Do Powerful Politicians Cause Corporate Downsizing?*

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This draft: October 7, 2011
First draft: April 3, 2009

* We would like to thank Stephen Ansolabehere, Malcolm Baker, Utpal Bhattacharya, Bo Becker, Dan Bergstresser, John Campbell, Jonathan Carmel, John Cochrane, Randy Cohen, Shawn Cole, Jess Cornaggia, Stefano DellaVigna, Ray Fisman, Fritz Foley, Ken Froot, Eitan Goldman, David Goldreich, Rick Green, Chris Leach, Francisco Perez-Gonzalez, Thomas Philippon, Monika Piazzesi, Julio Rotemberg, Al Roth, Rick Ruback, Huntley Schaller, Chester Spatt, Erik Stafford, Jeremy Stein, Anjan Thakor, Tuomo Vuolteenaho, Eric Zitzewitz, three anonymous referees, and seminar participants at Colorado, Georgia Tech, Harvard Business School, Indiana, Michigan, Penn State, Princeton, Rotterdam, Singapore Management University, Texas, Tilburg, Toronto, UCLA, the NBER Political Economy Meeting, the Washington University Corporate Finance Conference, the UBC Summer Finance Conference, the Financial Research Association (FRA) Conference, the Western Finance Association Meetings (Victoria, Canada), the Utah Winter Finance Conference, and the United States Congress for helpful comments and discussions. We are grateful to Diego Garcia and Oyvind Norli for providing data on geographic locations of firm operations. We also thank David Kim for excellent research assistance. We are grateful for funding from the National Science Foundation.

ABSTRACT

This paper employs a new empirical approach for identifying the impact of government spending on the private sector. Our key innovation is to use changes in congressional committee chairmanship as a source of exogenous variation in state-level federal expenditures. In doing so, we show that fiscal spending shocks appear to significantly dampen corporate sector investment and employment activity. This retrenchment follows both Senate and House committee chair changes, occurs in large and small firms and within large and small states, and is most pronounced among geographically-concentrated firms. The effects are economically meaningful and the mechanism - entirely distinct from the more traditional interest rate and tax channels - suggests new considerations in assessing the impact of government spending on private sector economic activity.

JEL Classification: E13, E62, G31

Key words: Government spending, seniority, corporate behavior, investment, earmarks

Does public sector spending complement or crowd out private sector economic activity? This question, which has occupied economists for much of the past century, remains largely unresolved. Keynesian and neoclassical macroeconomic theories reach strong and generally conflicting conclusions regarding the ability of public spending to stimulate the private sector. A major obstacle limiting empirical progress on the topic is the difficulty in identifying changes to government spending that are truly exogenous. Because government behavior is influenced by developments in the private economy, changes in private sector investment and productivity confound the effects of government spending and the factors that cause that spending to change.

This paper offers a novel empirical approach that allows us to overcome this challenge and shed considerable light on the impact of government spending on the private sector. Our key innovation is to use changes in congressional committee chairmanship as a source of exogenous variation in state-level federal expenditures. Since chairmanship is determined almost entirely by seniority – to be appointed chair a congressman must simply become the most senior member of the party in power on that committee[1] – this means that chair turnover can only result from the resignation (or defeat) of the incumbent, or a change in the party controlling that branch of congress. And because both of these events depend almost entirely on political circumstances in *other* states, ascension to chairmanship is essentially unrelated to events or conditions in the new chairman's home state (e.g., most Senators will not even be up for election during the year of their ascension). We show that becoming a powerful committee chair results in a significant increase in federal funds flowing to the ascending chairman's state.[2] Thus, a congressman's ascension to a powerful committee chair creates a positive shock to his or her state's share of federal funds that is virtually independent of the state's economic conditions.

We focus specifically on the 232 instances over the last 42 years where the

[1] This use of seniority-based chairmanship has been a governing practice in both houses of Congress for over 100 years. In recent years there have been occasional deviations from this rule (see Deering and Wahlbeck (2006)), but our results are very similar if we exclude these exceptions, or if we use changes in the identity of the most-senior committee member in place of changes in committee chairmanship.

[2] In describing the impact of his Senate seniority on his home state of Pennsylvania, Arlen Specter recently remarked: "My senior position on appropriations has enabled me to bring a lot of jobs and a lot of federal funding to this state. Pennsylvania has a big interest in my seniority, a big interest."

senator or representative of a particular state ascends to the chairmanship of a powerful congressional committee. During the year that follows the appointment, the state experiences an increase of 40-50 percent in their share of federal earmark spending, a 9-10 percent increase in total state-level government transfers, and a 24 percent increase in total government contracts. Because these spending shocks are sufficiently numerous, are spread out across time and different locations, and are economically meaningful, they provide us with significant power to examine the impact of fiscal policy on the private sector.

We then investigate the private sector consequences of these seniority-linked government spending shocks by studying the behavior of the public corporations headquartered in the congressman's state. From the Keynesian perspective, the predicted response to spending shocks is unambiguous. Because the spending is "free" from the recipient state's perspective – it will largely be paid for by residents of other states as opposed to increases in the state's taxes or interest rate – the funds are expected to help firms increase investment, employment, and output. The neoclassical point of view is quite different. We present a simple model in which individuals face a labor-leisure trade-off in the presence of government spending. Increased resources from the government that are not expected to be funded by taxes or borrowing induce individuals to increase their consumption and leisure. The resulting decline in the marginal productivity of capital compels companies to scale back investment and output.

Our empirical results support the predictions of the neoclassical model. Focusing on the investment (capital expenditure), employment, R&D, and payout decisions of these firms, we find strong and widespread evidence of corporate retrenchment in response to government spending shocks. In the year that follows a congressman's ascendency, the average firm in his state cuts back capital expenditures by roughly 15%. These firms also significantly reduce R&D expenditures and increase payouts to their investors. The magnitude of this private sector response is nontrivial: in the median state (which receives roughly $452 million per year in increased earmarks, federal transfers, and government contracts as a result of a seniority shock), capex and R&D reductions total $48 million and $44 million per year, respectively, while payout increases total $27 million per year. These changes in firm behavior persist throughout the chairmanship and begin to reverse after the congressman relinquishes the chairmanship. We also find some evidence that firms scale back their employment, and experience a decline in sales growth in response to the government spending shock.

To explore the robustness of these findings, we verify that the patterns hold up under a wide variety of conditions and specifications. We employ panel regressions using state and time fixed effects and a range of controls. We also conduct state-level regressions, averaging coefficients across states, verifying that a powerful committee chair has a statistically and economically large impact on the decisions of firms in their state.

We also examine a variety of other predictions of how spending is likely to impact private sector firms. In particular, we find that our results are mainly found in firms with geographically concentrated operations, e.g., firms that operate only in the state that experiences a spending shock, as well as firms with high capacity utilization (i.e., those with little slack in their capital stock). Also, consistent with Keynes' view that crowding out should only occur under conditions of full employment, we find a stronger firm response to spending shocks when state-level employment, state-level real GDP growth, and US real GDP growth are at or above their long-term historical averages.

A unique feature of our approach is that we can rule out the standard tax and interest rate channels as explanations for how government spending crowds out private sector investment. Since our mechanism entails simply shifting the same government spending from the former chairman's state to the new chairman's state, no new government funds are implied; as a result, no increased taxation or increased borrowing costs are required. In addition, we conduct cross-state comparisons, thus abstracting from all national level effects. Thus, our approach identifies a distinct and alternative mechanism by which government spending deters corporate investment. In particular, we provide evidence that crowding out occurs through factors of production including the labor market and fixed industrial assets. These findings argue that tax and interest rate channels, while obviously important, may not account for all or even most of the costs imposed by government spending. Even in a setting in which government spending does not need to be financed with additional taxes or borrowing, its distortionary consequences may be nontrivial.

The remainder of the paper is organized as follows. Section I provides a brief background and literature review. Section II presents a simple neoclassical model to motivate our empirical approach. Section III describes the data we use, while Sections IV and V explore our findings on the effects of seniority on congressional spending, and firms' responses to these seniority shocks in their respective states. Section VI explores state-level outcome variables. Section VII provides a more detailed discussion of these findings. Section VIII concludes.

I. Background and literature review

There is a large empirical literature investigating the impact of government spending on consumption, investment, and output variables. The standard approach in this literature is to apply a VAR methodology to macroeconomic data in order to identify shocks to government spending.[3] Most of these studies focus on quarterly post-war data in the U.S., which places a heavy burden on the econometrics to uncover the relationship from a limited time series of highly persistent variables. Although some studies consider international panel data, variation in economic size and openness, labor market rigidities, and other considerations limit the amount of additional power these data add.[4] The literature has also pursued some alternative strategies to isolate changes in government spending that are exogenous. For instance, several studies focus on periods of significant expansion in US defense spending (the so-called "Ramey-Shapiro episodes") to examine the impact of spending shocks.[5] Because defense spending is viewed to be largely independent of domestic macroeconomic considerations, major changes therein offer opportunities to examine exogenous spending shocks. Unfortunately, the occurrence of large and unambiguous shocks to government defense spending is somewhat rare, which restricts the power of these tests.[6]

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[3] See, for example, Rotemberg and Woodford (1992), Blanchard and Perotti (2002), Fatás and Mihov (2001), Mountford and Uhlig (2002), Perotti (2005), Pappa (2005), Caldara and Kamps (2006), and Galí, López-Salido, and Vallés (2007), Ramey (2008).
[4] See Giavazzi and Pagano (1990).
[5] See Ramey and Shapiro (1998), Edelberg, Eichenbaum, and Fisher (1999), Burnside, Eichenbaum, and Fisher (2004), and Cavallo (2005)).
[6] More recently, Nakamura and Steinsson (2011) use differential state-level responses to aggregate military spending fluctuations in order to identify the effects of government spending on output. Cullen and Fishback (2006) also document significant county-level variation in WWII spending increases and use this to examine the impact of government spending on longer-term private sector economic activity.

An advantage of our approach is that we are able to examine numerous exogenous shocks to state-level federal expenditures over an extended period of time and to quantify their impact on the behavior of US public corporations. In doing so, we add to a growing literature that instruments for government spending at the state-level. Many of these papers examine the effects of government spending during specific periods of economic stress and factor underutilization, when the multiplier should be at its largest according to the Keynesian model. For example, Clemens and Miran (2010) use variation across states in balanced budget amendments to estimate the income effects around periods of fiscal stress and spending cuts; Chodorow-Reich, et al. (2010) and Wilson (2011) examine state-level spending effects around the 2009 American Recovery and Reinvestment Act (ARRA); and Fishback and Kachanovskaya (2010) instrument for New Deal spending at the state level using the political competitiveness of different states. Finally, Serrato and Wingender (2011) and Shoag (2011) also exploit local variation to identify the impact of government spending; Serrato and Wingender (2011) use county-level Census population count revisions to identify increases in federal, non-discretionary spending, while Shoag (2011) uses idiosyncratic pension returns to instrument for windfall-driven state-level spending.

Finally, our paper relates to another strand of the literature that examines how political representation translates to government expenditures. These studies include Atlas et al. (1995), Hoover and Pecorino (2005), Crain and Tollison (1977, 1981), Goss (1972), Greene and Munley (1980), Kiel and McKenzie (1983), Ray (1980, 1981), Ritt (1976), Rundquist (1978), and Rundquist and Griffiths (1976). Atlas et al. (1995) and Hoover and Pecorino (2005) document a positive relationship between per capita representation in the Senate and state-level federal expenditures but find only limited evidence with respect to House representation. Levitt and Poterba (1999) also find somewhat mixed evidence linking congressional seniority to federal spending; they do, however, find that senior Democratic members of the House were able to use their positions to improve their state's economic performance. Lastly, Aghion et al. (2009) show that representation on appropriations committees has an effect on education expenditures to states, finding support for some of these expenditures translating into future growth.[7] Taken as a whole, the literature finds only modest linkages between the nature of congressional representation and the distribution of congressional spending. Using novel data on government discretionary earmark spending, our paper adds new evidence to this literature by showing that changes in congressional committee chairmanship can have a significant influence on government spending outcomes.

