary on political competition may be biased by correlation between salary and unobserved local conditions, we consider a spatial discontinuity design of legislative districts straddling state borders. To address the concern that wages may be correlated with changes in legislature characteristics or political institutions, we examine how our coefficients change as we control for observable proxies, in the spirit of Altonji et al. (2005). To assuage the concern that higher quality politicians may strategically try to avoid receiving higher salaries, we show our results are similar when we restrict to states where politicians have less control over their salary, and that past observable measures of quality do not appear correlated with reluctance to raise salary. While our results are quite robust to these tests, we recognize these tests are imperfect, and without quasi-experimental variation in politician salary in the US, possible concerns about endogeneity may still remain. Based on our accumulated evidence, however, we feel comfortable concluding that moderate increases in politician salary, in the range of salaries we study, are unlikely to have large impacts on the performance of US state politicians.

The obvious question given our findings is why are impacts modest. Based on time use data, we show that US politicians may respond to salary increases on other effort margins, such as time spent fund-raising. Further research is clearly warranted.

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Figure 1: Legislator Pay Over Time By State, In Nominal Terms

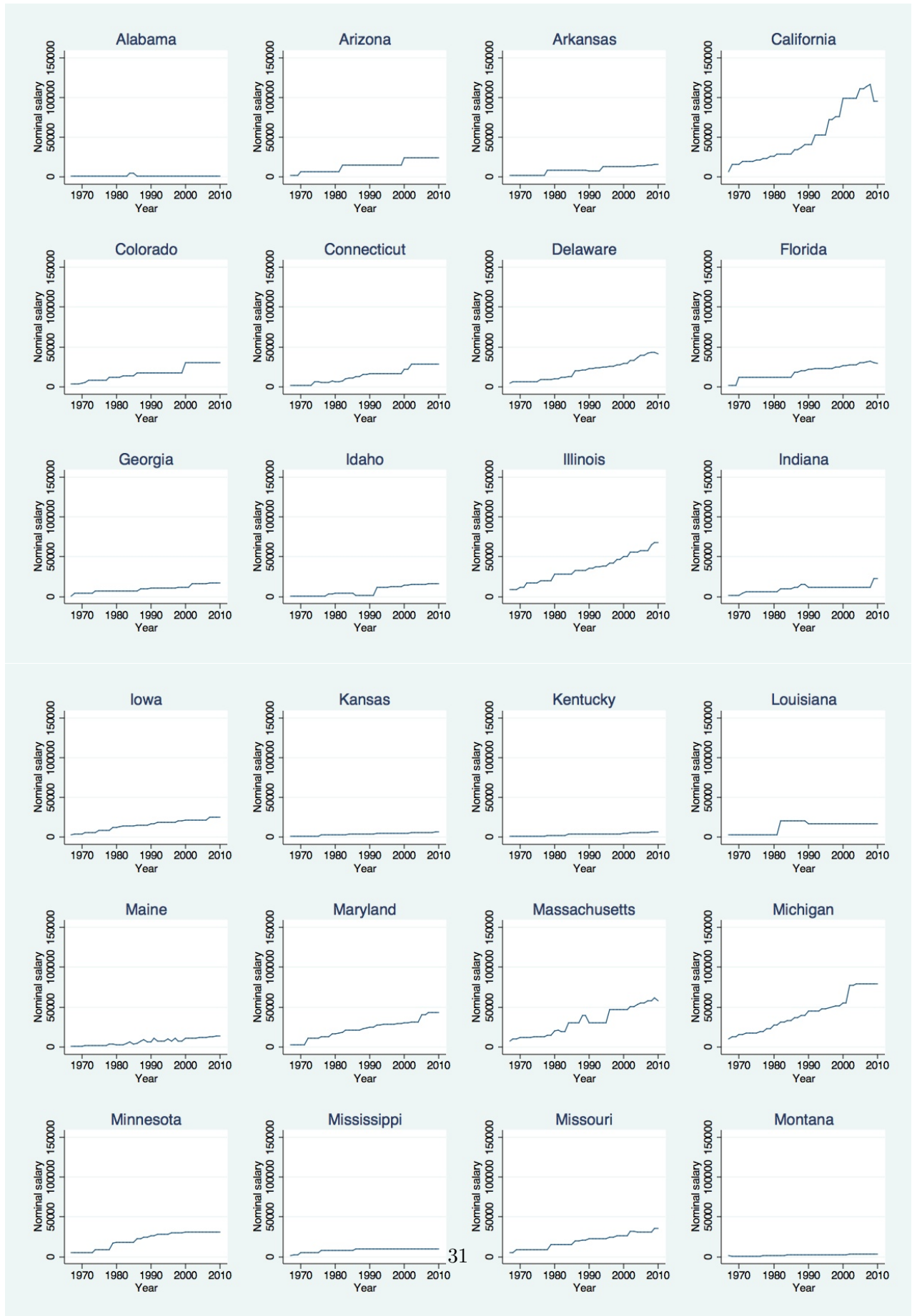


Figure 1: Legislator Pay Over Time By State, In Nominal Terms (Cont.)

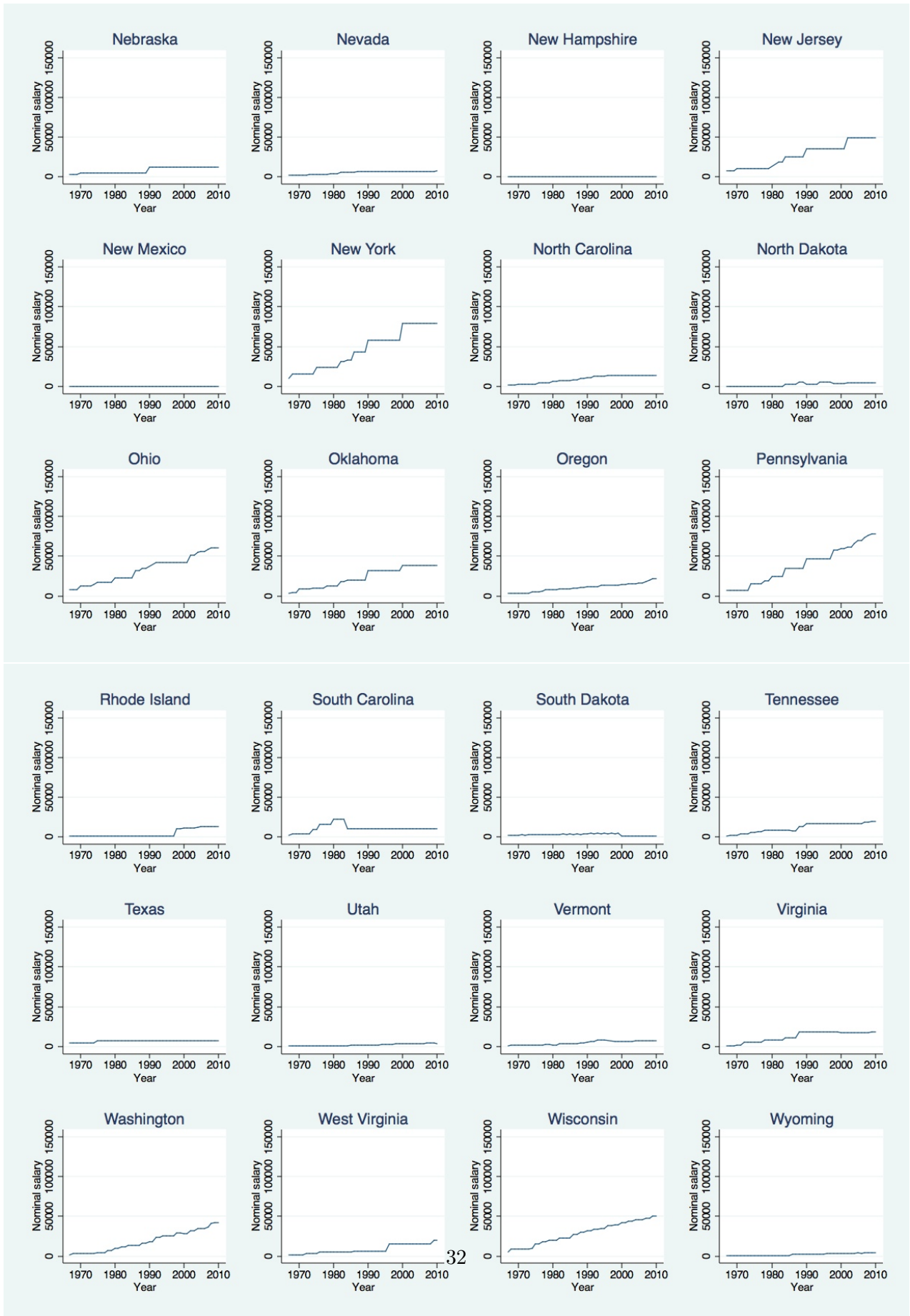


Table 1: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A - State Legislators					
<u>Legislature & Election Characteristics</u>					
Nominal salary	84,592	15420.23	15609.71	0	99,000
Contested election	86,401	0.73	0.43	0	1
Number of candidates	86,401	1.93	0.74	1	20
Number of candidates, 6 or fewer candidates	86,378	1.92	0.70	1	6
Margin of victory (percentage points)	84,043	47.04	35.51	0	100
Incumbent is re-elected	86,222	0.68	0.46	0	1
Incumbent runs for re-election	86,212	0.74	0.44	0	1
Incumbent re-elected conditional on running	63,510	0.93	0.26	0	1
Incumbent re-elected conditional on running and facing an opponent	41,312	0.90	0.30	0	1
Election won by a Democrat	85,889	0.59	0.49	0	1
Election for term-limited seat	86,404	0.06	0.24	0	1
Election for seat with four-year term length	86,404	0.18	0.38	0	1
Election for seat with two-year term length	86,404	0.82	0.38	0	1
Election for seat where pay is set by constitution	71,182	0.16	0.36	0	1
Election in a border district	86,404	0.20	0.40	0	1
<u>Legislator Characteristics</u>					
Female legislator	12,052	0.21	0.41	0	1
Black legislator	12,052	0.10	0.30	0	1
Latino legislator	11,978	0.06	0.23	0	1
Legislator age	11,313	49.61	11.44	19	94
College Degree	11,012	0.77	0.42	0	1
Graduate Degree	11,012	0.44	0.50	0	1
Lawyer	11,041	0.23	0.42	0	1
Has political experience	11,204	0.39	0.49	0	1
<u>Legislature Productivity</u>					
Number of bills introduced	1,771	2054.12	2339.15	6	21,435
Number of bills introduced per legislator	1,305	13.87	12.50	0.06	101.59
Number of bills approved	1,771	467.91	337.56	0	2,361
Number of bills approved per legislator	1,305	3.51	2.53	0.05	17.51
Share of bills approved	1,771	0.31	0.17	0.00	0.97
Missed roll-call vote (observation is a vote)	3,282,096	0.09	0.28	0	1
<u>Other</u>					
Strong State Party	48	0.27	0.45	0	1
Panel B - State Governors					
Real Salary (in 1982 dollars)	1,326	61,035	19,714	16,598	172,414
<u>Gubernatorial Primary Election Characteristics</u>					
Contested election	1,323	0.80	0.40	0	1
Number of candidates	1,325	3.12	2.17	1	18
Number of candidates, 6 or fewer candidates	1,232	2.70	1.43	1	6
Margin of victory (percentage points)	1,325	0.46	0.36	0	1
Margin of victory (percentage points), contested election	1,062	0.33	0.27	0	1
Election for term-limited seat	1,326	0.50	0.50	0	1
<u>Governor Characteristics</u>					
Female governor	344	0.03	0.16	0	1
Governor age	344	48.30	8.03	33	71
College degree	344	0.95	0.22	0	1
Lawyer	343	0.55	0.50	0	1
Has military experience	344	0.58	0.49	0	1
Born out of state	344	0.30	0.46	0	1
Worked in private sector after office	231	0.44	0.50	0	1
Worked in public sector after office	231	0.60	0.49	0	1
Retired after office	231	0.05	0.21	0	1

Notes: For legislators, summary statistics are based on our sample period (1967-2003).