II. Model[8]

In this section, we review the predictions of the standard neoclassical model in the context of shocks to state-level federal transfers that are the focus of this study. Specifically, consider each state as a small, open economy inhabited by a representative agent who maximizes the following utility function:

$$V(W_t) = \sum_t \beta^t U(C_t, L_t), \qquad 0 < \beta < 1, \quad (1)$$

where U is concave, strictly increasing, and twice continuously differentiable in both arguments. Period-t consumption is captured by C_t and leisure by L_t. The agent is endowed with one unit of time such that period-t labor is $1\text{-}L_t$. The agent also receives transfers from the federal government and pays lump-sum taxes to the state government. The agent possesses an in-state Cobb-Douglas production technology to which he can allocate his labor and capital. Period-t wealth (net of state taxes and federal transfers) that is not consumed or allocated to in-state production can be invested outside of the state and earns a fixed rate of return r. Thus, equation (1) is maximized subject to the following wealth transition equation:

$$W_{t+1} = (W_t - C_t - G_t + E_t - K_t)(1+r) + K_t^{\alpha}(1\text{-}L_t)^{1-\alpha} + (1-\delta)K_t \qquad (2)$$

where W_t is wealth, G_t is state taxes, E_t is state-level federal transfers, K_t is the in-state

[7] Other papers that have used instruments for state-level government spending include Knight (2002 and 2005) who uses transportation committee membership, and Anderson and Tollison (1991) and Gruber and Hungerman (2007), who use the tenure of appropriations committee members.

[8] We are grateful to an anonymous referee for articulating the main features of this model to us.

capital stock, L_t is leisure, α captures the relative productivity of capital, and δ reflects the rate of depreciation. Given period-t transfers, government spending, and wealth, the agent selects in-state capital stock, consumption, and leisure to maximize utility. Note that unanticipated increases in federal transfers to the state (E), to the extent they are not fully offset by increases in state government spending (G), will be tantamount to an increase in the representative agent's current period wealth (W).

First-order and envelop conditions place the following restrictions on consumption, labor, and capital:

$$V_W(W_t) = U_c \qquad (3)$$

$$U_L = U_c \frac{1-\alpha}{1+r}\left(\frac{r-\delta}{\alpha}\right)^{\frac{\alpha}{\alpha-1}} \qquad (4)$$

$$K_t = (1-L_t)\left(\frac{r-\delta}{\alpha}\right)^{\frac{1}{\alpha-1}} \qquad (5)$$

These immediately imply that shocks to state-level federal transfers (E) that effectively increase current wealth (W) result in increased consumption (C) and leisure (L) and a decline in the in-state capital stock (K). Intuitively, increased federal transfers induce the wealthier agent to consume more and work less. Reduced labor lowers the marginal product of in-state capital and therefore leads to a lower optimal in-state capital stock. The decline in labor and capital means that output -- and therefore sales -- will also decline. And clearly if the optimal reduction in capital stock exceeds the rate of depreciation, in-state capital stock must be liquidated. To the extent that one thinks of the in-state production technology as a firm, this implies that the firm's payouts to its shareholders (i.e. to the representative agent) must increase. The model has relatively little to say about R&D investments, but one can easily imagine an extension that includes intellectual capital as a third factor of production. When federal transfers cause a reduction in labor supply, the marginal product of intellectual capital also declines and therefore makes R&D investments less worthwhile. Thus, under the neoclassical model, one would expect shocks to state-level federal transfers to produce increased consumption, reduced employment, reduced capital expenditures, reduced sales, and increased shareholder payouts.

III. Data

The data in this study are collected and coded from several sources. For the majority of sources, we hand-collect, -code, and -match the data to combine the sources for our analysis. To start, we obtain congressional earmark data from Citizens Against Government Waste, which collects earmark data by state starting in 1991. An earmark is defined as a line item in an appropriations bill that designates tax dollars for a specific purpose in circumvention of established budgetary procedures. While some of the earmarks are state designated, many are not, and so we read through and hand-match over 24,000 of these undesignated earmarks to the specific designated state. In addition, for earmarks designated to more than one state, we split the amounts equally among the designated states. For instance, one $200,000 earmark had no specific state designation, but was simply listed as designated for the "Sokaogon Chippewa Community," to "investigate impacts of a mine." As this is a band of the Lake Superior Chippewa residing in Wisconsin, we match this earmark back to Wisconsin. In addition, an example of a multi-state designated earmark is a $5,500,000 earmark labeled: "Dalles Powerhouse (Units 1-14), WA & OR (Corps of Engineers - Construction, General)", which we split equally at $2,750,000 to both of the affected states, WA and OR.

In addition to the earmark data, we also collect data on broader categories of government expenditures. We obtain these from the annual survey of state and local government finances conducted by the US Census Bureau and reported on their website,[9] with the data starting in 1992, broken down at the state level. These transfers include highway and parks funding, agricultural funds, and other payments distributed to states.[10] In addition to this transfer data, we collect state-level population and square mileage figures from the Census Bureau.

Lastly, we use data on congressional committees from Stewart and Woon (2009) and Nelson (2005),[11] and link politicians (by state) to firms using the headquarters of all

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[9] http://www.census.gov/govs/www/estimate.html.

[10] In our tests using this transfer data, we exclude category B79, which consists of nondiscretionary spending on public welfare items, e.g., Medicaid.

[11] This data is available online on Charles Stewart's website: http://web.mit.edu/17.251/www/data_page.html.

firms listed on Compustat.[12] Congressional committee data is available for the 80th to 110th Congresses (corresponding to the time period 1947-2009), which allows us to match politicians to firms as far back as accurate Compustat accounting information for our firm measures is available (1967).[13] From Compustat, we extract firm-specific accounting variables, such as capital expenditures, research and development (R&D) expenditures, total payouts (equal to cash dividends plus repurchases), and number of employees.

We define seniority shocks by assigning a dummy variable equal to 1 if the senator (or representative) of a given state first becomes chairman of an influential congressional committee. The list of the 10 most influential committees is from Edwards and Stewart (2006); for the Senate these committees are Finance, Veterans Affairs, Appropriations, Rules, Armed Services, Foreign Relations, Intelligence, Judiciary, Budget, and Commerce, and for the House these committees are Ways and Means, Appropriations, Energy and Commerce, Rules, International Relations, Armed Services, Intelligence, Judiciary, Homeland Security, and Transportation and Infrastructure. We categorize shocks into various groups based on the committee rankings; for example, Shock Top1ChairOnly means the senator (representative) was appointed chairman of the top-ranked Senate Finance Committee (the House Ways and Means Committee). We also construct an alternative shock definition that includes both the chairman and the ranking minority member (i.e., the most senior committee member who is a member of the party *not* currently in control of that House of congress), so that Shock Top1Chair&Rank is equal to 1 if a senator becomes either chairman or the ranking minority member of the committee, when he/she was previously not in either position in the prior Congress. In our baseline specification, we code seniority shocks as starting in the year of appointment, and apply them for 6 years (term of a senator), although we vary this timing in a number of robustness checks.[14]

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[12] Compustat's firm headquarters variable is backfilled, so that firms that have moved are miscoded historically; however, the incidence of firm headquarters relocation is extremely rare, and we have corrected the obvious errors.
[13] For members of the House of Representatives, note that we are unable to historically match all firms to individual congressional districts, since mappings between zipcodes and congressional districts are only available from the 103rd Congress onwards; thus we map both senators and representatives to their home states.
[14] We apply these shocks only to firms alive in the initial year of the shock, but we have also run all of the tests in the paper with shocks applied to all firms alive at any point during the full shock period and the

IV. Results

A. Congressional Spending and Seniority

Our main sample focuses on the behavior of 16,734 firms over the past 42 years (1967-2008). Summary statistics are reported in Table I. In addition to our main dependent and control variables, Panel A reports the fraction of firm-year observations that occur in a state represented by a congressman who has been appointed chair (or has become the ranking minority member) of a powerful congressional committee within the past six years. We consider separately observations represented by a congressman chairing a Senate committee and chairing a House committee. We use the Edwards and Stewart (2006) ranking of committees to identify the most powerful committees (outlined in Section III) and report the fraction of firm-year observations from the top 1, 3, 5, and 10 most powerful Senate committees as well as the top 1 and 3 most powerful House committees.

Table I indicates that, depending on how many committees are included, between 3.0% and 19.6% of the firm-year observations are headquartered in states represented by a senator that has recently become chairman (or ranking minority member). We also report the fraction of firm-year observations in states where the senator stepped down from the chair within the past six years. The last six rows of Panel A also examine shocks to the most powerful House committees. For the House shocks, we see that a relatively greater fraction of firm-year observations occur in states represented by a House committee chair. This reflects the fact that larger states, which have larger House congressional delegations, are more likely to find one of their representatives chairing a powerful House committee. This also suggests that our House and Senate shocks are occurring in a relatively non-overlapping set of firm-year observations.

More generally, an advantageous aspect of our data and identification is that House and Senate committee chair shocks occur, in large part, in different states (and years). Thus, each chamber's shocks can be seen as independent testing samples for the effect of these government spending shocks on firm behavior. This state-shock difference is seen more clearly in the last two columns of Table II.

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results are very similar to those reported here.

We also report summary statistics for our state-level variables in Panel B of Table I. Since we only have earmark data from 1991 through 2008, the main variables are reported for this 18-year period. Average annual earmarks are $139 million per state (in 2008 dollars), with the median state receiving $91 million in a given year. Appendix Table A1 reports the average number of years that each chairman (or ranking member) remains in his position, for all Top 10 committees. In the Senate, the average chair tenure is 7.6 years (median of 6, max of 24), and in the House, the average chair tenure is 6.3 (median 6, max of 24); these figures suggest that firms are unlikely to view these seniority shocks as temporary events that might induce firms to simply shift capital or labor investments out a few years into the future, but rather as a long-term shock. For instance, it is unlikely that a firm could convince workers to take (in expectation) a seven-year furlow; thus the reductions we see are likely not short-run shifts in allocations over time.

Table II confirms that most states have had, at some point in the past 42 years, one of their senators or representatives chairing a powerful committee. And while earmark spending lines up somewhat well with population, a number of low-population states appear surprisingly high on the list. To see this more closely, in Figure I we plot earmarks against state population. The expected positive relationship is confirmed but the figure also reveals a number of significant positive outliers in terms of earmarks, the largest of which are Hawaii, Alaska, Mississippi, West Virginia, and Alabama: All states which had powerful congressional chairmen over our sample period.