Table 2: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes

	(1)	(2)	(3)	(4)	(5)
Dep var:	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.026** (0.013)	0.050** (0.020)	-0.747 (1.199)	-0.015 (0.013)	0.066** (0.025)
Upper house	0.070*** (0.021)	0.114*** (0.031)	-3.546** (1.526)	-0.017 (0.011)	-0.013 (0.022)
Session length in election year (in hundreds of days)	0.011 (0.014)	0.015 (0.028)	-0.191 (0.948)	0.008 (0.010)	0.020 (0.012)
Election for term-limited seat	0.068** (0.032)	0.109* (0.062)	-3.494 (2.708)	-0.142*** (0.030)	0.019 (0.035)
Election for seat with four year term length	-0.023 (0.023)	-0.026 (0.033)	-0.576 (1.665)	-0.110*** (0.016)	0.018 (0.026)
Observations	82,533	82,447	80,248	82,353	82,027
Mean dep var	0.734	1.928	47.10	0.685	0.588
R-squared	0.161	0.175	0.162	0.141	0.143

Notes: This table analyzes the effect of legislative salary on candidate selection using US state legislative elections from 1967-2003. An observation is an election. Robust standard errors clustered by state in parentheses. All regressions include state and year fixed effects, as well as controls for the log of state population and the log of state GDP per capita. The contested election variable is a dummy for whether the election had more than one candidate. Column 2 is restricted to elections with 6 or fewer candidates. Log salary is in real dollars. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes, Analysis for Border Districts

Panel A - Border District & Year Fixed Effects					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	-0.001 (0.015)	-0.012 (0.026)	1.236 (1.849)	0.020 (0.016)	0.079** (0.034)
Session length in election year (in hundreds of days)	0.037 (0.029)	0.067* (0.040)	-0.998 (2.092)	-0.011 (0.015)	0.020 (0.027)
Election for term-limited seat	0.006 (0.039)	0.073 (0.056)	1.025 (4.197)	-0.150*** (0.038)	-0.014 (0.055)
Election for seat with four year term length	0.046 (0.031)	0.059 (0.048)	-6.451** (3.151)	-0.052** (0.025)	0.018 (0.046)
Observations	26,490	26,454	26,142	26,456	26,387
R-squared	0.225	0.255	0.264	0.188	0.392
Mean dep var	0.725	1.910	48.61	0.690	0.594
Panel B - Border District X Year Fixed Effects					
Dep var:	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.056 (0.043)	0.035 (0.064)	-2.702 (2.516)	0.061 (0.048)	0.065 (0.050)
Session length in election year (in hundreds of days)	0.036 (0.050)	0.038 (0.068)	1.095 (2.902)	-0.001 (0.032)	-0.025 (0.048)
Election for term-limited seat	0.164* (0.091)	0.203** (0.094)	-12.868 (8.963)	-0.267** (0.100)	-0.120 (0.107)
Election for seat with four year term length	0.048 (0.051)	0.089 (0.063)	-8.559 (5.799)	-0.037 (0.060)	0.015 (0.096)
Observations	26,490	26,454	26,142	26,456	26,387
R-squared	0.739	0.783	0.760	0.754	0.768
Mean dep var	0.725	1.910	48.61	0.690	0.594

Notes: This table analyzes the effect of legislative salary on candidate selection using US state legislative elections from 1967-2003, restricting to elections in districts on state borders. An observation is an election. Robust standard errors clustered by state in parentheses. In Panel A, all regressions include border district pair and year fixed effects. In Panel B, all regressions include border district pair-year fixed effects (e.g. fixed effect for elections in District A in eastern California and District B in western Nevada in 1970). Regressions in both panels include state fixed effects, as well as controls for the log of state population and the log of state GDP per capita. The contested election variable is a dummy for whether the election had more than one candidate. Column 2 is restricted to elections with 6 or fewer candidates. Log salary is in real dollars.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: The Effect of Governor Salary on Electoral Competitiveness and Electoral Outcomes, Gubernatorial Primary Elections

	(1)	(2)	(3)	(4)
Dep var:	Contested Election	Number of Candidates	Margin of Victory (in percentage points)	Margin of Victory for Contested elections (in percentage points)
Log salary	0.034 (0.049)	0.016 (0.276)	-0.291 (6.032)	-0.921 (5.832)
Democratic primary	-0.012 (0.017)	0.303* (0.161)	-1.421 (2.785)	-1.879 (2.690)
Election for term-limited seat	0.084 (0.054)	-0.151 (0.352)	-3.111 (5.037)	-1.408 (4.998)
Observations	1137	1150	962	947
R-squared	0.407	0.508	0.483	0.481
Mean dep var	0.721	2.294	49.15	29.44

Notes: This table analyzes the effect of legislative salary on candidate selection using gubernatorial primary elections from 1950-1994. An observation is an election. Robust standard errors clustered by state in parentheses. All regressions include state and year fixed effects, as well as controls for the log of state population and the log of state GDP per capita. The contested election variable is a dummy for whether the election had more than one candidate.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: State Politician Salary and Politician Characteristics

Panel A - State Legislative Salary and Legislator Characteristics							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep var:	Female	Black	Latino	Age	College Degree	Lawyer	Has Political Experience
Log salary	0.024 (0.015)	-0.017 (0.010)	0.027 (0.023)	-0.615 (0.775)	-0.019 (0.024)	0.015 (0.021)	0.016 (0.027)
Election for term-limited seat	-0.029 (0.022)	-0.004 (0.012)	0.026 (0.024)	-3.110*** (0.755)	-0.009 (0.016)	0.000 (0.013)	0.040 (0.029)
Session length in election year (in hundreds of days)	-0.005 (0.010)	0.001 (0.005)	0.004 (0.011)	0.133 (0.424)	0.001 (0.020)	-0.021* (0.010)	-0.018 (0.021)
Observations	9,904	9,903	9,839	9,286	8,981	9,000	9,094
R-squared	0.034	0.017	0.076	0.077	0.025	0.032	0.084
Mean dep var	0.203	0.096	0.051	49.03	0.766	0.237	0.380
Panel B - Governor Salary and Governor Characteristics							
	(1)	(2)	(3)	(4)	(5)		
Dep var:	Female	Age	College Degree	Lawyer	Has Political Experience		
Log salary	-0.042 (0.088)	6.540** (2.524)	0.004 (0.048)	-0.132 (0.165)	0.117 (0.175)		
Election for term-limited seat	-0.110** (0.046)	-0.936 (2.802)	0.005 (0.044)	-0.186 (0.190)	0.055 (0.180)		
Observations	1710	1710	1710	1707	1710		
R-squared	0.276	0.269	0.284	0.277	0.286		
Mean dep var	0.025	48.25	0.945	0.551	0.589		

Notes: Panel A analyzes the effect of legislative salary on candidate selection using US state legislative elections from 1967-2003. Panel B analyzes the effect of gubernatorial salary on candidate selection using gubernatorial primaries from 1950-1990. An observation is one candidate. The dependent variable is a candidate characteristic. For example, we examine whether higher pay is associated with a greater chance that a legislator is female or is a lawyer. Panel A is restricted to data from 11 states, whereas Panel B contains data from all states. Robust standard errors clustered by state in parentheses. All regressions include state and year fixed effects, and controls for the log of state population and the log of state GDP per capita.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: The Effect of Politician Salary on Productivity

Panel A - Legislator Salary and Bill Introduction, and Approval						
	(1)	(2)	(3)	(4)	(5)	(6)
Dep var:	Log(Number of Bills Introduce)		Log(Number of Bills Approved)		Share of Bills Approved	
Log salary	0.017 (0.036)	0.004 (0.037)	0.047 (0.040)	0.034 (0.040)	0.013* (0.007)	0.013* (0.007)
Session length in election year (in hundreds of days)		0.215* (0.119)		0.242** (0.114)		0.012 (0.012)
Observations	1,467	1,463	1,466	1,462	1,467	1,463
R-squared	0.777	0.783	0.639	0.648	0.679	0.680
Mean dep var	7.274	7.272	5.961	5.960	0.323	0.323
Panel B - Legislator Salary and Taxes, Expenditures, and Income						
	(1)	(2)	(3)	(4)	(5)	(6)
Dep var:	Log(Income per Capita)		Log(Expenditure per Capita)		Log(Taxes per Capita)	
Log salary	-0.005 (0.003)	-0.005 (0.003)	0.014 (0.009)	0.013 (0.009)	0.016 (0.010)	0.015 (0.010)
Session length in election year (in hundreds of days)		-0.000 (0.002)		0.004 (0.009)		-0.008 (0.010)
Observations	1,061	1,058	1,061	1,058	1,061	1,058
R-squared	0.998	0.998	0.989	0.989	0.984	0.985
Mean dep var	2.705	2.704	7.518	7.517	6.832	6.831
Panel C - Governor Salary and Taxes, Expenditures, and Income						
	(1)	(2)	(3)			
Dep var:	Log(Income per Capita)	Log(Expenditure per Capita)	Log(Taxes per Capita)			
Log Salary	0.016 (0.020)	-0.001 (0.031)	0.023 (0.032)			
Election for term-limited seat	-0.032*** (0.011)	-0.031 (0.019)	0.002 (0.023)			
Observations	1943	1943	1943			
R-squared	0.995	0.991	0.987			
Mean dep var	2.674	7.500	6.809			

Notes: This table analyzes the effect of salary for state politicians on politician performance. Panel A studies the impact of legislator salary on bill-passing using data from 1968-2007. Panel B studies the impact of legislator salary on state finances using data from 1972 and 1977-2000, as described in the Data Appendix. Panel C studies the impact of governor salary on state finances using data from 1972 and 1977-1994, as described in the Data Appendix. All regressions include state fixed effects, year fixed effects, a dummy for the state having term limits for legislature seats in that year, and controls for the log of state population and the log of state GDP per capita. An observation is a legislature-year. Observations are missing for some states in some years due to missing data on bills introduced and enacted. Robust standard errors clustered by state in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Legislative Salary and Missed Roll Call Votes

	(1)	(2)
Dep var:	Missed Vote (0 or 1)	
Log salary	-0.024*** (0.007)	-0.023*** (0.006)
Session length in election year (in hundreds of days)	0.009 (0.016)	0.008 (0.016)
Log(Population)	0.071 (0.046)	0.091* (0.046)
Biennial session frequency	-0.079*** (0.015)	-0.087*** (0.016)
Staff Controls	No	Yes
Observations	3,282,096	3,282,096
R-squared	0.021	0.025
Mean dep var	0.089	0.089

Notes: This table analyzes the effect of legislative salary on missed roll call votes using OLS. Standard errors clustered by state in parentheses. An observation is a roll call vote for one legislator in 1999-2000. The roll call vote data do not indicate whether the vote took place in 1999 or 2000; the salary date is from 2000, and is merged to all observations from a corresponding state. All columns include census region (South, Northeast, Midwest, and West) fixed effects. Column 2 also includes controls for log GDP per capita. The “Staff Controls” are three dummy variables: whether a legislator has any personal staff, whether a legislator has any shared staff, and whether a legislator has any district staff.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Legislator Salary and Corruption

	(1)	(2)
Dep var:	Log Number of Convictions	Perceptions of Corruption
Log salary	-0.024 (0.218)	0.040 (0.151)
Observations	9255	14597
R-squared	0.95	0.301
Mean dep var	-2.740	0.063

Notes: OLS regressions with standard errors clustered by state in parentheses. The dependent variable in column 1 is the log number of convictions relative to the number of elected officials (Maxwell and Winters, 2005). The dependent variable in column 2 is an overall corruption scale, computed as the average of several normalized variables on perceived corruption levels from a survey of state house news reporters (Boylan and Long, 2003). Controls in both regressions are session length, a dummy for term limited seats, a dummy for seats with 4 year terms, a dummy for states with biennial session frequency, and measures of legislator staff. Column 1 also includes controls for the log of state population and the log of state GDP per capita. Corruption committed today may take time before it shows up in convictions or in perceptions of corruption. Thus, in column 1, five-year lagged salary is used. Likewise, in column 2, the sample is restricted to observations between 1994 and 1999 (five years before the corruption survey).

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Politician Salary and (Normalized) Time Use

Panel A: Time on Legislative Activities				
Dep var:	(1)	(2)	(3)	(4)
	Studying proposed legislation	Developing new legislation	Building coalitions within party	Building coalitions across parties
Total salary (in tens of thousands of dollars)	-0.025 (0.018)	0.034 (0.021)	-0.010 (0.016)	-0.038*** (0.014)
Session length in election year (in hundreds of days)	-0.008 (0.049)	-0.126** (0.047)	0.028 (0.050)	-0.107*** (0.037)
Observations	2,869	2,859	2,832	2,844
R-squared	0.019	0.053	0.003	0.018
Panel B: Time on Constituent Services				
Dep var:	(5)	(6)	(7)	
	Keeping in touch with constituents	Helping constituents with problems	Making sure the district gets a fair share	
Total salary (in tens of thousands of dollars)	0.043* (0.024)	0.046* (0.025)	0.048** (0.022)	
Session length in election year (in hundreds of days)	0.121* (0.064)	0.079 (0.066)	0.085 (0.065)	
Observations	2,854	2,859	2,867	
R-squared	0.093	0.103	0.104	
Panel C: Time on Fundraising				
Dep var:	(8)	(9)	(10)	
	Campaigning and fundraising	Fundraising for self	Fundraising for caucus	
Total salary (in tens of thousands of dollars)	0.054*** (0.019)	0.065*** (0.021)	0.028 (0.026)	
Session length in election year (in hundreds of days)	-0.013 (0.042)	0.023 (0.051)	-0.082 (0.073)	
Observations	2,857	2,799	2,735	
R-squared	0.087	0.103	0.034	

Notes: OLS regressions with standard errors clustered by state in parentheses. The dependent variable in each regression is a measure from 1 to 5 of the amount of time a politician spends with each activity. We normalize these variables so the regression coefficients can be interpreted in terms of standard deviations. The question was "How much time do you actually spend on each of the following activities?" with 1=Hardly Any to 5=Great Deal. Data is from the survey of Carey et al. (2002). All regressions also include the same set of controls in column 1 of Table 7.

* significant at 10%; ** significant at 5%; *** significant at 1%

Web Appendix: For Online Publication Only

This Appendix consists of four parts. Appendix A provides additional discussion on potential identification concerns, on standard errors for the border discontinuity analysis, and on how our results may extrapolate for thinking about policy. Appendix B gives more details on the data. Appendix C presents a formal model accompanying our conceptual framework. Appendix D provides additional results and robustness checks.

Appendix A Additional Discussion

A.1 Additional Discussion on Endogeneity Concerns

In the main text, we discuss a number of potential endogeneity concerns, focusing on the ones that we believe to be of greatest concern. For brevity, we do not attempt to address all possible concerns, even though there are others which may be important.

One additional concern is that politicians could be more or less willing to have their salaries raised depending on public perception of government (which could also correlate with whether people are interested in running for office, or how easy it is to pass bills). For example, if public perception is very negative, it could be that politicians may be reluctant to have their salaries increased. Further, only certain types of people may be willing to run in toxic political climates. By including year fixed effects, we address national shocks to public perception of government (e.g., the Watergate scandal in the 1970s), but there may be state-level shocks (e.g., a state-level scandal). One way we test for this possibility is by looking at whether legislator salary is less likely to increase after periods of high corruption levels within states, where corruption is measured by the number of convictions per 1,000 state government officials. It is likely that public perceptions about state governments are lower than average after an unusually high number of state official convictions. To examine this, for each state, we identify the year with the greatest number of corruption convictions. Then, we create a dummy for the four years following that year, identifying a period of time in each state where corruption convictions may be most salient in the public mind. We regress salary on this dummy, controlling for state and year fixed effects, state population and GDP, and legislature characteristics. We find no evidence that salary is more or less likely to change during periods following a high number of corruption convictions.¹

Assuming that the public takes a more negative view of government after state officials are convicted of corruption, this provides some evidence that negative changes in public perception do not seem to inhibit salary increases. We acknowledge, however, that such a concern could still be relevant for our results.

A.2 Standard Errors for Spatial Discontinuity Analysis

In the spatial discontinuity analysis, districts that belong to multiple pairings will be included multiple times. To address any impacts of this on standard errors, one approach is to use multi-way clustering (Miller et al., 2009). We experimented with several different methods of clustering, including two-way clustering by state and by electoral district pair fixed effects, and two-way clustering by state and by electoral district pair times year fixed effects (similar to Naidu (2009), who uses two-way clustering both across states and county pair time year fixed effects). We found similar standard

¹In case memory of corruption convictions is more fleeting, we also looked at two years following convictions, and obtained similar findings.

errors across the different methods. However, because the two-way clustering was sometimes highly computationally taxing, we present results clustering by state.

A.3 Policy Extrapolation

While our paper shows that within-state variation in politician salary appears to have only a modest impact on selection and performance, what can our paper tell us about a policy, say, of permanently increasing salary for US state politicians? Two important issues are, (1) to what extent do politicians regard historical increases in their salary as permanent, and (2) can we extrapolate our linear estimates to salary levels observed outside our sample. (Both these issues arise in many reduced-form panel estimation papers which attempt to use variation over time to inform the impact of policies.) On (1), nominal salaries usually go up, so it is likely that salary increases will not later be reversed. On (2), it is difficult to say what would happen if Alabama increased legislator salary to, say, \$150,000 per year, because Alabama is never observed paying near that level. Structural studies simulating larger salary changes (Diermeier et al., 2005; Keane and Merlo, 2010) have tended to confirm our reduced-form result that increasing salary would have a modest impact.² Thus, while it is important to note the limitations of our analysis to extrapolate on changes outside observed salary variation, we believe our estimates are still generally informative for policy discussions.

Appendix B Data Appendix

In this section, we explain the sources for different variables, as well as provide variable definitions for the variables in our regressions.

B.1 Politician Salaries

Data on politician salaries was collected from the *Book of the States*. For legislators, we create a single annual salary figure for each state by annualizing the relevant time frame. Salaries that are given for biennial legislative sessions are divided by two to obtain an annual salary figure. Daily salaries are multiplied by the average number of days a legislature is in session over time. Governors are paid annual salaries. As discussed in the main text, following Di Tella and Fisman (2004), we focus on base salary excluding non-salary benefits.

Data on non-salary benefits are included in the different editions of the *Book of the States*. However, these editions reveal that it would be difficult to construct a standardized measure across states. States seem to compensate non-salary expenses in different ways. Some states provide legislators with a fixed non-salary payment per time period, whereas other states compensate actual expenses (which we do not observe), whereas still other states compensate actual expenses plus a fixed non-salary payment per time period. Moreover, some states vary non-salary compensation with geographic features of a district or with whether the legislator is a legislative leader.

Salary is analyzed in real dollars. For state legislators, we use 2009 dollars. For governors, we use 1982 dollars. When we use the term “Total Salary,” we are referring to salary in un-logged form.

State level CPIs. State level CPIs are calculated using a weighted sum of food, apparel, transportation, medical, educational, recreational, and other prices in states. Weights capture average consumer income allocation, and are consistent with Moretti (2013), who constructs price indices at the city (MSA) level. We have data on the prices necessary to calculate the state CPIs for 13

²It is important to note that the counterfactual simulations conducted in Diermeier et al. (2005) and Keane and Merlo (2010) are for US congresspeople, not state-level politicians.

years between 1980 and 2001. For years missing within that period, state CPIs are filled in using linear interpolation. For years before 1980 and after 2001, we use the national CPI.

B.2 Border Districts

We identify whether two legislative districts are bordering one another according to the 2006 cartographic boundary files from the US census. These are available at https://www.census.gov/geo/maps-data/data/cbf/cbf_sld.html. Extracting the list of bordering districts, we merge this information with the election data from ICPSR 21480. We merge according to an identifier created by combining the state FIPS code with the district ID.

Our approach guarantees that the legislative districts were bordering one another in 2006, but it is possible that the districts were not bordering one another in the past, depending on how the various legislative districts have evolved over time as a result of legislative redistricting.³ We believe that such districts are likely to have been close to one another in the past, even if they are not exactly bordering in a particular year; thus, we hypothesize that this should not be a major source of bias for our border district pairs analysis. As a robustness check, we re-did the analysis in Panel A of Table 3 restricting the sample to states where the number of legislative seats did not change during our data period.⁴ Reassuringly, results from this are qualitatively similar.⁵

B.3 Election Results.

Data on state legislature elections comes from ICPSR 21480, containing almost all state legislature general elections from 1967 to 2003. It includes information on the number of candidates, the margin of victory, and office term length for each contested office. (Term length is believed to be a potentially important determinant of political performance; for a recent exploration of the effect of term length on politician productivity in Argentina, see Dal Bo and Rossi (2010).) Data on gubernatorial primaries are from Ansolabehere et al. (2007) and were kindly provided by James Snyder.

Contested Election. A dummy for whether or not an election is contested.

Number of Candidates. A measure of the number of candidates running in an election. In our analysis, we restrict the sample to those elections with six or fewer candidates.

Margin of Victory. This measures how much the elected politician won the vote by. It is equal to the election winner's votes minus second the place candidate's votes divided by the total votes cast.

Incumbent Re-elected. A dummy for whether or not the incumbent is re-elected.

Democrat Wins. A dummy for whether or not the elected politician is a Democrat. This variable is equal to zero if the politician is a Republican, and Independent, or a member of another party.

Election for seat with four year term length. A dummy for whether or not an election is for a seat with a four year term limit.

³As of March 2015, the oldest available cartographic boundary files on the census website are from 2000, thereby precluding us from creating separate maps over time for our data period to measure which districts were bordering one another.

⁴We identify whether the number of legislative seats changed using data kindly provided by Ernesto Dal Bo on legislative composition from 1967 to 1996. We had a research assistant call state legislatures to identify whether additional changes had been made between 1996 and 2003.

⁵One minor difference is the coefficient on "Democrat wins" drops from 0.08 to 0.028 (std error = 0.027), but we cannot statistically reject that the two coefficients are the same.

Democratic Primary A dummy for whether or not a Gubernatorial election is a Democratic primary. This variable is equal to zero if the election is a Republican primary.

B.4 Term Limits.

Obtained from the website of the Council of State Governments for state legislators and from List and Sturm (2006) for governors.

Election for Term-Limited Seat. A dummy for whether or not an election is for a term-limited seat.

B.5 Other Legislature Characteristics.

We classify legislatures as part-time or full-time based on the classification of the National Council of State Legislatures. We collected data on legislator staff and legislative session frequency from the Council of State Governments.

Any Personal Staff A dummy for whether a legislator has any staff working directly for him/her.

Any Shared Staff A dummy for whether a legislator has any shared staff.

Any District Staff A dummy for whether a legislator has access to any legislative district staff.

Biennial Session Frequency A dummy for whether or not legislative sessions occur biennially. This variable is equal to zero if sessions occur annually.

B.6 Politician Characteristics.

For state legislators, information on characteristics were kindly provided by Kathleen Bratton. The data are an extension of characteristics in Bratton and Haynie (1999). The data contain information on lower-house legislators in 11 states: Arizona, Arkansas, California, Connecticut, Florida, Illinois, Nevada, Ohio, Texas, South Carolina, and North Carolina. The legislator characteristics data go from 1967 - 2003.

For governors and candidates in gubernatorial primaries, we collected our own using using the biographies on the website of the National Governor's Association and Google Searches. This information was given in paragraph form and coded into attributes by hand. The attributes of interest including gender, age, having a college degree, having a law degree, and having military experience were chosen (1) because they were straightforward to measure and (2) because they were similar to the variables provided by Bratton. Gender was coded by use of gender-specific pronouns in the biography (e.g. "he", "wife", and "her"). Age was coded up using the information on the governor's date of birth, and was defined as the governor's age upon entering office. Information on having a college degree or law degree was coded up based on the mention of this information in the biographical paragraph. For governors, characteristics data are from 1950 - 1990.

Female. A dummy for whether or not a candidate is a female.

Black. A dummy for whether or not a candidate is black.

Latino. A dummy for whether or not a candidate is of Latin American origin.

Age. Candidate age.

College Degree. A dummy for whether or not a candidate has a college degree.

Lawyer. A dummy for whether or not a candidate has a law degree.

Has Political Experience. A dummy for whether or not a candidate has political experience prior to the focal election.

B.7 Bill-passing.

Information on legislative productivity, that is, the number of bills proposed and passed was hand-collected from the *The Book of the States*. Information here is given in terms of the number of bills during regular sessions and during special sessions (there is no division by upper and lower house). Most of our analysis focuses on using the data on bill introduction during regular sessions.

Number of Bills Introduced. A count of the number of bills introduced by the legislature.

Number of Bills Approved. A count of the number of bills approved by the legislature.

Share of Bills Approved. A count of the number of bills introduced by the legislature divided by a count of the number of bills approved by the legislature.

B.8 Roll call voting.

Roll call voting data were obtained from Wright (2004). The data contain roll-call voting records in state legislatures during the years 1999 and 2000. Our outcome measure is whether a legislator was present for a vote. Only “competitive” roll call votes are included in the data from Wright (2004). That is, votes with under 5% of legislators voting in the minority are not part of the data.

Missed Roll Call Vote A dummy for whether or not a legislator missed a given roll call vote.

B.9 Politician Corruption.

We use two measures of politician corruption: the number of convictions per 1,000 state government officials Maxwell and Winters (2005), and a survey measure of the level of perceived corruption in state government developed by Boylan and Long (2003).