In Table III, we report the results of regressions that seek to explain variation in annual state-level earmarks with changes in congressional committee chairmanship. We include state and year fixed effects in each regression, and standard errors are clustered at the state level.[15] The analysis reveals a strong relationship between seniority shocks and earmark spending. A state whose senator is appointed chair of one of the three most powerful committees receives roughly a 40-50% increase in earmark spending. For

[15] Also, we have run these tests clustering standard errors by state-shock period, rather than just state. Since our analysis exploits variation within state, clustering by state is conservative. Clustering by state-shock period produces smaller standard errors (and so larger t-stats), for instance the full specification Column 2's t-stat is 9.07 (vs. 8.77 with the currently reported clustering by state), and so we report the more conservative measure.

instance, the coefficient on ShockTop1ChairOnly in Column 2 indicates that having a top committee chair increases that state's earmarks over the subsequent six years by 48.1% (t=8.77) per year. From Table I, the average (median) annual earmarks per state are \$139 million (\$91 million) in 2008 dollars, so this implies a \$67 million (\$44 million) increase in earmarks per year to a state upon having its senator appointed chairman of the Senate Finance Committee (most powerful Senate committee). In Column 2, we add a series of controls, including (the log of) state-level population, the state-level average of (log) per capita income over the past 6 years, and lagged values of state level (log) per capita income growth and state-level unemployment rates. Including these controls leaves the results unchanged.

As we broaden the set of powerful committees, the effect gets weaker but remains large and statistically significant (for the Top 10 committees, the increase is 22.4% (t=2.49)). The same holds true as we include ranking minority members (e.g. Chair&Rank vs. ChairOnly). To the extent that these senators are less powerful than those chairing one of the very top committees, we might expect a decline in their ability to deliver earmark spending to their state. However, even in these Chair&Rank measures, the effect remains large and statistically significant. In unreported tests we also find evidence that earmark spending *declines* upon the departure of a committee chair, with states represented by a senator who relinquishes one of the top committees experiencing a 20-30% decline in their earmarks.[16]

We also examine whether our results apply to broader measures of state-level federal spending. Specifically, Column 10 of Table III uses data from the US Census Bureau's annual survey of state and local government finances. We use annual federal transfers to state governments from 1992-2007 on the left-hand side of these regressions in place of the annual earmarks employed previously. These transfers, which include highway and parks funding, agricultural funds, etc., average roughly \$3.7 billion per year per state (in 2008 dollars). Although this measure is noisier and likely contains elements of non-discretionary federal spending, we do explicitly exclude category B79, which consists of obvious non-discretionary spending on public welfare items (e.g., Medicaid).

[16] For instance, following the drop of a chair or ranking member of a Top 3 committee, earmarks are cut back by 33.3% (t=1.72).

Using this transfer measure, we find similar results to those with earmark spending. In particular, a seniority shock results in a 8.7% (t=2.54) increase in total federal transfers to the state. Since the average (median) state receives $3.7 billion ($2.4 billion) in transfers, this translates to an increase of roughly $322 million ($211 million) per year in federal transfers.

We also aggregate up government procurement contracts from 1992-2008, provided by the company Eagle Eye, at the state-year level in order to create an additional state-level measure of discretionary spending. Using this annual state-level measure of government contracts, we again find similar results to those with earmark spending. In particular, a seniority shock results in a 23.5% (t=2.33) increase in total government contracts to the state. Since the average (median) state receives $2.3 billion ($840 million) in contracts in 2008 dollars, this translates to an increase of roughly $534 million ($197 million) per year in government contracts.

B. Seniority and Corporate Retrenchment

Having shown that the ascension of a state's congressman to a powerful committee chairmanship leads to a large increase in federal transfers to that state, we now study the impact this spending has on corporate behavior. Whereas the Keynesian framework suggests these exogenous spending shocks will have a stimulative impact on corporations, the model presented in Section II predicts that firms will retrench. Because the federal transfers result in an increase in the wealth of state residents, it encourages them to substitute from labor to leisure. This lowers the productivity of capital and compels firms to cut back investment and reduce output. To test these predictions, we study a number of measures of corporate investment including capital expenditure, R&D expenditure, payout, and employment decisions. We regress each of these firm-level dependent variables on the state-level seniority shocks as well as a number of firm-level controls. We consider separately positive and negative shocks to seniority as well as shocks to the seniority of Senate and House members.

Our first analysis focuses on the capital expenditure decision of firms. A secondary motivation behind this test is that, in addition to lowering the productivity of private sector capital, the federal transfer itself may structurally substitute for private capital investment. An often cited example of this is the Tennessee Valley Authority's (TVA) construction of electricity plants along the Tennessee Valley in the 1930's. Private enterprises that had planned expansion and provision of service of this same region were forced to decrease investment and to downsize employees. For instance, the nation's largest electric utility holding company entering into the depression, Commonwealth and Southern Corporation, was unable to compete with the TVA in the Tennessee Valley and as a result was forced to decrease investment there, and to eventually dispose of properties in the Tennessee Valley, selling them directly to the TVA for $78.6 million in 1939 (Barnard (1966) and Manchester (1974)).

The regressions in Table IV regress capital expenditures, measured as firm capex scaled by (lagged) firm assets, on Senate seniority shocks and a number of control variables. This represents a reduced form estimation using our shocks to senior chairmanship. We explore in the next section (and in Table V) a two stage least squares estimation (regressing earmarks on seniority in the first stage) that yields the instrumented value of government transfers. The regressions in Table IV include firm and year fixed effects, and standard errors are clustered at the state-year level (e.g., AK-1992).

From Columns 1-7 of Table IV, seniority shocks result in economically and statistically significant declines in firm capital expenditures. Across all measures of seniority, the declines are large and highly significant. For instance, again looking at ShockTop1ChairOnly, the coefficient implies a 1.2% drop in scaled capital expenditures (t=3.46). Since firms have average capital expenditures of 8 percent of assets, Senate chairmanship causes a roughly 15 percent reduction in the representative firm's capex.[17] Including controls in Columns 2-7 has only a modest effect on the magnitude of shocks, and all are still statistically significant. In line with the earmarks results in Table III is the fact that chairmen of more powerful committees have a larger impact on firm capex as well. Lastly, from Columns 8 and 9, again consistent with the reduction in earmarks following the relinquishing of chairmanship, following replacement of the chairman firms

[17] Further, as we demonstrate later in Table V, when we split the sample into above- and below-median sized firms, both groups of firms respond significantly to seniority shocks, and the estimated coefficients are similar, suggesting that our results are not limited to small firms.

in the state partially restore their capex spending, increasing it by 0.6 to 0.7 percent of assets which represents around 8 percent of the average firm's capital expenditures.

C. Robustness: House Shocks, Subsamples, and Pre-Shock Behavior

In Table V, we repeat the capital expenditure analysis, but this time using a series of alternative specifications. First we examine House seniority shocks, as opposed to the Senate seniority shocks used earlier. The House results are statistically strong but slightly smaller in economic magnitude. Columns 1 and 2 of Table V indicate that capital expenditures decline between 0.3 - 0.4 percent.[18] The coefficient of -0.4 percent (*t*=2.26) on ShockTop1ChairOnly, now corresponding to the House Ways and Means Committee, represents a 5 percent reduction the representative firm's capex. The more modest effect might be expected as House members may be more interested in directing funds towards their particular district (as opposed to their state in general). Thus, firms headquartered in other districts within their state may be less impacted by state-level federal spending increases that result from seniority shocks in the House, as opposed to those from the Senate. In Appendix Table A2 we again show firms increasing their capex after their congressman relinquishes his or her chairmanship, consistent with the Senate shock results in Table IV.

In Columns 3 and 4, we repeat the basic capex regression used in Table IV, but now on two different subsamples. In Column 3, we restrict our sample to only those firms above the median in lagged market capitalization each year (computed from our sample), in order to assess if our results are driven entirely by small firms. Column 3 reveals a large and significant effect of Senate seniority shocks on the capex decisions of large firms as well, with the coefficient of similar magnitude to the full sample result (-0.005 vs. -0.006 for the full sample from Table IV Column 5). Next we run the same specification from Table IV Column 5, but this time on a subsample of the Senate shocks that excludes those seniority shocks where the prior chairman lost an election or

[18] Results for the complete set of shock variables shown in Table IV, but for the House, reveal similar patterns as in the Senate, with coefficients declining with the importance of the committee. These results are available in the online Appendix, in Table A2.

primary.[19] Column 4 of Table V indicates that this filter has no effect on our results, as the magnitude of the coefficient on the shock variable (=-0.006, *t*=3.44) is identical to the coefficient reported in Table IV Column 5 (=-0.006, *t*=3.57).

Column 5 of Table V compares the regression coefficients for our main dependent variables of interest prior to and following the shock, but only for the subset of firms that are shocked during the sample period. Column 5 reports the coefficient produced if the shock variable (Shock_Top3Chair&Rank) is turned on for the six years following the appointment, as in Tables III-VII, as well as the coefficient when the variable is turned on during the six years *preceding* the appointment (Pre-Shock). The shock coefficient is similar to that reported earlier, while the pre-shock coefficient is insignificant and essentially zero in magnitude. Thus we find no evidence that firms anticipate and prepare for changes in committee chairmanship.

Finally, we explore the subsample for which we have earmark data (1991-2008). Column 6 indicates that the coefficient on Shock_Top3Chair&Rank in the more recent sub-period is actually somewhat larger in point estimate (-0.007, *t*=4.63) than the full sample result shown in Table IV (-0.006, *t*=3.57).[20]

D. Instrument of Committee Chairman Shocks

In this section we discuss our econometric approach in greater depth. We have in mind the following econometric model:

$$y = \alpha + \beta_1 Government\ Spending + \vec{\beta}_2 \vec{X} + \vec{\beta}_3 \overrightarrow{Time\ FE} + \vec{\beta}_4 \overrightarrow{Firm\ FE} + \epsilon \quad (6)$$

$$y = firm\ action\ (e.g., capital\ expenditures)$$
$$\vec{X} = vector\ of\ firm\ controls\ (e.g., leverage, Tobin's\ Q, etc.)$$

The problem, of course, is that government behavior is affected by private sector economic activity, making it difficult to identify the *effects* of government spending from

[19] Our sample consists of 232 seniority shocks to a Top 10 committee chair or ranking minority seat, 115 from the Senate and 117 from the House. More than half of these Top 10 shocks were due to the death or resignation of the prior chairman, and only 17 of these shocks in the Senate (and 19 in the House) involve a lost election by the prior chair or ranking member.

[20] For all firm-level specifications in the paper we have also clustered our standard errors at the state level and state-shock period level, in addition to the state-year level we report here, and the results are similar to those reported here. For instance, for the specification we report above in Column 6 of Table V, the SEs clustering by state and state-shock period level imply t-stats of 4.01 and 4.73, respectively.

the factors that cause that government spending to change. In other words, government spending is correlated with omitted (and unobservable) variables in the error term, ε in (1). As disentangling this is the key to gaining insight into the impact of government spending on private sector activity, some kind of exogenous variation in government spending is needed. As noted above, the instrument we use is the appointment to powerful committee chairmanship in the Senate and House. Our choice is motivated by the fact that one's appointment to committee chairmanship is based almost entirely on seniority within the chamber (see Polsby et al. (1969)).[21] Thus, the only way for a congressman to be appointed the chairman of a committee is for the current chairman to relinquish the chairmanship: either through that chairman's election defeat, resignation, death, or through the chairman losing party-control of that chamber of Congress. Since all of these events depend largely on political circumstances or events in *other* states, a congressman's ascension to a powerful committee chair creates a positive shock to his or her state's share of federal funds that is virtually independent of his or her state's economic conditions. We have shown in Table III that this shock results in economically large and significant government spending transfers to the new chairman's state.