Number of Convictions. The number of convictions of state government officials in corruption cases per 1,000 state government officials. In correspondence with TRAC (trac.syr.edu), we learned that corruption convictions for state legislators had not been collected separately from overall corruption convictions among state government officials.

Perceptions of Corruption. A survey measure of State House new reporters’ perceptions of state public officials’ corruption level from Boylan and Long (2003).

B.10 State Fiscal Policy.

We also use state fiscal policy to measure the quality of state politicians. We collected data on state GDP, expenditures, and taxes from U.S. Census Bureau’s Annual Survey of Government Series.

Income per Capita. State income per capita.

Expenditure per Capita. State government expenditures per capita.

Taxes per capita Total state taxes per capita. This includes income, sales, and corporate taxes.

B.11 Politician Surveys.

Information on politician outside labor supply, family income, and time use was collected from the survey of legislators done by Carey et al. (1995) and Carey et al. (2002). Surveys were conducted anonymously through the mail. The 1995 survey was mailed to every upper house legislator and 77 percent of lower house legislators. In addition, it was mailed to all former legislators who last served in 1993 or 1994. For the 1995 survey, there were 3,542 legislators who responded and the response rate was 47 percent. For the 2002 survey, there were 2,982 state legislators who responded and the response rate was 40.1 percent.

Family Income. Reported family income in US\$ across 5 categories. We assign the category of \$0-\$25,000 to \$25,000, the category of \$25,000-\$50,000 to \$37,500, category of \$50,000-\$75,000 to \$62,500, the category of \$75,000-\$250,000 to \$175,000, and the category of \$250,000+ to \$250,000.

Has Outside Job. A dummy for whether or not a legislator has an outside job while serving as a legislator.

Studying Proposed Legislation The amount of time on a scale from one to five a legislator reports to spend studying proposed legislation.

Studying Proposed Legislation. The amount of time on a scale from one to five a legislator reports to spend studying proposed legislation.

Developing New Legislation. The amount of time on a scale from one to five a legislator reports to spend developing new legislation.

Building Coalitions Within Parties. The amount of time on a scale from one to five a legislator reports to spend building coalitions within his/her own party to develop legislation.

Building Coalitions Across Parties. The amount of time on a scale from one to five a legislator reports to spend building coalitions across parties to develop legislation.

Keeping in Touch with Constituents. The amount of time on a scale from one to five a legislator reports to spend keeping in touch with constituents.

Helping Constituents with Problems. The amount of time on a scale from one to five a legislator reports to spend helping constituents with problems related to government. This variable is not included in the 1995 survey.

Making Sure the District Gets a Fair Share. The amount of time on a scale from one to five a legislator reports to spend making sure his/her district gets a fair share of government money and projects. This variable is not included in the 1995 survey.

Campaigning and Fundraising. The total amount of time on a scale from one to five a legislator reports to spend campaigning and/or fundraising.

Fundraising for Self. The amount of time on a scale from one to five a legislator reports to spend fundraising for him/her self. This variable is not included in the 1995 survey.

Fundraising for Caucus. The amount of time on a scale from one to five a legislator reports to spend fundraising for caucus. This variable is not included in the 1995 survey.

B.12 Party Strength.

We measure political party strength using the Mayhew (1986) measure of Total Party Organization which captures the degree to which local politics are run by organized party machines. Each state is scored from 1 to 5 on the Total Party Organization scale.

Strong Party. A dummy for whether or not a state has strong political parties. A strong party state has a Total Party Organization score of 4 or 5.

B.13 Narrative Analysis

In this section, we describe how we assembled the sample for our narrative analysis. We attempted to create a list of potential salary increases whose passage or failure was determined by forces which are plausibly exogenous for our analysis. The list was assembled by a research assistant.

We began with an extensive systematic search of historical newspapers. We used search terms such as “legislative salary increase” and “legislative compensation commission.” We conducted our searches on ProQuest’s historical newspaper database and the Google newspaper archive (<http://news.google.com/newspapers>). Most searches were done on Google newspaper archive because it has a wide range of local newspapers, whereas ProQuest is more concentrated in national newspapers.

We began our searches aiming at large absolute salary changes that were observed in our data.⁶ For example, we would start the Google newspaper archive searches with “california legislative salary increase 1998.” We looked for information about the nature of the decision, such as whether the commission vote was contested, or if the executive or different branches of the legislature were at odds with one another about the decision.

After doing searches related to large salary changes, we broadened the search within particular states. We looked for other salary changes within that state (e.g., “california legislative salary increase”), as well as proposed and rejected salary changes (e.g., “california legislators salary rejected”). We looked for the same sort of information about the nature of the decision.

We emphasize that this list is non-exhaustive. It should thought more of as a list of changes (or potential changes) that were covered in newspapers or that we were able to find information for in some other manner (e.g., because someone from a state compensation commission was willing to scan and send us minutes from past meetings of the compensation board). Although the list is surely not all non-assured salary changes, we believe that it represents some of the most important changes or non-changes, including ones that would be salient for politicians or potential candidates.⁷

- **Florida, 1970.** Increase from \$1,000/year to \$12,000/year. Passed by the legislature. The governor tried to veto, but a judge eventually ordered him to approve it because his veto was unconstitutional.
- **Rhode Island, 1973.** Failed proposed increase from \$300/year to \$2,000/year. “Narrowly defeated” by the electorate, taken directly from the article: “In Rhode Island, a proposal to lift legislative salaries from \$300 a year to \$2,000 by 1975 was narrowly defeated”.
- **Texas, 1973.** Failed proposed increase from \$4,800 to \$15,000. Rejected by the electorate, with the proposed increase gaining 43.3% of the vote.
- **Washington, 1973.** Failed proposed large increase of 193%; enacted instead a small increase of 5.5%. The proposed increase was passed by the legislature. However, a Seattle furniture salesman initiated a ballot measure to lower the proposed raise to only 5.5%. The ballot measured was passed by the electorate.

⁶We began with large changes because they seem to be more widely covered in the news, and news coverage of salary changes several decades back was often incomplete.

⁷As an example of what our list does not cover, many small proposed salary changes which passed or did not pass may not be in our sample, as many of them are not covered in newspapers.

- **Washington, 1976.** Increase from \$3,800 to \$7,200. Passed by the House on the relatively narrow margin of 53 to 42. (After passing the House, the bill was sent to the Senate, but we could not locate the Senate vote total.)
- **Arizona, 1978.** Failed proposed increase from \$6,000/year to \$9,200/year. Rejected by the electorate, 54.6% to 46.4.⁸
- **Arizona, 1980.** Increase from \$6,000/year to \$15,000/year. Passed by the electorate, 55.1% to 44.9%.
- **Pennsylvania, 1982.** Increase of \$10,000/year. Passed by the legislature, but controversial. According to one account, the bill drew heavy criticism from legislators and public interest groups, but passed nonetheless. It was not possible to locate a record of the vote tally.
- **California, 1990.** Increase of \$13,282/year. The increase was instituted by a salary commission.⁹
- **Alabama, 1991.** Failed proposed salary increase of 30%. Passed by the House, but rejected by the Senate.¹⁰
- **Idaho, 1991.** \$5,400/year increase. While it was passed by the legislature, the GOP caucus leader noted that his caucus was divided on the subject.
- **Michigan, 1993.** Increase from \$45,450 to \$47,722.50. Passed by the state compensation commission, but nearly overturned by the State House.¹¹
- **Arizona, 1994.** Failed proposed increase from \$15,000/year to \$19,750/year. Rejected by the electorate, 60.47% to 39.53%.
- **California, 1994.** Increase of \$22,449/year. Passed by the compensation commission in a divided vote of 5 to 2.¹²
- **Arizona, 1998.** Increase from \$15,000/year to \$24,000/year. Passed by the electorate, 56.6% to 43.4%.
- **California, 1998.** Increase of \$23,837/year. Passed by the compensation commission in a divided vote of 4 to 3.¹³
- **New Jersey, 1999.** Increase from \$35,000/year to \$49,000/year. Passed by the House, 48 to 24 and by the Senate, 21 to 14.

⁸In Arizona, the state compensation commission does not have a final say. Instead, the compensation commission proposes the salary change; the proposed salary changes goes to a public ballot and is passed by a simple majority.

⁹No record of a vote by the salary commission could be located. In 1990, California voters approved proposition 112, which included a series of ethical reforms proposed by the legislature. Reforms included giving up honorariums for making speeches to special interest groups, and placing a \$250 limit on gifts from most sources. Prop 112 also created the compensation commission, which set new legislator salaries in 1990. Normal legislators were granted a 28.5% increase from \$40,816 to \$52,500 yearly “partially to help make up for the loss of speaking fees” (Gillam, 1993, May 10).

¹⁰The House voted for a 30% raise but the senate refused to even consider it and instead voted 19-16 for a separate bill to create a compensation commission. The House then passed the senate proposed bill.

¹¹The House tried to overturn the state compensation commission, but missed the two-thirds threshold by 3 votes.

¹²The large California legislator salary increase appearing in our data in year 1996 resulted from a proposal made in 1994 by the California Citizens Compensation Committee.

¹³The increase that appears in our data for the year 2000 was the result of a Citizens Compensation Commission decision in 1998, in which the group decided in a 4-to-3 vote to increase legislator salaries by 25.9%.

- **Michigan, 2000.** Increase from \$55,054 to \$77,400. Passed by the Compensation Commission despite disagreement in the state legislature.

Appendix C Formal Model

We develop a very simple model to analyze how politician wages affect the number of candidates running for an office; the quality of candidates and politicians; and politician productivity and public good provision, accompanying our conceptual framework in Section 3. The purpose of this model is to lay out a framework for interpreting our comparative statics rather than to provide a realistic account for how salary impacts performance. The basic idea is that when politicians are paid more, they face a greater incentive to work hard once in office in order to keep their seat. In addition, the greater reward from being in office leads to both higher quality people wishing to run, as well as more candidates.

Politicians are risk-neutral citizen-candidates endowed with quality θ , where θ is distributed across the population with distribution function $F(\cdot)$. The size of the population is set to unity. We denote the measure of candidates who chose to run by N . Politicians receive a salary of w per period in office, whereas working in the private sector leads to a salary of $r(\theta)$ per period. We assume that $r'(\theta) > 0$, that is, higher ability people have higher outside options. In addition to their salary, politicians also receive a non-pecuniary benefit, z , from serving in office. While in office, politicians choose a costly effort (productivity) level e , with a convex cost function $c(\cdot)$. Effort and quality lead to public goods G_t , with $G_t = G_t(e_t)$, $G' > 0$. Politicians can serve for two periods so those who are elected in the first period can be re-elected for the second period but cannot run again after the second period. Politicians are re-elected with probability π , which is increasing, but concave with respect to public goods ($\pi' > 0$ and $\pi'' < 0$). That is, we assume that voters vote retrospectively. Running for office is assumed to be costless.

For simplicity, we will assume that $G(e_t) = e_t$. (The analysis is similar with a more general public good production, where public goods are weakly concave in e , i.e. $G''(e) \leq 0$.)

Many models of politician salary analyze an infinite period economy (Besley, 2004; Ferraz and Finan, 2010). For our purposes, however, we are merely using the model to illustrate the main economic forces. Thus, we assume a 2-period economy with no discounting. We consider the case of an office that has been newly created with no incumbent going into the first period. Thus, at the start of the first period, one candidate is randomly selected to serve among those who choose to run. At the end of the first period, if the incumbent loses, a new politician is randomly selected from among the challengers.

We deal with two cases. First, we analyze elections where there are no political parties. In the second case, we add political parties.

C.1 No Political Parties

In the second period, a politician will exert 0 effort because there is no incentive to get re-elected. Conditional on having exert e_1 in the first period, the politician's expected utility from running is $\pi(G_1(e_1))(w + z) + (1 - \pi(G_1(e_1)))r(\theta)$, whereas the utility from not running is $r(\theta)$; thus, the incumbent politician runs if $w + z > r(\theta)$. Challenger politicians will run for office if $w + z > r(\theta)$, which occurs when $\theta < \theta^* \equiv r^{-1}(w + z)$. By inspection, the share of candidates running, $N = F(\theta^*)$ is increasing in politician salary. In addition, since only citizens below a certain quality threshold choose to run, increasing the quantity of candidates also increases the average quality.

The incumbent politician optimally chooses first period effort trading off the benefit from being re-elected against the cost of effort, leading to the first order condition: $M = \pi'(e_1)(w + z - r(\theta)) -$

$c'(e_1) = 0$. To examine the incumbent politician's effort changes with respect to wage, we apply the implicit function theorem:

$$\frac{de_1}{dw} = -\frac{\frac{\partial M}{\partial w}}{\frac{\partial M}{\partial e_1}} = \frac{-\pi'}{\pi''(e_1)(w+z-r(\theta)) - c''(e_1)} = \frac{\pi'}{c''(e_1) - \pi''(e_1)(w+z-r(\theta))}$$

Thus, we have that $\text{sgn}\left(\frac{de_1}{dw}\right) = \text{sgn}(c''(e_1) - \pi''(e_1)(w+z-r(\theta)))$. Since the politician will only choose to run if $w+z > r(\theta)$, we have that $c''(e_1) - \pi''(e_1)(w+z-r(\theta)) > 0$, conditional on the politician intending to run. Given $c'' > 0$ and $\pi'' < 0$, we have thus shown that $\frac{de_1}{dw} > 0$ for first period politicians who intend to run in the second period.

In our highly stylized model, since challengers are selected randomly from among those who choose to run, increasing the wage will unambiguously increase the chance the incumbent politician is re-elected. However, if having more candidates decreases the probability that the incumbent is re-elected, than the impact of the wage on the probability of re-election is ambiguous, as in the simulation of the infinite period model in Ferraz and Finan.

A politician is willing to serve in office in the first period if $\max_{e_1}[w+z-c(e_1)+\pi(e_1)(w+z)+(1-\pi(e_1))r(\theta)] > 2r(\theta)$. If $w+z < r(\theta)$, it is not worthwhile for the citizen in the first period to enter politics. If $w+z > r(\theta)$, he can always obtain at least $w+z+r(\theta)$, the return from exerting 0 effort, and therefore he prefers to run. Thus, the same people will choose to run in the first period election are the same people who choose to run in the second period election. Hence, we have that $\frac{de_1}{dw} > 0$ for all politicians who run for office at time 0. To summarize, we have proven the following the result:

Result: An increase in politician salary (1) Increases the number of candidates running for both open and non-open seats; (2) Increases the quality of politicians running for both open and non-open seats; and (3) Increases the amount of effort politicians provide and increases the quantity of public goods provided.

We now consider the impact of political parties on the relationship between politician salary and outcomes.

C.2 Political Party Strength as Barrier to Entry

In the empirical results, we show that there the relationship between wage and the number of candidates is very small. One possibility, that we now illustrate, is that in states with strong political parties, entry is decided by the party. As a result, potential candidates are less able to respond to salary increases.

When political parties are strong, a citizen who would like to run for office may not be able to simply enter a race. Rather, he must affiliate himself with a party and choose to fulfill certain responsibilities for the party. These may come before the party backs him to run for office or they may come after has assumed office. In either case, such party responsibilities may be viewed as a tax on the return from political office. That is, a certain share, τ , of the gains from office are "taxed" away by a political party. The stronger are political parties, the larger the presumed tax; thus, τ is a measure of the strength of a political party.

When there is a tax, a politician will run for office in the second period if $(1-\tau)(w+z) > r(\theta)$, or if $\theta < \theta^* \equiv r^{-1}((1-\tau)(w+z))$. Denoting $g(\cdot) = r^{-1}(\cdot)$, we have, similar as before, that $\frac{d\theta^*}{dw} = (1-\tau)g'$. To examine how the impact of salary on candidate quality varies with party strength, we would like to compute the cross partial derivative $\frac{d^2\theta^*}{dwd\tau}$. We get that $\frac{d^2\theta^*}{dwd\tau} = -g' - (1-\tau)(w+z)g''$. We have not made assumptions so far on the second derivatives of $r(\theta)$. However,

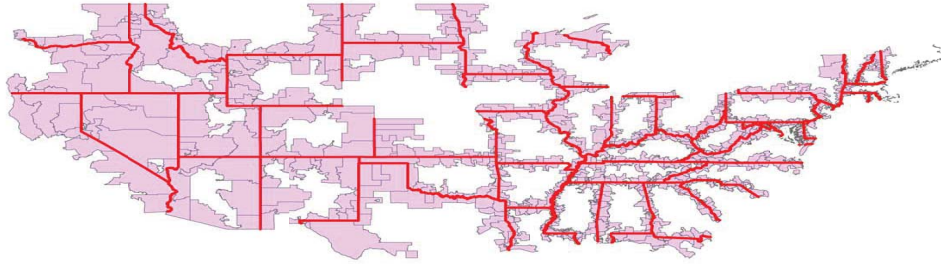
if $r''(\theta) < 0$, then $g''(\theta) > 0$, since $g''(\theta) = -[r'(g(\theta))]^2 \cdot r''(g(\theta)) \cdot g'(\theta)$. Thus, if $r'' < 0$, we have that $\frac{d^2\theta^*}{dw d\tau} < 0$.

Likewise, increasing τ may dampen the impact of w on e_1 .

It is important to reiterate that our model is highly stylized and is simply intended to ground the empirical analysis, and not provide new theoretical insights. The contribution of the paper is the empirical analysis.

Appendix D Additional Results

Figure D1: Lower House State Legislative Districts Lying Along State Boundaries



Notes: The source is the 2006 US Census cartographic boundary file. Note that Nebraska has a unicameral legislature which we code as an upper house legislature. Thus, there are no lower house districts in Nebraska, nor are there any bordering districts from districts in other states that border Nebraska.

Figure D2: Average Salary of Legislators Over Time (Real Salary in 2009 Dollars)

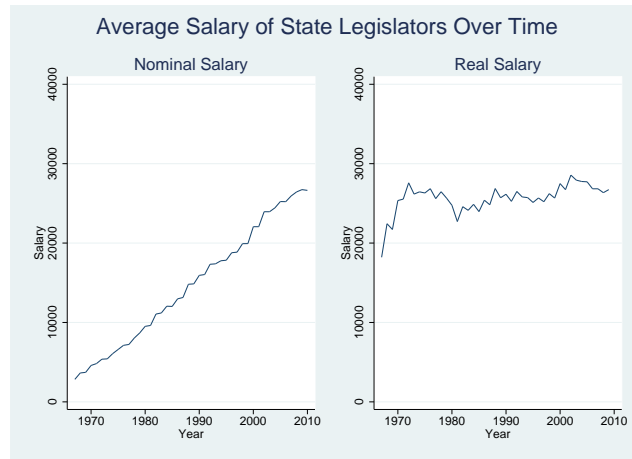


Figure D3: Legislator Pay Over Time By State, In Real Terms (2009 dollars)

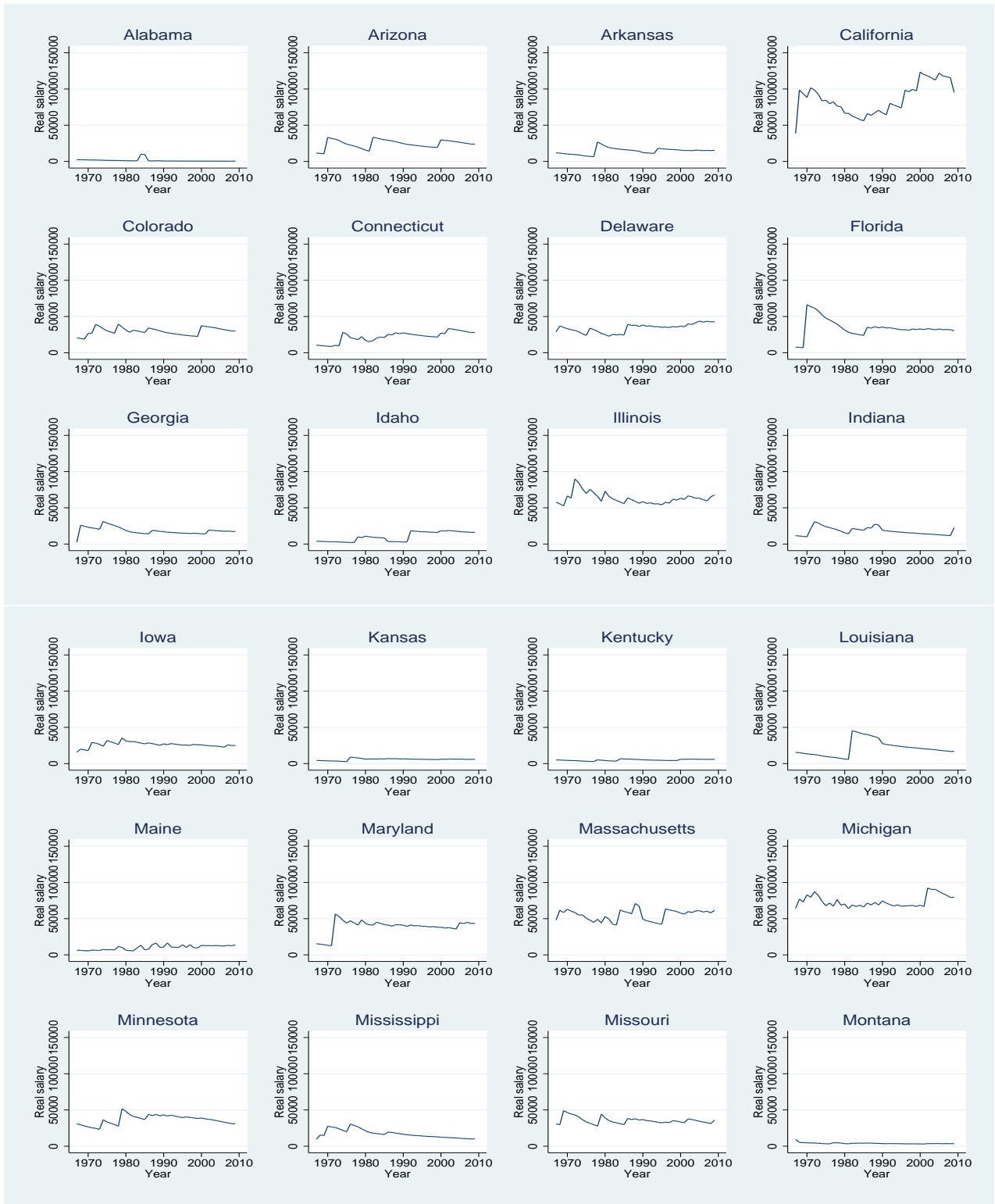


Figure D3: Legislator Pay Over Time By State, In Real Terms (Cont.)

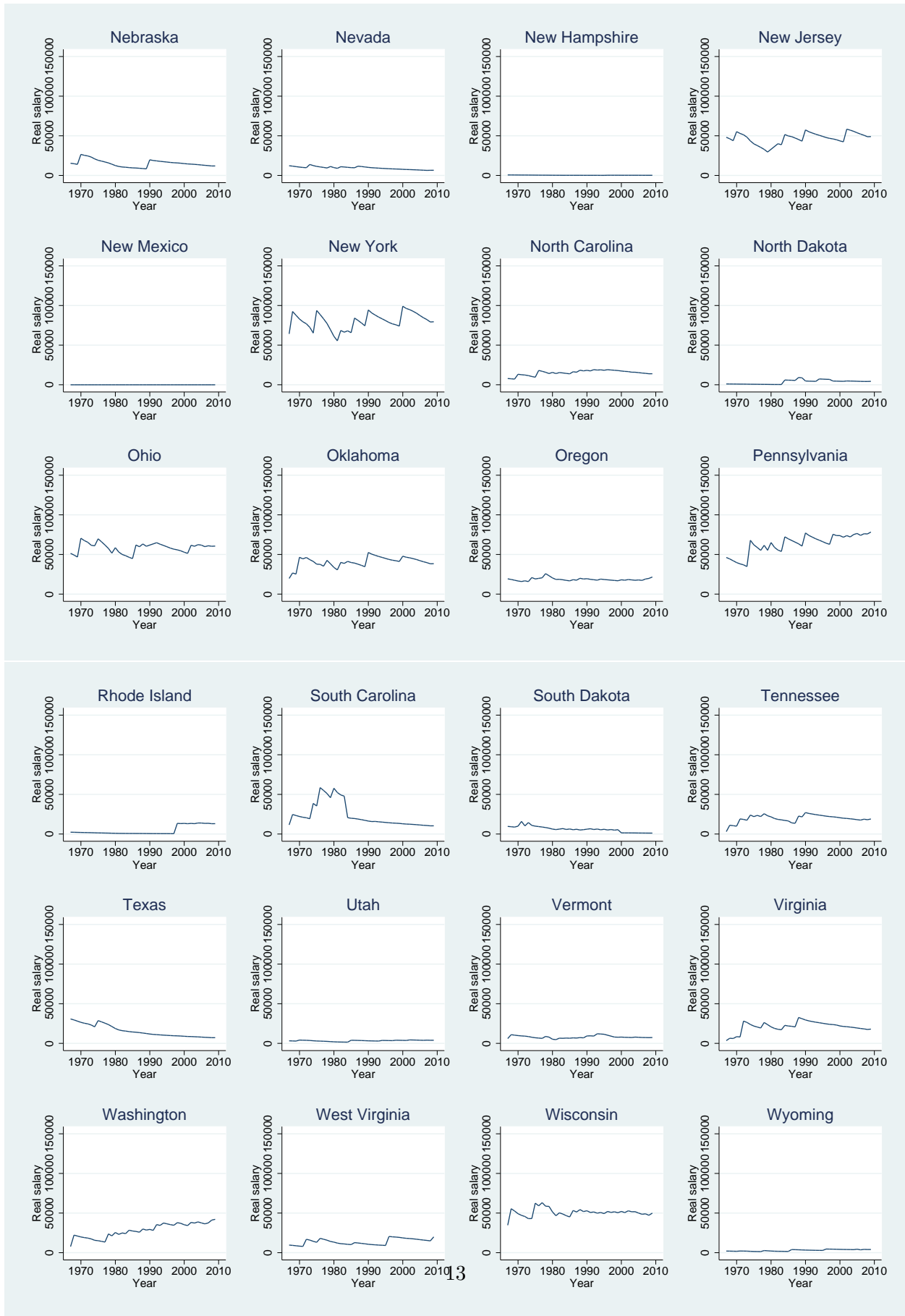


Figure D4: Legislative Productivity Over Time, Number Of Bills Introduced and Passed