In the absence of an instrument, the direction of the expected corporate response is unclear; the government may choose to send money to struggling areas, populated by firms with relatively poor investment opportunities, or the government may try to maximize spending-impact by sending capital to regions with especially good investment opportunities. Thus, in terms of (1) the error term (ε) is made up of two components ($\epsilon = \theta + \eta$), where θ is the endogenous piece correlated with government spending, and η is the piece uncorrelated with all regressors (including spending). As mentioned above, θ may be positively or negatively correlated with government spending, as the government's objective function may include investment into predicted high investment

[21] As mentioned in Footnote 1, the seniority system has been the prevailing determinant of committee chairmanships since the early days of Congress in both chambers. Although occasional deviations from seniority-based chairmanship do appear in the data (see Deering and Wahlbeck (2006) for a discussion), our results are very similar if we use changes in the identity of the most-senior committee member in place of changes in committee chairmanship. For example, the coefficient in column 3 of Table IV (=-0.008, t=2.94, using changes in chairmanship) is virtually identical (=-0.008, t=3.11) when we instead use changes in the identity of the most-senior committee member to define our shocks. This fact holds true across all shock variables, and across all outcome variables (i.e., earmarks, capex, R&D, payouts, employment changes, and sales growth).

opportunities, or low investment opportunities areas.

To illustrate this endogeneity between government spending and private sector economic activity, and the problems this can cause for identification, in Column 7 of Table V we simply regress capital expenditures on earmark spending. From Column 7, the regression exhibits no relationship, with the coefficient on earmarks being small and statistically zero (t=0.55). However, when we use the instrumented value in Column 8, which consists solely of the portion of earmarks that is related to seniority shocks (the first stage is given in Table III),[22] the strong negative relationship returns. Comparing this to the analogous reduced-form specification in Column 6 in Table V (computed over the same time period), the IV estimate here of –0.008 (t=6.28) implies even a slightly larger point estimate than the reduced form, and is a more reliable estimator of the impact. Thus, the results in Columns 7 and 8 of Table V suggest that a significant portion of the variation in earmark spending is, indeed, determined endogenously in the context of corporate investment behavior (with the positive investment opportunities being quite important), and breaking this endogeneity is critical to making the correct inference regarding the impact of government spending on firm behavior.

Even though we obtain stronger results in magnitude when we use the instrumented values for earmark spending (Table V, Column 8), we use the reduced form shocks for most of the tests performed in the paper. The reason we prefer these is that we have data on the shocks going back to the late 1940's, as opposed to only 1991 for the earmark data, so we get a richer period of time and events (more changes in committee seniority and chairmanship, more investment decisions by firms, etc.) to examine the relationship between government spending and the behavior of firms.

E. Research and Development Investments and Payouts

We next examine other firm behaviors that may be affected by a firm facing a different set of investment opportunities following a government spending shock. Specifically we look at both R&D spending and payout decisions of firms, where payouts

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[22] The marginal F-stat of the corresponding seniority shock instrument from Table III, Column 7 is 9.76 ($p<0.01$).

are defined as share repurchases plus dividends. The model in Section II suggests that, because the productivity of capital – both physical and intellectual – has been reduced, companies should reduce their investments in physical capital and R&D and, to the extent that they are left with unused funds, remit them back to shareholders.

We present these results in Table VI with firm-level R&D in Panel A, and payout decisions in Panel B, with both scaled by lagged assets (as in the capital expenditure tests). Again we include firm level controls, in addition to firm and year fixed effects. Consistent with firm capital expenditure behavior from Tables IV and V, Panel A of Table VI illustrates that seniority shocks result in material reductions in R&D investment. Specifically, looking at House and Senate shocks,[23] from Columns 1 and 4, Senate and House seniority shocks results in a reduction in R&D spending of between 0.5-0.9% (t=2.64 and 3.31) per firm. Since the average firm R&D is 7.3 percent of assets, the impact is non-trivial in economic terms (a roughly 7-12 percent scaling back of R&D). In Appendix Table A3 we again provide corroborating evidence that upon the departure of the committee chair, R&D spending is restored.

Panel B then examines the effect on payout decisions. If public firms are crowding out the investment opportunity sets of firms, we might expect firms to respond to this reduced investment opportunity set by (investing less and) paying more out to shareholders. This is precisely what we see in Panel B of Table VI. Following a seniority shock, we see payouts significantly increasing. Column 1 reveals that payouts increase after seniority shocks by 0.3% (t=4.15); for our firms, payout averages 2.3 percent of assets, so this represents a 13% increase in payouts. The results are similar after House shocks (with the effect ranging between 0.1-0.3%). Appendix Table A3 indicates that this effect again appears to reverse following the congressman's departure.

Consistent with the capital expenditure results shown in Tables IV-V, we find that spending shocks have a slightly larger effect in the most recent sub-period (1991-2008), for both R&D and payouts. Further, the pre-shock coefficients are again insignificant and essentially zero in magnitude for both R&D and payouts, indicating that firms do

[23] For brevity here, we only report Shock_Top1ChairOnly and Shock_Top3Chair&Rank committee shocks. The results for other measures, as in Tables IV, are both stronger for ChairOnly, and gradually weaken as we allow in relatively less powerful committees, which is also consistent with the estimated impacts on earmark appropriations from the relative power of the different committee chairmanships.

not anticipate spending shocks.

F. Firm Level Employment and Valuation Consequences

In our next set of tests, we examine the impact of shocks to congressional seniority on firm-level employment. To the extent that federal transfers cause individuals to perceive a wealth increase and therefore increase their leisure allocation, employment levels will necessarily decline. Moreover, to the extent that the government increases its hiring of state employees, it may compete for skilled (and perhaps specialized) labor and thereby reduce the labor pool for the private sector.

We test this prediction of the impact of seniority shocks on firm-level employment growth in Panel C of Table VI. Panel C illustrates a modest effect of seniority shocks on firm-level changes in the number of employees. In Columns 1 and 2, the sign is negative for Senate shocks (and also for the rest of the unreported shocks), but the effect is only significant for Shock_Top3Chair&Rank (=-0.011, t=2.41). House seniority shocks have a consistently negative and significant effect on firm-level changes in employment. The magnitudes of the coefficients ranges between -0.6% to -2.7%, implying that corporations scale down their employment growth rates by 3-15%. For both the House and the Senate, Columns 6 and 7 demonstrate that the effects are stronger in the second half of the sample period (1991-2008): the coefficients on Shock_Top3Chair&Rank during this period are -0.025 (t=3.06) and -0.020 (t=2.64), respectively. And once again, the zero coefficient on the pre-shock variable suggests a lack anticipation on the part of firms.

Although our evidence thus far identifies corporate retrenchment in response to federal spending shocks, the valuation consequences of these shocks for public corporations remains somewhat ambiguous. In particular, it is conceivable that, although firms cut capex, R&D, and to a lesser extent employment (and meanwhile increase payouts to shareholders), the federal spending shocks generate spillovers from which they benefit. Typically, endogeneity concerns lead researchers to study the valuation consequences of such shocks by examining share price responses. Unfortunately, such event-study market price tests lose power rapidly in settings where the event window is necessarily wide because the precise timing of the event is poorly known. Our setting likely requires a window on the order of several months to a year, because spending shocks are revealed gradually as the probability of a given congressman's ascension evolves with changes in polling data about election outcomes and factors influencing incumbent retirement.

An alternative is to examine accounting measures directly. Because of our reliance on a clean instrument for federal spending shocks, we can directly infer the causal effect of increased government spending on accounting measures of corporate welfare. In Panel D of Table VI, we present regressions of sales growth on our seniority shocks.[24] The results suggest that a seniority shock causes firm sales growth to retract 1.4 to 5.4 percent per year during the subsequent six years relative to non-shocked firms and periods. However, these results are only statistically significant for the House shocks and for the Senate Shock_Top3Chair&Rank specification; thus we interpret these results as less strong, yet still suggestive evidence of possible negative valuation consequences for the public firms that operate out of states that are recipients of federal government transfers. As with the capex, R&D, payout, and employment results, the negative impact on sales growth is more pronounced in the second half of the sample: the coefficients on Shock_Top3Chair&Rank for the House and Senate during the 1991-2008 period are -0.042 (t=3.55) and -0.030 (t=2.90), respectively.

V. Mechanism and Robustness

A. Geographic Concentration

In this section we begin to explore the mechanism at work behind our results. We start by testing an additional implication of our central finding that government spending can crowd out private sector economic activity. Specifically, the effect of government spending shocks on firm behavior should be larger for those firms with concentrated operations in a single state; i.e., firms that cannot shift investment out of state, and that have more difficulty accessing inter-state capital, land, and labor markets. In Column 1 of Table VII we exploit variation in firm-level geographic concentration by re-running our basic capex specification (Column 2, Table IV) on the subsample of firms that operate

[24] We have run similar tests where we replace sales growth in these regressions with return on assets (a measure of profitability), and find generally negative (but insignificant) effects of seniority shocks on profitability.

entirely in a single state.[25] Column 1 indicates that the coefficient on Shock_Top1ChairOnly (=-0.031, *t*=2.20) is almost three times larger for single-state firms than for all firms. We have also run these same regressions for our other outcome variables (i.e., R&D, payouts, employment, and sales). The effect of government spending shocks is greater for each of these. For instance, the coefficient on Shock_Top1ChairOnly for R&D expenditures in the most geographically concentrated firms is -0.015 (*t*=2.57), again three times as large as for all firms. Thus, firms with more limited ability to shift their operations to other countries or states are more compelled to reduce their capital expenditures in response to government spending shocks.

B. Factor Slack: Heterogeneous Effects by Economic Conditions

In our next set of tests we explore the particular firms for which (and conditions under which) we might expect state-level increases in federal spending to have a more pronounced effect on corporate behavior. From a Keynesian perspective, the stimulative capabilities of government spending should be largest when there are vast amounts of underutilized resources in the economy. And although the model developed in Section II does not explicitly allow for this, one would expect the impact of shocks to wealth to be muted if labor market frictions have created conditions where individuals would like to allocate more to labor than they are able to (e.g. when unemployment is high). Similarly, the response should also be muted if capital market frictions have left firms with physical capital that they cannot divest (e.g. capacity utilization is low) and therefore investment rates that are low. To test this, we examine whether firms operating under conditions of low unemployment or with high capacity utilization are those for whom spending shocks have a larger negative effect on employment and investment.

We use a capacity utilization measure collected by the US Federal Reserve for industries in manufacturing, mining, and electric and gas utilities. The measure, which is available on their website,[26] captures each industry's seasonally-adjusted output level relative to its maximum sustainable level of output. The latter value, which is measured

[25] We use the data from Garcia and Norli (2010), and thank Diego Garcia and Oyvind Norli for providing us with their data on state-level operations of public firms.
[26] http://www.federalreserve.gov/releases/G17/About.htm

at the plant level, is the maximum level of output the plant can achieve under a reasonable work schedule and with sufficient availability of inputs to operate the capital in place. We then regress firm-level scaled capital expenditures on the seniority shock as well as an indicator variable that identifies firms operating at a time when their industry's capacity utilization is at or above the median capacity utilization across all firms in a given year (*High Util*).

The regression in Column 2 of Table VII confirms the basic negative relation between seniority and capital expenditures, but the interaction term between shock and high capacity utilization is negative and significant. This suggests the crowding out of private investment is particularly pronounced in industries operating at a high level of capacity where firms have plenty of room to lower their rate of investment and competition for additional factors of production including facilities and specialized capital is expected to be strong.