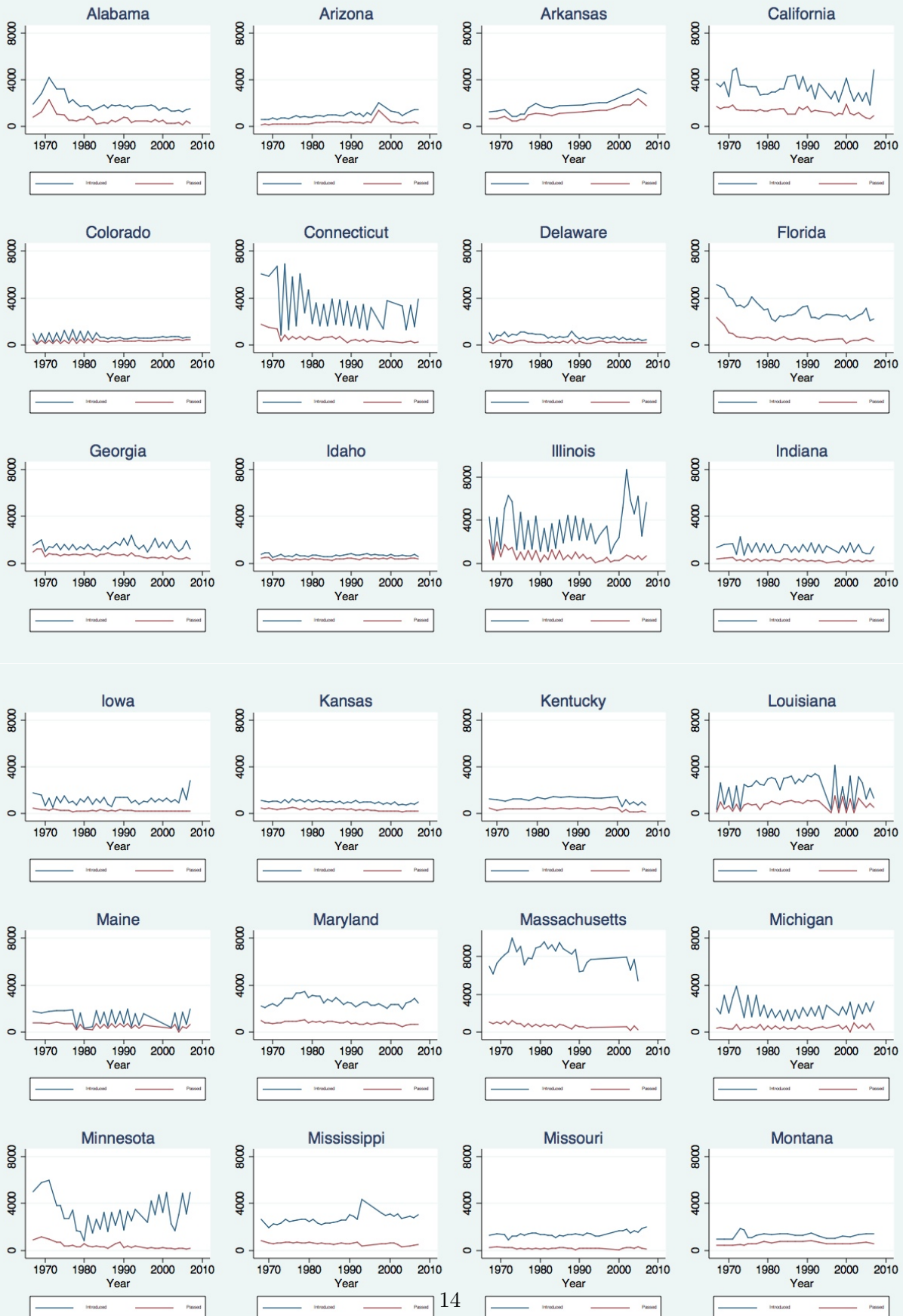


Figure D4: Legislative Productivity Over Time, Number Of Bills Introduced and Passed (Cont.)

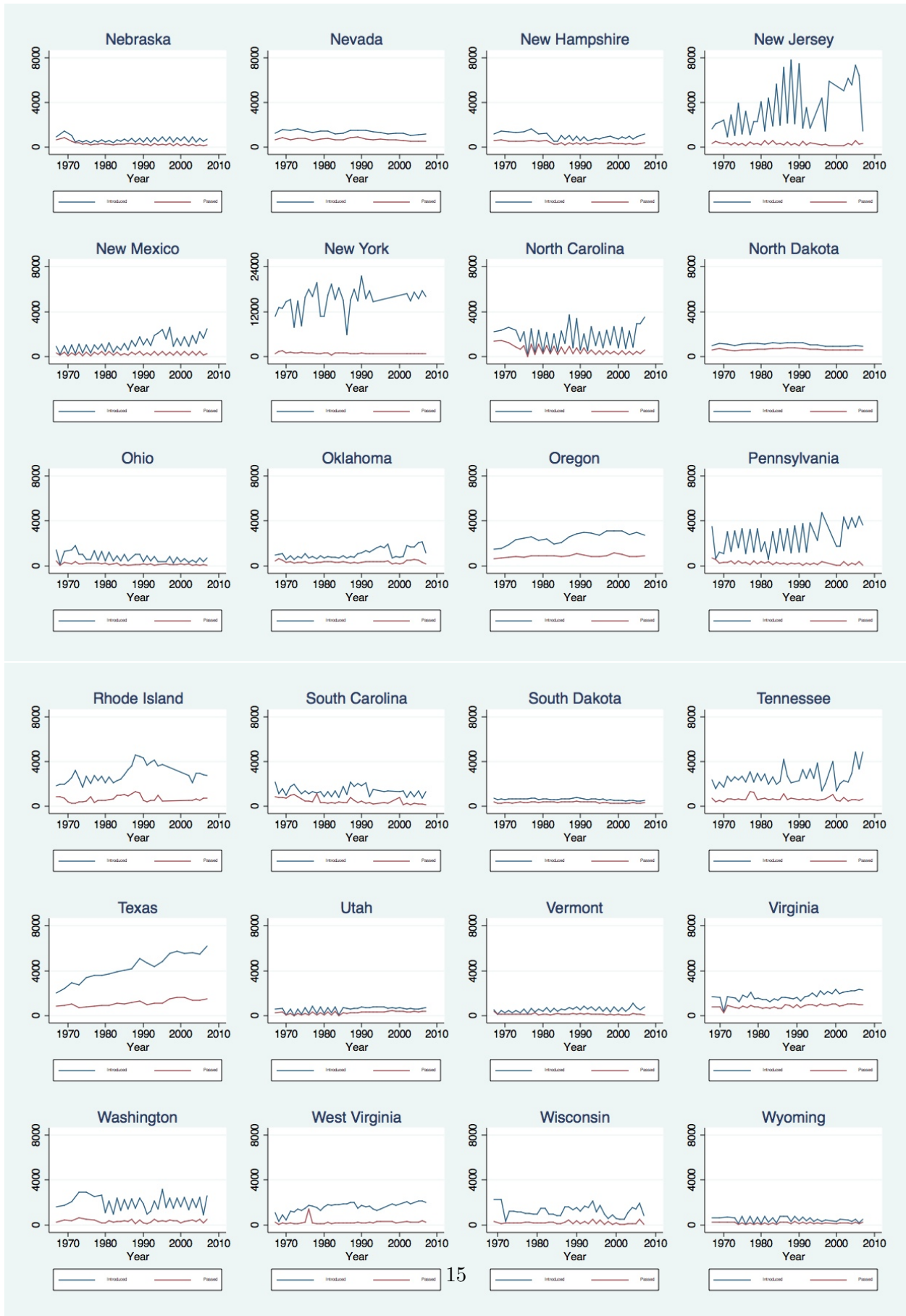


Figure D5: Governor Pay Over Time By State, In Real Terms (1982 dollars)

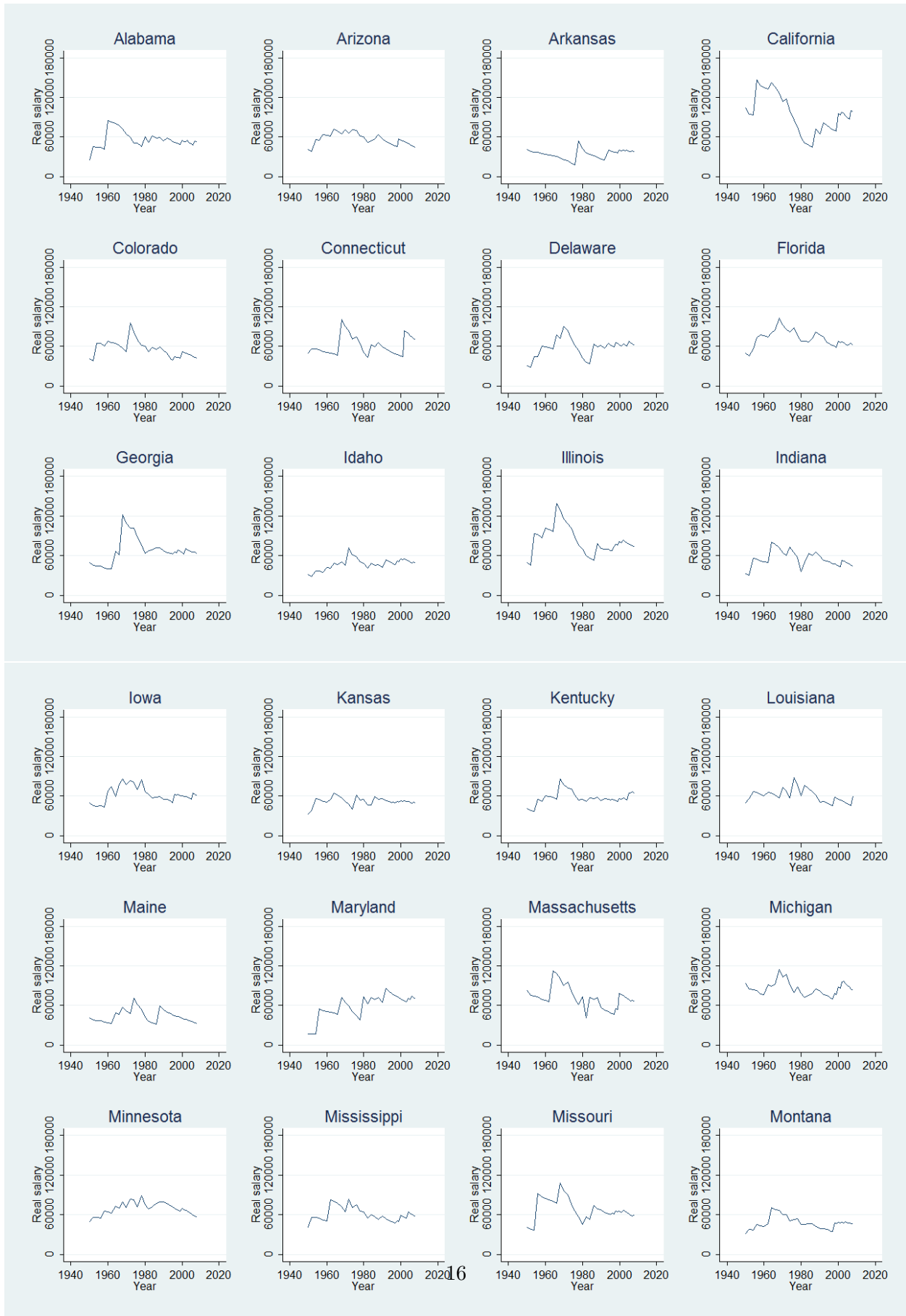


Figure D5: Governor Pay Over Time By State, In Real Terms (Cont.)



Table D1: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes By House

Panel A - Lower House Elections					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.039** (0.017)	0.070*** (0.026)	-1.041 (1.615)	-0.020 (0.016)	0.071** (0.033)
Session length in election year (in hundreds of days)	0.014 (0.017)	0.015 (0.032)	-0.076 (1.088)	0.013 (0.010)	0.024* (0.013)
Election for term-limited seat	0.088** (0.034)	0.131* (0.067)	-4.831 (2.965)	-0.143*** (0.033)	0.020 (0.035)
Election for seat with four year term length	-0.102* (0.053)	0.251** (0.114)	-9.551 (12.284)	-0.173*** (0.061)	0.407*** (0.149)
Observations	62,733	62,673	60,870	62,579	62,325
Mean dep var	0.718	1.904	48.38	0.706	0.596
Panel B - Upper House Elections					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.006 (0.011)	0.019 (0.019)	-0.388 (1.424)	-0.003 (0.012)	0.053*** (0.019)
Session length in election year (in hundreds of days)	-0.001 (0.014)	0.014 (0.029)	-1.039 (1.224)	0.004 (0.017)	0.008 (0.015)
Election for term-limited seat	-0.006 (0.037)	0.020 (0.060)	1.000 (3.480)	-0.133*** (0.033)	0.022 (0.039)
Election for seat with four year term length	-0.026 (0.026)	-0.022 (0.038)	0.837 (1.007)	0.005 (0.030)	0.023* (0.013)
Observations	19,800	19,774	19,378	19,774	19,702
Mean dep var	0.782	2.005	43.06	0.620	0.564

Notes: The regressions are the same as in Table 2 except we restrict the sample by lower house or upper house elections.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D2: The Effect of Governor Salary on Electoral Competitiveness and Electoral Outcomes, Gubernatorial Elections By Party

Panel A - Democratic Primaries				
	(1)	(2)	(3)	(4)
	Contested Election	Number of Candidates	Margin of Victory (in percentage points)	Margin of Victory for Contested elections (in percentage points)
Log salary	0.022 (0.043)	0.041 (0.346)	-4.052 (7.027)	-2.951 (7.162)
Election for term-limited seat	0.053 (0.055)	-0.077 (0.401)	-17.837** (7.429)	-15.948** (7.112)
Observations	537	545	452	440
Mean dep var	0.760	2.497	44.58	27.11
Panel B - Republican Primaries				
	(1)	(2)	(3)	(4)
	Contested Election	Number of Candidates	Margin of Victory (in percentage points)	Margin of Victory for Contested elections (in percentage points)
Log salary	0.044 (0.064)	0.103 (0.436)	20.456** (8.421)	17.136** (7.623)
Election for term-limited seat	0.044 (0.054)	-0.107 (0.410)	5.819 (9.260)	5.998 (9.435)
Observations	676	683	588	552
Mean dep var	0.678	2.076	54.05	32.24

Notes: The regressions are the same as in Table 4 except we restrict the sample by either Democratic primaries or Republican primaries.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D3: Political Parties, Family Income, and Prior Experience in Government

	(1)	(2)
Dep Var:	Family Income	Prior Office in Government
Republican	6,906.484*** (2,446.366)	-0.053*** (0.011)
Observations	3118	3410
Mean dep var	93,000	0.094

Notes: This table analyzes whether Republican candidates have different family incomes and prior experience in public office than Democratic candidates. The data is from the surveys of state legislators done by Carey et al. (1995) and Carey et al. (2002). The sample is restricted to Republican or Democratic candidates. State and year fixed effects are included. Standard errors clustered by state in parentheses. Controls for session length, the logarithm of population, and the logarithm of state GDP are included.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D4: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes with State-Specific Time Trends

	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.031*** (0.011)	0.024 (0.019)	-1.747** (0.737)	-0.007 (0.012)	0.020* (0.012)
Upper house	0.070*** (0.022)	0.114*** (0.031)	-3.666** (1.555)	-0.016 (0.011)	-0.015 (0.021)
Session length in election year (in hundreds of days)	0.012 (0.009)	0.029 (0.019)	0.304 (0.655)	0.007 (0.009)	0.000 (0.006)
Election for term-limited seat	0.040 (0.035)	0.055 (0.056)	-2.307 (1.943)	-0.130*** (0.029)	-0.012 (0.018)
Election for seat with four year term length	-0.029 (0.023)	-0.035 (0.033)	0.004 (1.710)	-0.111*** (0.016)	0.018 (0.025)
Observations	82,533	82,447	80,248	82,353	82,027
Mean dep var	0.734	1.928	47.10	0.685	0.588

Notes: Panel A here is similar to Table 2 except here we add state-specific time trends.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D5: The Effect of Legislative Salary on Legislative Productivity with State-Specific Year Trends

	Log(Number of Bills Introduce)		Log(Number of Bills Approved)		Share of Bills Approved	
	(1)	(2)	(3)	(4)	(5)	(6)
Log salary	0.010 (0.042)	-0.002 (0.040)	0.049 (0.040)	0.014 (0.043)	0.003 (0.007)	0.003 (0.007)
Session length in election year (in hundreds of days)		0.250* (0.127)		0.247* (0.126)		0.002 (0.010)
Election for term-limited seat	0.042 (0.059)	0.099 (0.077)	0.340*** (0.101)	0.121 (0.084)	-0.001 (0.017)	-0.001 (0.017)
Observations	1,467	1,463	1,466	1,462	1,467	1,463
Mean dep var	7.269	7.269	5.897	5.897	0.309	0.309

Notes: The regressions are the same as in Panel A of Table 6 except we add state-specific time trends.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D6: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes by Full-Time and Part-Time Legislatures

Panel A - Full-Time Legislatures					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	-0.010 (0.055)	-0.083 (0.149)	7.878* (3.972)	-0.052 (0.042)	0.002 (0.061)
Upper house	0.075 (0.067)	0.142 (0.097)	-0.739 (0.725)	0.000 (0.026)	-0.082 (0.055)
Session length in election year (in hundreds of days)	0.024* (0.012)	0.031 (0.029)	-1.858* (0.962)	-0.006 (0.007)	0.003 (0.008)
Election for term-limited seat	0.127 (0.075)	0.224* (0.107)	-8.172 (4.809)	-0.193*** (0.041)	-0.019 (0.028)
Election for seat with four year term length	-0.042 (0.065)	-0.034 (0.097)	-3.845*** (1.121)	-0.148*** (0.037)	0.052 (0.054)
Observations	23,079	23,078	22,466	23,015	22,970
Mean dep var	0.821	2.116	45.08	0.723	0.574
Panel B - Part-Time Legislatures					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.031** (0.012)	0.060*** (0.016)	-1.342 (1.109)	-0.014 (0.014)	0.068** (0.026)
Upper house	0.067*** (0.018)	0.095*** (0.030)	-4.513** (1.891)	-0.021** (0.009)	0.013 (0.013)
Session length in election year (in hundreds of days)	0.019 (0.045)	0.068 (0.087)	2.131 (3.022)	0.044 (0.036)	0.063 (0.061)
Election for term-limited seat	0.048 (0.035)	0.050 (0.089)	-2.125 (3.299)	-0.117*** (0.035)	0.033 (0.046)
Election for seat with four year term length	-0.018 (0.021)	-0.010 (0.035)	0.707 (2.106)	-0.099*** (0.016)	0.006 (0.023)
Observations	59,529	59,506	57,857	59,413	59,132
Mean dep var	0.700	1.862	47.88	0.670	0.594

Notes: The regressions are the same as in Table 2 except we split the sample by full-time or part-time legislatures.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D7: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes, Restricted to States that Do Not Allow Legislators to Choose Own Salaries

	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.030* (0.016)	0.056** (0.021)	-0.172 (1.434)	-0.021 (0.018)	0.062* (0.031)
Upper house	0.079*** (0.025)	0.126*** (0.029)	-6.261** (2.459)	-0.039*** (0.012)	0.003 (0.015)
Session length in election year (in hundreds of days)	0.024 (0.027)	0.075* (0.039)	-0.715 (2.102)	0.053** (0.024)	0.049 (0.044)
Election for term-limited seat	0.064 (0.046)	0.074 (0.069)	-5.066 (4.839)	-0.123** (0.048)	-0.017 (0.064)
Election for seat with four year term length	-0.040 (0.024)	-0.040 (0.033)	2.630 (2.400)	-0.083*** (0.025)	0.040 (0.031)
Observations	33,625	33,602	32,267	33,606	33,527
Mean dep var	0.734	1.933	47.04	0.685	0.590

Notes: Panel A here is similar to Table 2. The difference is that our sample excludes states that allow legislators to set their own salaries (i.e., we restrict to states with salaries set by a compensation board or by the constitution).

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D8: The Effect of Legislative Salary on Legislative Productivity, Restricted to States that Do Not Allow Legislators to Choose Own Salaries

	(1)	(2)	(3)	(4)
	Log(Number of Bills Introduced)	Log(Number of Bills Approved)	Share of Bills Approved	Income to Corporate Tax Ratio
Log salary	0.022 (0.048)	0.020 (0.075)	-0.002 (0.016)	0.281 (0.793)
Session length in election year (in hundreds of days)	0.195 (0.140)	0.216 (0.143)	0.013 (0.014)	-0.394 (0.293)
Election for term-limited seat	0.033 (0.089)	0.323*** (0.107)	0.067*** (0.023)	-0.214 (0.703)
Observations	807	807	807	503
Mean dep var	7.269	5.897	0.309	4.796

Notes: This table is similar to Panel A Table 6. The difference is that our sample excludes states that allow legislators to set their own salaries (i.e., we restrict to states with salaries set by a compensation board or by the constitution).

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D9: Lagged Productivity and Salary: Ruling Out that Observably More Productive Politicians Are Less Likely to Experience Salary Increases

Panel A - Legislator Salary		Dependent Variable: Log Salary	Observations	R-squared	Mean dep var
(1) Log(Number of Bills Introduced) $_{t-1}$	0.039 (0.032)		1,215	0.945	7.269
(2) Log(Number of Bills Approved) $_{t-1}$	0.041 (0.030)		1,214	0.945	5.897
(3) Share of Bills Approved $_{t-1}$	0.146 (0.100)		1,215	0.945	0.309
(4) Log(Income per Capita) $_{t-1}$	0.0362 (0.765)		986	0.951	2.764
(5) Log(Expenditures per Capita) $_{t-1}$	0.400* (0.211)		986	0.951	7.580
(6) Log(Taxes per Capita) $_{t-1}$	0.527* (0.289)		986	0.951	6.890
Panel B - Governor Salary		Dependent Variable: Log Salary	Observations	R-squared	Mean dep var
(1) Log(Income per Capita) $_{t-1}$	-0.548 (0.765)		665	0.759	2.716
(2) Log(Expenditures per Capita) $_{t-1}$	0.065 (0.228)		665	0.757	7.545
(3) Log(Taxes per Capita) $_{t-1}$	0.279 (0.270)		665	0.760	6.847

Notes: This table analyzes the effect of legislative productivity on legislative salary in the subsequent year using data from 1968-2007 in rows 1-3 and from 1972-2000 in rows 4-6 of Panel A, and, in Panel B, the effect of governor productivity on governor salary in the subsequent year using data from 1968-1994 in rows 1-3 and from 1972-1994 in rows 4-6. An observation is an election. Robust standard errors clustered by state in parentheses. Log salary is in real dollars. Regressions include the same set of controls and fixed effects as in Tables 2 and 4.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D10: Lagged Average Productivity to Date and Salary: Ruling Out that Observably More Productive Politicians Are Less Likely to Experience Salary Increases

	(1)	(2)	(3)	(4)	(5)	(6)
L. Avg Bills introduced per year	0.001 (0.001)	0.001 (0.001)				
L. Avg Number of Bills Approved			0.001 (0.001)	0.001 (0.001)		
L. Avg Share of bills approved					0.011 (0.022)	0.010 (0.022)
Session length in election year (in hundreds of days)		0.082** (0.039)		0.081** (0.039)		0.081** (0.039)
Election for term-limited seat	0.169 (0.156)	0.183 (0.159)	0.169 (0.156)	0.183 (0.159)	0.170 (0.155)	0.184 (0.158)
Observations	1,359	1,356	1,359	1,356	1,359	1,356
Mean dep var	7.269	7.269	5.897	5.897	0.309	0.309

Notes: This table analyzes whether politicians with higher average productivity to date, measured in terms of lagged values of bill-introduction and bill-passing, are more or less likely to receive salary increases. Additional controls included in all regressions are the log of state population, and the log of GDP per capita. State and year fixed effects are included in all regressions. Standard errors clustered by state in parentheses.