Next, we explore if shocks to wealth the government's hiring of skilled labor may be especially harmful to private firms when there is scarce employable labor (a notion of full employment), while conversely, with slack in the labor market, government hiring shocks may have an attenuated effect. To investigate whether the crowding out of corporate employment is particularly pronounced when unemployment is low, we split seniority shocks into those that occur when the difference between a state's unemployment rate and the national unemployment rate is below its historical difference (i.e., times of low unemployment in the state) and those that occur when it is above (i.e., times of high unemployment in the state). As Table VII reports (in Column 3), the coefficient on the main effect, which measures the response of firms in states during high unemployment times, is actually positive (albeit insignificant). Meanwhile, for firms in states during low unemployment times, the interaction term is -0.024 (t=2.17) larger, which indicates that the negative impact of seniority shocks on corporate employment is concentrated at times when the supply of employable labor is scarce. This result can be interpreted as providing evidence consistent with the view that government stimulus crowds out private sector employment when the economy has little slack in the labor market, but does not when the economy is experiencing significant slack in the labor market.

We extend this idea further by examining interactions of seniority shocks with state- and national-level measures of growth in real GDP. In Columns 4 and 5, we interact seniority shocks with state-level real GDP growth (drawn from the Bureau of Economic Analysis (BEA) and extending back to 1967), and find that this interaction term is negative and significant for both capex and employment. The interaction term in Column 4 can be interpreted as follows: at times when state-level real GDP is one-standard deviation higher than normal, seniority shocks reduce capex by almost twice as much as normal. Column 5 indicates that at times when state-level real GDP is one-standard deviation higher than normal, seniority shocks lead to decreases in firm-level employment of 1.3%.

In place of this continuous measure of GDP, we also include a specification that splits seniority shocks into those that occur when the difference between a state's real GDP growth rate and the national real GDP growth rate is above its historical difference (i.e., times of high state-level real GDP growth), and those that occur when it is below (i.e., times of low state-level real GDP growth). Column 6 and 7 indicate that for both capex and unemployment, the impact of seniority shocks is larger when state-level GDP growth rates are higher. For example, for employment, the negative effect of spending shocks is concentrated entirely at times when state-level GDP growth is high.

We also interact the spending shocks with measures of *national* GDP growth, as opposed to state-level measures of growth, as some types of labor and capital may be more mobile across states, but still face international frictions. Specifically, we create a dummy variable called High US GDP growth, equal to one if the US real GDP growth rate is higher than its historical average, and then interact this dummy variable with our spending shocks. Columns 8 and 9 indicate that these interaction terms are again negative and significant for both capex and employment, although they are somewhat weaker (particularly for capex) than the state-level GDP interactions. Taken as a whole, the evidence in this section suggests that the firm-level retrenchment we document in this paper in response to government spending shocks is most pronounced when economic conditions are relatively strong and resources are being fully utilized.

C. Timing and Robustness of Corporate Response

We have performed a variety of additional robustness checks on our main results, and in this section we highlight two such checks. First, although the results in Tables III-VII define seniority shocks as applying for 6 years, in Appendix Figure A1 we plot results for shock durations of 1-10 years. We find that for capex and corporate payouts, the firm-level response is rapid: the adjustment that occurs during the first two years following the shock captures most of the long-run effect. On the other hand, the employment and R&D adjustments appear more gradual, with very little of the long-run adjustment occurring during the first year following a shock. This suggests that firms can more easily alter capital expenditures and payouts, as opposed to longer-term commitments like R&D and labor.

We also examine whether our results hold up when we consider each state separately in our regressions and then evaluate the average coefficients produced. This approach effectively treats all observations in a given state as a single observation. To the extent that our results thus far are driven disproportionately by the firms of a few large states, this specification will severely limit their ability to impact our results. Also, to the extent that a large amount of correlation exists within states in the investment, R&D, payout, and employment decisions, this specification will conservatively consider these decisions to be effectively perfectly correlated with one another. Thus we are sacrificing a large amount of power to get an alternate estimate of our effects. In Appendix Table A4 we report the cross-sectional average of the state regression coefficients and the associated test statistic against a null that the average coefficient is zero. Overall, the results are remarkably similar to those reported earlier. Seniority shocks lead to a 2.6 percent decline in capex, a 0.3 percent decline in R&D, a 9.1 percent decline in employment growth, a 13.0 percent decline in sales growth, and a 0.4 percent increase in total payouts. Considering that we include any of the top 10 committees in this specification, the economic magnitudes here are generally larger when states are treated as single observations. A non-parametric test that asks whether the fraction of states with coefficients of the predicted sign is significantly different from 0.5 is rejected at the one percent level for four of the five dependent variables, again reported in Appendix Table A4.

VI. State-Level Outcomes

Up to this point, we have focused on firm-level responses, rather than state-level measures of economic activity such as GDP or employment. The firm-level unit of analysis offers several advantages over state-level aggregates. First, firm-level data are inherently more accurate than state-level aggregates. Our panel of capital and R&D expenditures, payout decisions, and employment changes is captured from the audited financial statements from over 16,000 publicly-traded firms over a 42-year time period. In contrast, state-level aggregates, which often rely on survey data, present noisy measures of economic activity. As a consequence, our approach produces a panel with substantial time-series and cross-sectional variation in relatively stationary dependent variables that delivers ample power--even when standard errors are adjusted to allow for correlation across firms or time--to accurately assess the impact of spending shocks. Because the number of state-year cells is large, clustering will correctly account for the degree of intra-class correlation. So long as intra-class correlation is less than one, conducting analysis at the firm level will offer additional statistical power relative to simply collapsing the dataset at the state-year level. In addition, by examining the impact of spending shocks on the average firm in each state, we can directly control for a variety of characteristics known to influence firm spending and hiring decisions and thereby accurately gauge the effects of government spending on individual firms, rather than on a single firm (or set of firms) that may dominate a state-level aggregate measure.

Nevertheless, our firm-level analysis is inherently restricted to the data provided by publicly traded corporations. It remains an open question as to whether our results extend to all private sector businesses and to the economy as a whole. To pursue this, we extend our analysis to state-level measures of economic activity drawn from the BEA. We focus on three key state-level indicators: GDP (from 1964-2008), employment (from 1969-2008), and personal income (from 1958-2008). Similar to our earlier earmark regressions, Appendix Table A5 presents state-level regressions of log(GDP), log(Employment), and log(Personal Income) on our spending shock measure (Shock_Top1ChairPlusRank). We also examine the following industry breakdowns for each measure of activity: total, private, manufacturing, construction, transportation, wholesale trade, retail trade, finance and real estate, and services.

We find that spending shocks lead to declines in state-level GDP, employment, and personal income. The statistical significance of these findings is mixed, but the economic magnitudes are nontrivial: e.g., seniority shocks lead to a 5.2% decline in GDP, a 4.8% decline in employment, and a 5.8% decline in personal income. The responses are particularly negative across all three indicators for several sectors, most notably manufacturing, construction, and retail trade. Thus the state-level evidence is consistent with our earlier findings in Tables IV-VI on the response of publicly-traded firms to spending shocks.

VII. Discussion

The central finding of this paper is that positive shocks to the seniority of a state's congressional delegation cause large and persistent increases in government allocated funding to the states, *and* significant retrenchment on the part of the corporations headquartered in the state. This retrenchment appears to be a response to the large and persistent increase in federal funding that the state receives following the shock. Following the appointment of a senator to the chair of a powerful committee, we estimate that his state experiences, on average, a 40-50 percent increase in its share of congressional earmark spending, a 9-10 percent increase in its share of total state-level government transfers, and a 24 percent increase in its share of government procurement contracts. At the same time, firms residing in the state cut their capital expenditures by 8-15 percent, reduce R&D by 7-12 percent, and increase payout by 4-13 percent. Employment and sales growth are also impacted, as corporations scale back employment growth by 3-15%, and sales growth falls by up to 15%.

Our results demonstrate that the *average* firm retrenches in the face of government spending shocks, but it is certainly possible (and perhaps likely) that some individual firms do in fact benefit from these spending shocks. The incidence of lobbying by firms certainly suggests that firms perceive some possible benefits from currying favor with politicians (see Roberts (1990), Jayachandran (2006), Goldman et al. (2007, 2008), Faccio (2006), Faccio et al. (2006), Faccio and Parsley (2006), and Fisman (2001) for evidence linking politicians and political connections to firm benefits).[27] While we have much less power to detect benefits to individual firms using our approach (which relies on state-level seniority shocks that apply to all firms in a given state), we do find some indirect evidence that certain specific firms that we can identify as having received valuable contracts from the government are less likely to retrench in the face of seniority shocks. Using the sample of procurement contracts discussed in Section IV, but restricting the data to the contracts we can specifically match to individual firms, we re-run our baseline capex regressions from Table IV, but also interact the seniority shock variable (Shock_Top1ChairOnly) with a dummy for whether or not the firm in question received a government procurement contract at some point earlier in our sample. The magnitude of this interaction term (=0.009, t=3.12) is equally large (but of opposite sign) as the main effect on the shock variable (=-0.010, t=6.27), suggesting that firms that have received government procurement contracts do *not* cut back in response to government spending shocks.

A related question can be posed of the voters: If electing a powerful senator is so bad for jobs -- even state-level aggregate employment appears to decline -- what benefit do the voters perceive in continuing to elect him? There are several possibilities here. First, the jobs created from federal transfers are generally much easier to identify and quantify than those lost -- indeed, Senators often tout the number of jobs that their earmarks have been able to create in their home states. Identifying and measuring those that have been lost is not as easy. When a firm shuts down because labor costs have become prohibitive, it can never be cleanly tied to the wage pressure produced by federal transfers. Moreover, because federal transfers are endogenous, they may appear to create jobs even if they are merely targeting states where jobs will be created. Finally, it is worth noting that employment does not necessarily mean utility. Even in the simple model presented in Section II, employment declines while utility -- through increased consumption and leisure -- increases. It remains an open question as to whether the residents of states such as West Virginia have seen their utility increase or decline during an era of large federal transfers and modest employment levels.

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[27] For counter-evidence, see Fisman et al. (2007), who find the value of corporate ties to Richard Cheney to estimated to be new zero (albeit with a large standard error).

A key feature of our results is that they show up under a variety of specifications, many of which offer essentially independent tests of the main result. First, firm responses to seniority shocks in the Senate are confirmed when we look at those in the House instead. This test gets its power from the fact that the House and Senate shocks are nearly a non-overlapping set of state-year shocks. Second, we find evidence that this behavior is partially reversed when the congressman relinquishes the chairmanship. Third, our coefficients are essentially identical whether we include state or time fixed-effects or when we include other regressors known to account for variation in firm spending, payout, and employment decisions. Fourth, our results show up when we simply take cross-state averages of within-state time-series regression coefficients. Finally, restricting our analysis solely to those firms that receive shocks, we see zero anticipatory effect of any firm behavior in the years prior to the shocks, and large, significant changes in behavior thereafter. Taken together, these results suggest a link between congressional seniority shocks and corporate behavior that is not confined to particular points in time or driven by observations in a handful of states.

Our findings also include a number of results that corroborate the link between congressional spending and corporate retrenchment. First, the link grows weaker as we broaden our definition of what constitutes a powerful committee. The results are also generally weaker (in economic terms) in the House than the Senate, which one would predict given the fact that a congressional representative may have less impact on federal spending directed towards other districts within his state. Relatedly, we show that congressional spending has more impact on firms that operate solely within the state affected by the spending shocks.