Table D11: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes, Robustness to Controlling for Legislature Characteristics

Panel A - All Elections									
	Contested			Number of Candidates			Margin of Victory		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log salary	0.025** (0.012)	0.026** (0.013)	0.026** (0.012)	0.046** (0.020)	0.052** (0.020)	0.052*** (0.018)	0.014 (1.399)	-0.730 (1.187)	-0.685 (1.144)
Session length in election year (in hundreds of days)			0.009 (0.013)			0.022 (0.025)			0.118 (0.885)
Any personal staff			0.034 (0.020)			0.046 (0.035)			-0.933 (1.554)
Any shared staff			-0.021 (0.020)			0.008 (0.033)			1.852 (1.246)
Any district staff			-0.058* (0.030)			-0.162*** (0.054)			1.769 (2.315)
Biennial session frequency			-0.009 (0.021)			0.036 (0.054)			2.212 (1.794)
Other Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	82,992	82,992	82,992	82,992	82,992	82,992	80,707	80,707	80,707
R-squared	0.155	0.161	0.162	0.158	0.164	0.166	0.157	0.162	0.162
Mean dep var	0.734	0.734	0.734	1.933	1.933	1.933	47.04	47.04	47.04
Panel B - Lower House Elections									
	Contested			Number of Candidates			Margin of Victory		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log salary	0.035* (0.018)	0.039** (0.017)	0.039** (0.017)	0.062** (0.027)	0.071*** (0.026)	0.073*** (0.024)	0.119 (1.925)	-1.014 (1.603)	-0.952 (1.551)
Session length in election year (in hundreds of days)			0.012 (0.016)			0.017 (0.028)			0.212 (1.045)
Any personal staff			0.035 (0.022)			0.044 (0.035)			-1.040 (1.715)
Any shared staff			-0.028 (0.023)			-0.005 (0.036)			2.037 (1.322)
Any district staff			-0.060* (0.035)			-0.168*** (0.059)			1.014 (2.614)
Biennial session frequency			-0.011 (0.025)			0.034 (0.059)			2.057 (2.181)
Other Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	63,095	63,095	63,095	63,095	63,095	63,095	61,232	61,232	61,232
R-squared	0.164	0.167	0.168	0.169	0.171	0.173	0.163	0.167	0.167
Mean dep var	0.719	0.719	0.719	1.908	1.908	1.908	48.30	48.30	48.30

Notes: This table analyzes the effect of legislative salary on candidate selection using US state legislative elections from 1967-2003. An observation is an election. Robust standard errors clustered by state in parentheses. The contested election variable is a dummy for whether the election had more than one candidate. Additional controls are the log of state population, a dummy for upper-house elections in panel one only, a dummy for elections for term limited seats and a dummy for elections for four-year term lengths term limits. State and year fixed effects are also included in all regressions.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D12: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes Using State Level CPI

Panel A - All Elections					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.027 (0.018)	0.056* (0.029)	-0.349 (1.572)	-0.023 (0.017)	0.069** (0.028)
Upper house	0.063*** (0.022)	0.106*** (0.030)	-3.298** (1.526)	-0.017 (0.012)	-0.012 (0.027)
Session length in election year (in hundreds of days)	0.003 (0.015)	0.007 (0.031)	-0.484 (1.061)	0.005 (0.009)	0.019 (0.012)
Election for term-limited seat	0.072** (0.034)	0.120* (0.068)	-2.832 (2.708)	-0.149*** (0.031)	0.019 (0.038)
Election for seat with four year term length	-0.016 (0.023)	-0.016 (0.032)	-0.884 (1.670)	-0.110*** (0.016)	0.017 (0.030)
Observations	74,835	74,762	72,629	74,734	74,428
Mean dep var	0.734	1.933	47.04	0.685	0.590
Panel B - Lower House Elections					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.043 (0.026)	0.081** (0.039)	-0.267 (2.101)	-0.029 (0.020)	0.079** (0.036)
Session length in election year (in hundreds of days)	0.007 (0.019)	0.010 (0.036)	-0.380 (1.215)	0.009 (0.009)	0.023* (0.013)
Election for term-limited seat	0.095** (0.036)	0.142* (0.073)	-4.280 (3.081)	-0.150*** (0.035)	0.018 (0.039)
Election for seat with four year term length	0.227*** (0.065)	0.728*** (0.218)	-14.063 (15.329)	0.027 (0.180)	0.461** (0.192)
Observations	57,030	56,980	55,220	56,939	56,700
Mean dep var	0.719	1.908	48.30	0.706	0.597
Panel C - Upper House Elections					
	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	0.005 (0.015)	0.021 (0.028)	-0.471 (1.803)	-0.011 (0.020)	0.048** (0.023)
Session length in election year (in hundreds of days)	-0.010 (0.018)	0.001 (0.034)	-1.380 (1.368)	0.004 (0.018)	0.008 (0.015)
Election for term-limited seat	-0.008 (0.038)	0.030 (0.065)	1.627 (3.442)	-0.138*** (0.035)	0.023 (0.042)
Election for seat with four year term length	-0.019 (0.026)	-0.012 (0.039)	0.639 (1.062)	-0.001 (0.032)	0.021 (0.015)
Observations	17,805	17,782	17,409	17,795	17,728
Mean dep var	0.782	2.011	43.05	0.617	0.565

Notes: Panel A here is similar to Table 2. The difference is that log salary is deflated using using a state-level CPI, using an approach similar to Moretti (2013) (details in Section 4.6 and Appendix B.1). Panels B and C follow Panel A, but restrict to lower house or upper house elections.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D13: Impact of Salary on Bill Passing and Spending Using State Level CPI

Panel A - Legislator Salary and Bill Introduction, and Approval						
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Number of Bills Introduce)		Log(Number of Bills Approved)		Share of Bills Approved	
Log salary	0.032 (0.033)	0.017 (0.035)	0.075* (0.042)	0.060 (0.043)	0.014 (0.009)	0.014 (0.009)
Session length in election year (in hundreds of days)		0.219* (0.120)		0.245** (0.114)		0.013 (0.012)
Election for term-limited seat	0.033 (0.077)	0.073 (0.079)	0.321*** (0.099)	0.358*** (0.106)	0.077*** (0.018)	0.078*** (0.019)
Observations	1444	1440	1443	1439	1444	1440
Mean dep var	7.269	7.269	5.888	5.888	0.306	0.306
Panel B - Legislator Salary and Taxes, Expenditures, and Income						
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Income per Capita)		Log(Expenditure per Capita)		Log(Taxes per Capita)	
Log salary	-0.016* (0.009)	-0.006 (0.004)	0.009 (0.009)	0.013 (0.009)	-0.004 (0.020)	0.009 (0.011)
Session length in election year (in hundreds of days)		-0.001 (0.002)		0.001 (0.009)		-0.009 (0.009)
Election for term-limited seat	-0.033** (0.015)	-0.011 (0.009)	-0.047** (0.021)	-0.035* (0.018)	-0.042 (0.059)	-0.017 (0.040)
Observations	958	958	958	958	958	958
Mean dep var	2.720	2.720	7.555	7.555	6.858	6.858

Notes: This table is similar to Panels A and B of Table 6. The difference is that log salary is deflated using using a state-level CPI, using an approach similar to Moretti (2013) (details in Section 4.6 and Appendix B.1).

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D14: Narrative Sample Analysis: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes

	(1)	(2)	(3)	(4)	(5)
	Contested Election	Number of Candidates	Margin of Victory	Incumbent Re-elected	Democrat Wins
Log salary	-0.002 (0.034)	-0.053 (0.090)	3.212 (3.194)	0.005 (0.041)	-0.034 (0.025)
Upper house	0.017* (0.009)	0.015 (0.031)	-0.669 (1.225)	-0.037 (0.033)	0.009 (0.014)
Session length in election year (in hundreds of days)	0.010 (0.008)	0.066*** (0.016)	0.171 (0.803)	0.007 (0.007)	0.017*** (0.004)
Election for term-limited seat	0.086 (0.098)	0.230* (0.124)	-5.270 (7.398)	-0.155*** (0.040)	0.002 (0.050)
Election for seat with four year term length	-0.010 (0.010)	-0.000 (0.028)	-1.458 (1.731)	0.002 (0.037)	-0.012 (0.017)
Observations	24443	24331	24388	24439	24442
Mean dep var	0.848	2.244	39.52	0.340	0.297

Notes: This table is similar to Table 2. The difference is that we restrict to our “narrative analysis sample”, that is, restricting to the 8 year periods preceding and following the sample of salary change events listed in Appendix B.13.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D15: Legislative Salary, Family Income, and Outside Labor Supply

Panel A	(1)	(2)	(3)	(4)
	Dep var: Family Income (in \$)			
Total salary (in dollars)	0.759*** (0.102)	0.436*** (0.143)	0.706*** (0.106)	0.367** (0.139)
Session length in election year (in hundreds of days)		-3,823.909 (2,685.972)		-2,740.866 (2,505.795)
Year fixed effects	No	No	Yes	Yes
Observations	5,703	5,703	5,703	5,703
Mean dep var	101392	101392	101392	101392
Panel B	(1)	(2)	(3)	(4)
	Dep var: Has Outside Job (0 or 1)			
Total salary (in thousands of dollars)	-0.006*** (0.001)	-0.004*** (0.001)	-0.006*** (0.001)	-0.003*** (0.001)
Session length in election year (in hundreds of days)		-0.047** (0.022)		-0.056** (0.023)
Year fixed effects	No	No	Yes	Yes
Observations	5,539	5,539	5,539	5,539
Mean dep var	0.652	0.652	0.652	0.652

Notes: OLS regressions with standard errors clustered by state in parentheses. The data is from the surveys of state legislators done by Carey et al. (1995) and Carey et al. (2002). The log of state population, log of GDP per capita, a dummy for states with biennial session frequency, and measures of legislator staff are also included in regressions 2 & 4.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table D16: Is Salary Correlated with whether Politicians Plan on Becoming Lobbyists After Office?

Dep var: Politician Plans on Doing Lobbying or Consulting after Serving in Office (0-1)	(1)	(2)	(3)
Log(1+Salary)	0.002 (0.003)	0.004 (0.003)	-0.007 (0.017)
Session length in election year (in hundreds of days)		-0.010 (0.011)	0.005 (0.018)
State & Year fixed effects	No	No	Yes
Observations	6,367	6,367	6,367
Mean dep var	0.148	0.148	0.148

Notes: OLS regressions with standard errors clustered by state in parentheses. An observation is a politician. The dependent variable is whether a politician indicated that they were likely to do lobbying or consulting after service in the legislature. The data is from the surveys of state legislators done by Carey et al. (1995) and Carey et al. (2002). The log of state population, log of GDP per capita, a dummy for states with biennial session frequency, and measures of legislator staff are also included in regressions 2 & 3.

This table illustrate two points. First, the share of politicians intending to become lobbyists or consultants is 15%. Second, there is no relationship between salary and whether politicians later intending to become lobbyists or consultants.

Table D17: The Effect of Legislative Salary on Electoral Competitiveness and Electoral Outcomes, Effects By Party Strength (Mayhew Ratings)

	(1) Contested Election	(2) Number of Candidates	(3) Margin of Victory	(4) Incumbent Re-elected	(5) Democrat Wins
Log salary	0.040** (0.018)	0.066** (0.025)	-3.062* (1.795)	-0.025 (0.017)	0.086** (0.032)
Strong party * Log salary	-0.034 (0.023)	-0.038 (0.046)	5.396** (2.013)	0.024 (0.016)	-0.046 (0.038)
Observations	82,533	82,447	80,248	82,353	82,027
Mean dep var	0.734	1.933	47.04	0.685	0.590

Notes: The regressions are the same as in Table 2, with the addition of the interaction term “Strong party * Log salary”. Whether a state is a “strong party” state or not is absorbed in the state fixed effects.

* significant at 10%; ** significant at 5%; *** significant at 1%

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