The magnitude of this private sector response is nontrivial. For the period over which we have earmark and federal transfer data (1992-2008), the median state receives \$44 million in earmarks, \$211 million in transfers, and \$197 in government contracts as a result of a seniority shock. Meanwhile, over this same time period, capex and R&D reductions in the median state total \$48 million and \$44 million per year, respectively, while payout increases total \$27 million per year.[28] Taken alone, these magnitudes suggest

[28] We compute these state-level figures by calculating the average difference in scaled capex (or scaled

that a fairly high degree of complementarity between capital and labor in the production function. Importantly, however, they should not be interpreted as a precise estimate of the fiscal multiplier, the computation of which is a task beyond the scope of this paper. Since our estimates ignore the effect of government spending on private (non-publicly-traded) firms as well as on household consumption and they ignore the impact of other types of spending (e.g., other federal grants, defense spending, etc.), the fiscal multiplier implied by our results may not be comparable to the overall fiscal multiplier estimated elsewhere in the literature.

Our results beg the question of what mechanism causes firms to respond so negatively to state-level federal spending increases. What is essentially a transfer of funds from the residents of one state (i.e., the state of the relinquishing committee chair) to another (i.e., the state of the ascending committee chair) causes retrenchment in the corporations that serve and employ the residents of the recipient state. Since our results focus on reallocations of federal spending rather than increases thereof, we can rule out the standard interest rate and tax channels that have occupied the literature to this point. Consistent with the model, some of our results point towards the role of competition for state-specific factors of production, including labor and fixed assets such as real estate. Public spending appears to increase demand for state-specific factors of production and thereby compel firms to downsize and invest elsewhere. In particular, our capex results are stronger for firms with high capacity utilization, and our employment results are stronger when employment rates are at or above their long-term state-specific averages. Further, both our capex and employment results are stronger when state- and national-level real GDP growth rates are high. Thus, when slack exists in factories or the labor market, federal dollars do not appear to be as large of a deterrent to corporations in terms of investing or hiring. In unreported results, we also find evidence that the effects are most pronounced in sectors that are the target of earmark spending.

The ability of our results to speak to the net impact of national-level spending shocks on national-level economic outcomes depends critically on whether states are similar to national economies. To the extent that states are small open economies, any

R&D, or scaled payouts) between shocked and non-shocked firms in a given state over all firm-years, and multiplying that difference by the average of the yearly state-level sums of lagged assets.

"leakage" that blunts the impact of fiscal stimulus as firms shift investment to other states may be unique to the state-level results and limit the conclusions one can draw regarding fiscal policy in a large, national economy. However, a key feature of our data is that firm-level figures reflect capex, R&D, payout, employment, and sales growth aggregated across *all* operations of the firm, including divisions located in other states. This means that our results are calculated net of any within-firm shifts in resources and activity that occur in response to the spending shock. Thus, our results are only subject to leakage that occurs when investors in firms headquartered in the target state reallocate capital through their portfolios to other firms located in other states. To the extent that portfolio capital has greater mobility across states than across countries, the impact of fiscal stimulus may be weaker at the state level than the national level.

VIII. Conclusion

This paper provides a new empirical approach for identifying the impact of government spending on the private sector. Using changes in congressional committee chairmanship as a source of exogenous variation in state-level federal expenditures, we find that fiscal spending shocks lead to significant retrenchment on the part of corporations headquartered in the target state. Specifically, we find statistically and economically significant evidence that firms respond to government spending shocks by: i.) reducing investments in new capital, ii.) reducing investments in R&D, and iii.) paying out more to shareholders in the face of this reduced investment opportunity set. Further, we find that when the spending shocks reverse (through a relinquishing of chairmanship), most all of these actions are reversed. Finally, we also find some evidence that firms scale back their employment, and experience a decline in sales growth in response to these government spending shocks. Taken as a whole, our findings are consistent with the predictions of neoclassical theory.

Our findings demonstrate that new considerations – quite apart from the standard interest rate and tax channels – may limit the stimulative capabilities of government spending. Whether they are sufficient to materially lower the multiplier on fiscal stimulus in a large economy such as the US remains an open but important question.

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Figure I: State-Level Annual Earmarks Versus Population

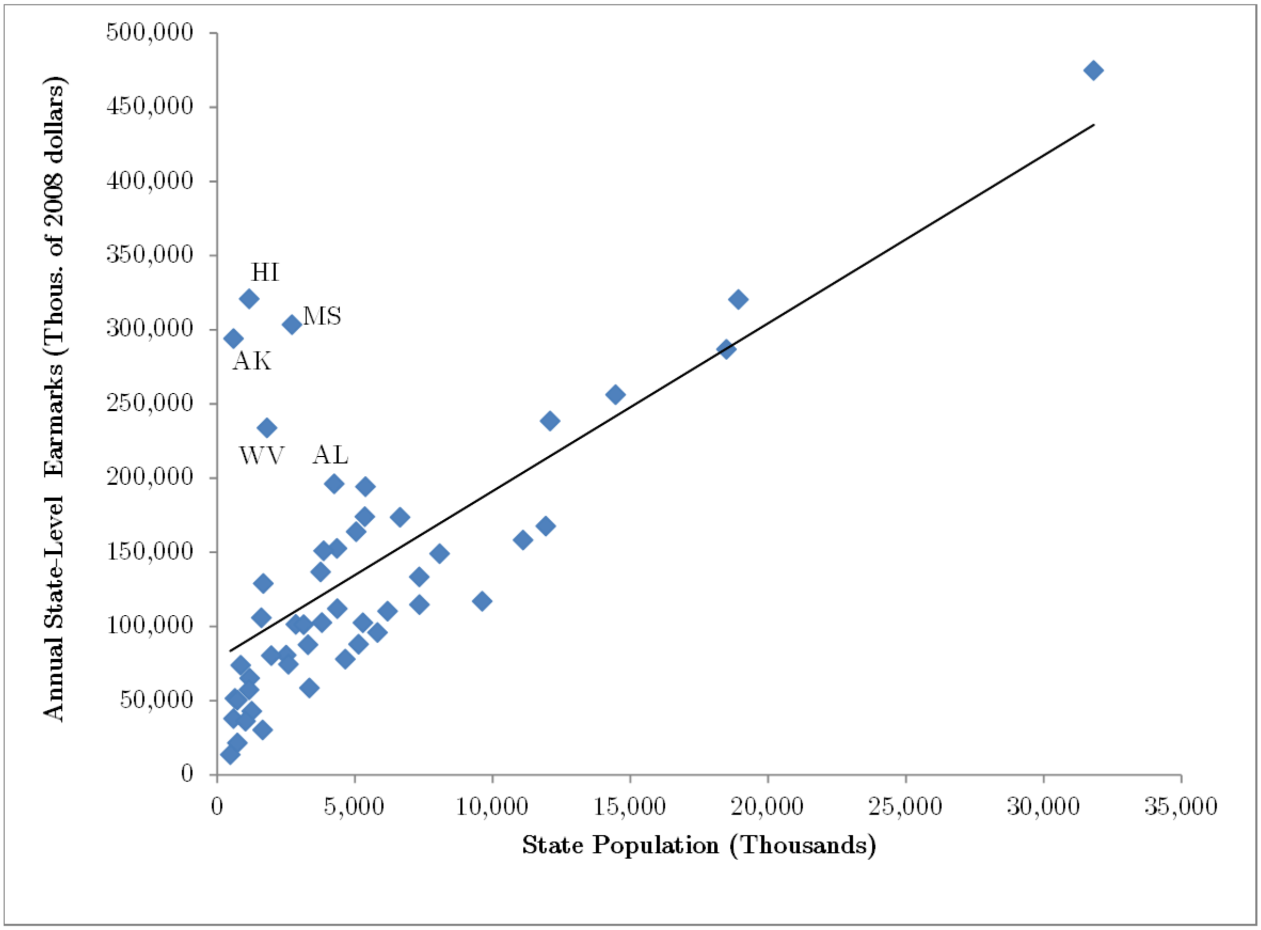

Table I: Summary Statistics

This table reports summary statistics for the sample. Seniority shocks are defined as follows: Shock Top1ChairOnly is a dummy variable equal to 1 if the senator (or representative) of a given state becomes chairman of the Senate Finance Committee (the House Ways and Means Committee); Shock Top1Chair&Rank is equal to 1 if a senator becomes either chairman or the ranking minority member of the committee. The list of the top 10 most influential committees is from Edwards and Stewart (2006); for the Senate these committees are Finance, Veterans Affairs, Appropriations, Rules, Armed Services, Foreign Relations, Intelligence, Judiciary, Budget, and Commerce, and for the House these committees are Ways and Means, Appropriations, Energy and Commerce, Rules, International Relations, Armed Services, Intelligence, Judiciary, Homeland Security, and Transportation and Infrastructure. Seniority shocks begin in the year on appointment, and are applied for 6 years. All accounting variables are winsorized at the 1st and 99th percentiles of their distributions. The earmark data is from 1991-2008, the transfer data is from 1992-2007, and the contract data is from 1992-2008. All dollar figures are in 2008 dollars.

Panel A: Firm-Level Annual Variables	Years 1967-2008, Firms = 16,734			
	Mean	Median	Std. Dev.	Observations
Capital Expenditures/Assets$_{-1}$	0.078	0.048	0.108	168,975
Total Payout/Assets$_{-1}$	0.023	0.006	0.044	154,832
R&D/Assets$_{-1}$	0.078	0.028	0.134	86,870
ChgEmployees	0.085	0.026	0.322	158,230
Cash Flow/Assets$_{-1}$	0.036	0.084	0.242	151,482
Leverage$_{-1}$	0.416	0.399	0.261	159,833
Tobin s Q$_{-1}$	1.822	1.230	1.826	153,348
Assets (in $ mil)	2,845	194	26,667	168,970
Shock_Top1ChairOnly	0.030	0	0.171	168,975
Shock_Top1Chair&Rank	0.032	0	0.177	168,975
Shock_Top3ChairOnly	0.044	0	0.204	168,975
Shock_Top3Chair&Rank	0.070	0	0.255	168,975
Shock_Top5ChairOnly	0.062	0	0.242	168,975
Shock_Top5Chair&Rank	0.118	0	0.322	168,975
Shock_Top10ChairOnly	0.098	0	0.297	168,975
Shock_Top10Chair&Rank	0.196	0	0.397	168,975
Drop_Top1ChairOnly	0.019	0	0.136	168,975
Drop_Top3ChairOnly	0.022	0	0.146	168,975
Shock_Top1ChairOnly (House)	0.037	0	0.188	168,975
Shock_Top1Chair&Rank (House)	0.100	0	0.300	168,975
Shock_Top3ChairOnly (House)	0.074	0	0.261	168,975
Shock_Top3Chair&Rank (House)	0.207	0	0.405	168,975
Shock_Top5ChairOnly (House)	0.113	0	0.317	168,975
Shock_Top10ChairOnly (House)	0.180	0	0.384	168,975

Panel B: State-Level Annual Variables	Years=1991-2008, States=50			
	Mean	Median	Std. Dev.	Observations
Total Earmarks (in $)	139,027,804	91,213,011	145,481,289	889
Ln(Total Earmarks)	18.17	18.33	1.25	889
State Population	5,327,111	3,665,228	5,811,533	889
Ln(State Population)	15.00	15.11	1.01	889
State Area (in square miles)	72,694	56,276	87,559	889
Total State Govt. Transfers (in $ mil)	3,703.1	2,424.9	4,474.5	800
Log(Total State Govt. Transfers)	21.61	21.61	0.88	800
Total Government Contracts (in $ mil)	2,272.7	839.7	3776.0	849
Log(Total Government Contracts)	20.10	20.55	2.20	849

Table II: Average Annual Earmarks By State

This table reports average annual earmarks by state, for the period 1991-2008. Earmark figures are in 2008 dollars. Population figures for each state are obtained from the 1990 and 2000 census. Total firms, average number of firms per year, average total capital expenditures per year (in millions of 2008 dollars), and average total corporate employees per year (in thousands), are from Compustat and are yearly averages by state over the full sample period (1967-2008). The shock variables are for the Shock Top3Chair&Rank specification, and are averages by state over the full sample period (1967-2008).

Earmark Rank	State	Annual Earmarks	Population	Pop. Rank	PerCap. Earmarks	Total Firms	Avg Firms	Capex	Num. Emp.	Senate Shock	House Shock
1	CA	474,744,643	31,815,835	1	14.9	3111	651.4	48,787.3	2,445.6	0	0.3571
2	HI	320,872,527	1,159,883	41	276.6	25	8.5	596.2	21.1	0.0976	0
3	TX	320,369,604	18,919,165	2	16.9	1643	402.9	79,670.3	2,303.5	0.1429	0.3095
4	MS	303,468,441	2,708,937	31	112.0	58	12.3	318.6	20.0	0.2381	0.2857
5	AK	294,110,808	588,488	48	499.8	8	1.7	90.8	2.3	0.4103	0
6	NY	286,856,506	18,483,456	3	15.5	1872	472.1	60,058.6	3,525.9	0.1429	0.4286
7	FL	256,255,682	14,460,152	4	17.7	936	204.1	10,845.5	707.8	0.1429	0.2381
8	PA	238,567,184	12,081,349	5	19.7	675	192.7	18,970.4	1,192.7	0.2857	0.2857
9	WV	233,943,573	1,800,911	35	129.9	27	6.0	144.0	9.2	0.2439	0
10	AL	196,160,333	4,243,844	23	46.2	92	24.9	1,494.8	93.9	0.1429	0
11	WA	194,234,223	5,380,407	15	36.1	286	62.6	5,808.8	285.5	0.1429	0
12	MO	174,110,006	5,356,142	16	32.5	232	70.3	7,881.7	670.2	0	0
13	VA	173,683,577	6,632,937	12	26.2	428	103.8	17,066.5	779.9	0	0.1429
14	IL	167,702,049	11,924,948	6	14.1	728	208.5	39,578.9	2,837.9	0	0.2857
15	MD	163,912,303	5,038,977	19	32.5	343	79.3	5,046.9	480.8	0	0
16	OH	158,257,270	11,100,128	7	14.3	521	169.3	20,429.4	1,804.9	0	0.0238
17	LA	152,659,937	4,344,475	22	35.1	99	27.4	4,109.6	75.2	0.1463	0.3415
18	KY	151,019,194	3,863,533	24	39.1	92	23.9	2,022.4	213.5	0	0.1429
19	NJ	149,090,487	8,072,269	9	18.5	916	233.1	36,555.6	1,618.6	0.1429	0
20	SC	136,795,164	3,749,358	26	36.5	102	24.0	1,585.0	157.2	0	0
21	GA	133,430,550	7,332,335	11	18.2	465	108.2	15,364.1	742.6	0.1429	0
22	NM	129,018,858	1,667,058	36	77.4	42	7.9	408.1	8.3	0	0
23	MI	117,090,209	9,616,871	8	12.2	315	100.3	37,643.1	2,104.6	0	0.2857
24	NC	114,748,484	7,338,975	10	15.6	305	81.3	8,295.5	593.3	0.0238	0.1429
25	AZ	112,029,336	4,354,830	21	25.7	243	55.5	3,344.4	163.2	0	0
26	MA	110,318,321	6,182,761	13	17.8	889	210.4	8,153.8	680.6	0	0.1429
27	NV	105,950,968	1,600,045	38	66.2	156	34.2	3,043.6	107.0	0	0
28	CO	102,720,845	3,797,828	25	27.0	645	120.3	10,592.9	246.2	0	0
29	TN	102,566,275	5,283,234	17	19.4	214	57.4	5,987.3	563.0	0	0.2857
30	IA	101,493,645	2,851,540	30	35.6	90	25.8	1,182.4	66.4	0.1429	0
31	OR	101,260,298	3,131,860	29	32.3	151	40.2	2,405.6	114.0	0.2381	0.1429
32	IN	95,933,350	5,812,322	14	16.5	204	56.3	3,792.9	215.5	0	0.1429
33	WI	88,049,241	5,127,722	18	17.2	175	60.6	4,148.6	447.8	0.1429	0.1429
34	OK	87,815,500	3,289,147	28	26.7	214	41.1	7,156.0	60.6	0	0
35	AR	80,768,613	2,502,572	33	32.3	46	16.5	6,439.8	726.6	0.1429	0
36	UT	80,426,189	1,963,000	34	41.0	150	30.8	1,541.1	112.3	0.1429	0
37	MN	78,073,598	4,647,289	20	16.8	481	134.9	10,407.7	843.5	0	0
38	KS	74,640,287	2,582,996	32	28.9	120	29.5	3,798.3	137.8	0.1429	0
39	MT	73,912,236	850,630	44	86.9	16	3.8	256.6	3.9	0.1463	0
40	NH	65,175,744	1,176,241	40	55.4	78	19.8	520.0	41.4	0	0
41	CT	58,615,499	3,346,341	27	17.5	471	131.9	18,969.2	1,135.5	0	0
42	ID	57,388,189	1,150,351	42	49.9	36	10.5	1,516.7	99.0	0.0952	0
43	ND	51,576,434	640,500	47	80.5	9	1.1	221.1	4.6	0	0
44	SD	50,399,507	725,424	46	69.5	17	5.3	279.8	16.5	0	0
45	ME	42,966,093	1,250,043	39	34.4	29	8.9	646.8	20.5	0	0
46	VT	38,018,251	585,793	49	64.9	18	5.6	109.5	4.0	0.1463	0
47	RI	36,205,932	1,024,572	43	35.3	50	14.5	1,222.8	174.0	0	0
48	NE	30,339,541	1,644,824	37	18.4	56	13.6	2,944.2	164.7	0.1463	0
49	DE	21,534,917	728,338	45	29.6	55	14.9	5,450.7	191.6	0.1429	0
50	WY	13,695,506	472,503	50	29.0	17	3.1	13.5	0.5	0.2857	0

Table III: The Impact of Seniority Shocks on State-Level Earmarks, Government Transfers, and Government Contracts

This table reports panel regressions of earmarks, transfers, and procurement contracts on Senate seniority shocks (defined as in Table I). The dependent variable in Columns 1-9 is ln(state-level annual earmarks); in Column 10 is ln(total state-level federal government transfers) from the Census Bureau (excluding category B79, which consists of nondiscretionary spending on public welfare items, e.g., Medicaid); and in Column 11 is ln(total state-level procurement contracts), drawn from the Eagle Eye database. The earmark data is from 1991-2008, the transfer data is from 1992-2007, and the procurement data is from 1992-2008. Control variables include ln(state-level population), the state-level average of ln(per capita income) over the past 6 years, and lagged values of state-level ln(per-capita income growth) and state-level unemployment rates. Year-fixed effects and state-fixed effects are included in all regressions. All standard errors are adjusted for clustering at the state level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. ***Significant at 1%; **significant at 5%; *significant at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Ear	Ear	Ear	Ear	Ear	Ear	Ear	Ear	Ear	Transfers	Contracts
Shock_Top1ChairOnly	0.446***	0.481***								0.087**	0.235**
	(9.42)	(8.77)								(2.54)	(2.33)
Shock_Top3ChairOnly			0.451***								
			(3.30)								
Shock_Top5ChairOnly				0.426***							
				(3.67)							
Shock_Top10ChairOnly					0.224**						
					(2.49)						
Shock_Top1Chair&Rank						0.330***					
						(3.30)					
Shock_Top3Chair&Rank							0.265***				
							(3.12)				
Shock_Top5Chair&Rank								0.205***			
								(2.76)			
Shock_Top10Chair&Rank									0.164**		
									(2.35)		
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Year	Year	Year	Year	Year	Year	Year	Year	Year	Year	Year
Fixed Effects	State	State	State	State	State	State	State	State	State	State	State
Adjusted R^2	0.76	0.76	0.76	0.76	0.76	0.76	0.76	0.76	0.76	0.98	0.88
No. of Obs.	889	839	839	839	839	839	839	839	839	791	790

Table IV: The Impact of Seniority Shocks on Corporate Investment, 1967-2008

This table reports panel regressions of capital expenditures on Senate seniority shocks (defined as in Table I). All models contain firm-fixed effects and year-fixed effects. Controls for lagged Q, cash flow, and lagged leverage are included where indicated. All standard errors are adjusted for clustering at the state-year level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. ***Significant at 1%; **significant at 5%; *significant at 10%.

	Dependent Variable: Capital Expenditures$_{i,t}$/A$_{i,t-1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Shock_Top1ChairOnly	-0.012***	-0.009***							
	(3.46)	(3.14)							
Shock_Top1Chair&Rank			-0.008***						
			(2.94)						
Shock_Top3ChairOnly				-0.006***					
				(2.78)					
Shock_Top3Chair&Rank					-0.006***				
					(3.57)				
Shock_Top5ChairOnly						-0.005***			
						(2.64)			
Shock_Top10ChairOnly							-0.003*		
							(1.95)		
Drop_Top1ChairOnly								0.007**	
								(2.22)	
Drop_Top3ChairOnly									0.006**
									(2.11)
$Q_{i,t-1}$		0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***
		(12.33)	(12.33)	(12.32)	(12.34)	(12.29)	(12.33)	(12.33)	(12.32)
(Cash Flow$_{i,t}$/A$_{i,t-1}$)		0.039***	0.039***	0.039***	0.039***	0.039***	0.039***	0.039***	0.039***
		(9.40)	(9.40)	(9.40)	(9.40)	(9.41)	(9.40)	(9.40)	(9.40)
Leverage$_{i,t-1}$		-0.116***	-0.116***	-0.116***	-0.116***	-0.117***	-0.116***	-0.117***	-0.117***
		(31.54)	(31.53)	(31.51)	(31.50)	(31.46)	(31.54)	(31.42)	(31.41)
Adjusted R^2	0.440	0.501	0.501	0.501	0.501	0.501	0.501	0.501	0.501
No. of Obs.	168975	139564	139564	139564	139564	139564	139564	139563	139563

Table V: Alternative Specifications for the Impact of Seniority Shocks on Corporate Investment

This table reports panel regressions of capital expenditures on House seniority shocks (from 1967-2008), earmarks directly (1991-2008), IV predicted values of earmarks (1991-2008), and various subsamples. Column 3 includes only stocks above the median lagged market capitalization in a given year in the regressions; Column 4 excludes all shocks where the prior chairman lost his chair because he/she was defeated in an election/primary. Column 5 presents capital expenditure regressions similar to those in Table IV but which also include a variable called PreShock, which is a dummy variable equal to one in the six years prior to a shock. Column 6 runs the same regression as in Column 4 of Table IV, but only for the 1991-2008 subperiod for which we have earmark data. Column 7 presents a regression of capital expenditures on ln(earmarks) directly. Column 8 presents the IV predicted value coming from an IV procedure using the first stage that regresses Shock Top3Chair&Rank (Senate Shock) on ln(earmarks), as in Table III. All models contain firm-fixed effects and year-fixed effects, and controls for lagged Q, cash flow, and lagged leverage. All standard errors are adjusted for clustering at the state-year level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. ***Significant at 1%; **significant at 5%; *significant at 10%.

	Dependent Variable: Capital Expenditures$_{i,t}$/A$_{i,t-1}$							
	Full Sample (1967-2008)					Earmark Sample (1991-2008)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shock_Top1ChairOnly (House Shock)	-0.004**							
	(2.26)							
Shock_Top3Chair&Rank (House Shock)		-0.003***						
		(2.74)						
Shock_Top3Chair&Rank (Only Large Stocks)			-0.005***					
			(3.14)					
Shock_Top3Chair&Rank (No Lost Elections)				-0.006***				
				(3.44)				
Pre-Shock					0.001			
					(0.37)			
Shock_Top3Chair&Rank					-0.009***	-0.007***		
					(5.75)	(4.63)		
Ln(Annual Earmarks)							0.000	
							(0.55)	
IV Predicted Value								-0.008***
								(6.28)
Adjusted R^2	0.501	0.501	0.611	0.501	0.393	0.554	0.510	0.510
No. of Obs.	139564	139564	68277	139564	42087	73861	88828	88828

Table VI: The Impact of Seniority Shocks on Corporate R&D, Payouts, Employment, and Sales Growth, 1967-2008

This table reports panel regressions of firm research and development (R&D), total payouts (cash dividends plus repurchases), firm-level changes in employment, and firm-level sales growth on Senate and House seniority shocks. Panel A reports results with firm-level R&D as the dependent variable, Panel B reports results with firm-level payouts (cash dividends plus repurchases) as the dependent variable, Panel C reports results with firm-level changes in employment as the dependent variable, and Panel D reports results with firm-level changes in sales as the dependent variable. Each panel contains a specification which also includes a variable called PreShock, which is a dummy variable equal to one in the six years prior to a shock, plus results for the sub-period for which earmark data is available. All models contain firm-fixed effects and year-fixed effects. All standard errors are adjusted for clustering at the state-year level, and t-stats using these clustered standard errors are included in parentheses below the coefficients. ***Significant at 1%; **significant at 5%; *significant at 10%.

Panel A: R&D	Dependent Variable: $R\&D_{i,t}/A_{i,t-1}$						
	Full Sample (1967-2008)					Earmark Sample (1991-2008)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shock_Top1ChairOnly	-0.005***						
	(2.64)						
Shock_Top3Chair&Rank		-0.003***	-0.003***			-0.003*	
		(3.04)	(2.82)			(1.66)	
Pre-Shock			0.000				
			(0.12)				
Shock_Top1ChairOnly (House Shock)				-0.009***			
				(3.31)			
Shock_Top3Chair&Rank (House Shock)					-0.001		-0.004**
					(1.28)		(1.98)
Adjusted R^2	0.782	0.782	0.708	0.783	0.782	0.777	0.777
No. of Obs.	74842	74842	19273	74841	74841	41442	41441

Panel B: Payouts	Dependent Variable: $Payouts_{i,t}/A_{i,t-1}$						
	Full Sample (1967-2008)					Earmark Sample (1991-2008)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shock_Top1ChairOnly	0.003***						
	(4.15)						
Shock_Top3Chair&Rank		0.001***	0.001			0.002**	
		(2.85)	(1.36)			(2.11)	
Pre-Shock			0.000				
			(0.34)				
Shock_Top1ChairOnly (House Shock)				0.001			
				(0.84)			
Shock_Top3Chair&Rank (House Shock)					0.001***		0.003***
					(3.59)		(4.84)
Adjusted R^2	0.392	0.392	0.418	0.392	0.392	0.412	0.412
No. of Obs.	129991	129991	39749	129990	129990	67981	67980

Panel C: ChgEmployees	Dependent Variable: $(Employ_{i,t}$- $Employ_{i,t-1})$/ $Employ_{i,t-1}$						
	Full Sample (1967-2008)					Earmark Sample (1991-2008)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shock_Top1ChairOnly	-0.009 (1.11)						
Shock_Top3Chair&Rank		-0.011** (2.41)	-0.011*** (2.67)			-0.020*** (2.64)	
Pre-Shock			0.000 (0.01)				
Shock_Top1ChairOnly (House Shock)				-0.027* (1.81)			
Shock_Top3Chair&Rank (House Shock)					-0.013*** (2.97)		-0.025*** (3.06)
Adjusted R^2	0.135	0.392	0.086	0.135	0.135	0.139	0.139
No. of Obs.	168267	129991	41577	168265	168265	89702	89702

Panel D: SalesGrowth	Dependent Variable: $(Sales_{i,t}$- $Sales_{i,t-1})$/ $Sales_{i,t-1}$						
	Full Sample (1967-2008)					Earmark Sample (1991-2008)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shock_Top1ChairOnly	-0.015 (1.30)						
Shock_Top3Chair&Rank		-0.014** (2.31)	-0.017*** (3.11)			-0.030*** (2.90)	
Pre-Shock			-0.001 (0.17)				
Shock_Top1ChairOnly (House Shock)				-0.054** (2.09)			
Shock_Top3Chair&Rank (House Shock)					-0.024*** (3.49)		-0.042*** (3.55)
Adjusted R^2	0.181	0.181	0.099	0.182	0.182	0.187	0.189
No. of Obs.	181489	181489	45418	181487	181487	96600	96599

Table VII: Heterogeneous Effects of Seniority Shocks by Economic Conditions

This table reports firm-level panel regressions of Capex and Employment on Senate seniority shocks and various interaction terms. As in Tables IV and VI, Capex is firm-level capital expenditures divided by lagged assets, and Employ is the change in number of employees. Column 1 only includes the sample of firms that have all operations in a single state. High Utilization is a dummy variable equal to one if the firm in question is above the median in terms of its capacity utilization rate. Low Unemployment is a dummy equal to one if the difference between the state-level unemployment rate (in the state the firm is headquartered in) and the national unemployment rate is less than its historical average difference. StateGDP equals the real growth rate in the state's GDP (drawn from the BEA). HighStateGDP is a dummy equal to one if the difference between the state-level real GDP growth rate and the national real GDP growth rate is greater than its historical average difference. HighUSGDP is a dummy equal to one if the national real GDP growth rate is greater than its historical average. All models contain firm-fixed effects and year-fixed effects; standard errors are adjusted for clustering at the state-year level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. ***Significant at 1%; **significant at 5%; *significant at 10%.

	Interactions with Utilization, Unemployment, State-Level Real GDP Growth, and US Real GDP Growth								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Capex (1 State)	Capex	Employ	Capex	Employ	Capex	Employ	Capex	Employ
Shock_Top1ChairOnly	-0.031**	-0.010***	0.008	-0.006***	0.004	-0.005**	0.005	-0.009***	0.007
	(2.20)	(3.28)	(0.85)	(2.91)	(0.57)	(2.38)	(0.60)	(4.31)	(0.79)
Shock_Top1ChairOnly*HighUtil		-0.014***							
(Sample Period: 1980-2008)		(2.95)							
Shock_Top1ChairOnly*LowUnemp			-0.024**						
(Sample Period: 1977-2008)			(2.17)						
Shock_Top1ChairOnly*StateGDP				-0.193***	-0.411*				
(Sample Period: 1967-2008)				(3.70)	(1.93)				
Shock_Top1ChairOnly*HighStateGDP						-0.014***	-0.025***		
(Sample Period: 1967-2008)						(5.85)	(2.56)		
Shock_Top1ChairOnly*HighUSGDP								-0.004*	-0.023**
(Sample Period: 1967-2008)								(1.79)	(2.06)
High Utilization		0.007***							
(Sample Period: 1980-2008)		(6.31)							
Low Unemployment			0.008**						
(Sample Period: 1977-2008)			(2.40)						
State GDP				0.250***	0.411***				
(Sample Period: 1967-2008)				(27.28)	(11.33)				
High State GDP						0.006***	0.012***		
(Sample Period: 1967-2008)						(13.84)	(7.83)		
High US GDP								0.024***	0.145***
(Sample Period: 1967-2008)								(14.04)	(21.88)
Adjusted R^2	0.730	0.500	0.227	0.443	0.136	0.440	0.136	0.440	0.135
No. of Obs.	3624	49520	147886	168975	168267	168975	168267	168975	168267

Appendix:
Supplementary Tables for
"Do Powerful Politicians Cause Corporate Downsizing?"

AI. Example from our sample of Chairman Ascension and Spending Shocks

To better understand our approach, consider the example of the appointment of Richard Shelby (Republican Senator, AL) to the chair of the Senate Select Intelligence Committee in 1997. Senator Shelby had been both a congressman in the US House of Representatives and Senate as a Democrat from Alabama. He switched affiliations in 1994, and the combination of his seniority and affiliation with majority party afforded him the opportunity to take the chair of the Committee. Following his appointment to this committee chairmanship, Alabama (a state which had no top committee chairmen[29] appointed in over 20 years) experienced a marked increase in its share of federal earmarks. This is represented in Figure A2, which compares Alabama's annual earmarks to those in the rest of the United States. Although earmark spending increased substantially in the US during this period, Alabama experienced roughly twice the average growth of all other states following Shelby's appointment. Specifically, while Alabama averaged 6 million dollars less in annual earmarks than the average of other US states before Shelby's appointment, they averaged over 90 million dollars *more* than other states after his appointment.

At this time, Homes Inc. was a large manufacturer of lower-cost fabricated homes headquartered in northern Alabama.[30] When Richard Shelby ascended to the chairmanship in 1997 and earmarks to Alabama increased, Homes Inc. significantly decreased its capital expenditures and employee base during the ensuing years. A comparison of Homes Inc. to the rest of its industry outside of Alabama[31] reveals that while Homes Inc. significantly reduced its capital expenditures from this pre-appointment period, the rest of its industry in fact increased its rate of capital accumulation (-79.5% vs. 16.6% as a percentage of assets). Further, while the industry reduced its rate of employment growth only modestly from the pre-appointment period, Homes Inc.'s curtailment in employment was much more substantial (-30.2% vs. -7.9%).

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[29] We use several measures of top committees throughout the paper. Here, we refer to our most broad category. The list of the top 10 most influential committees is from Edwards and Stewart (2006); for the Senate these committees are Finance, Veterans Affairs, Appropriations, Rules, Armed Services, Foreign Relations, Intelligence, Judiciary, Budget, and Commerce.

[30] We have masked the name of this firm, although this is an actual company from our sample.

[31] Industry is defined as 1-digit SIC Code, which is 2 for Homes Inc.'s industry.

Additionally, while Homes Inc. had zero payouts in the years pre-appointment, it began paying out (in the form of repurchasing stock) in the year of appointment and ramped up stock repurchasing in the immediate subsequent years. Lastly, while Homes Inc. saw a large and significant drop in sales, the rest of the industry remained approximately flat (-38.2% vs. -3.1%). As a more tangible example of the possible mechanism, in the post-appointment period over $15 million in earmarks went specifically to the construction of housing and facilities for lower income families (which are a direct competitor to the prefabricated homes produced by Homes Inc.), contrasted with only $500,000 over the entire pre-appointment period.

Table A1: Summary Statistics

This table reports additional summary statistics for the sample. The table presents the average years as Senate (House) Chair, as well as years of seniority in the chamber and on the committee in question, for all Top 10 Chairman and Ranking Members in our sample. The list of the top 10 most influential committees is from Edwards and Stewart (2006); for the Senate these committees are Finance, Veterans Affairs, Appropriations, Rules, Armed Services, Foreign Relations, Intelligence, Judiciary, Budget, and Commerce, and for the House these committees are Ways and Means, Appropriations, Energy and Commerce, Rules, International Relations, Armed Services, Intelligence, Judiciary, Homeland Security, and Transportation and Infrastructure.

	Top 10 Chairman/Ranking Member Characteristics, 1967-2008							
	All Chair/Rank				High Inc. States	Low Inc. States	Democrat	Republican
	Mean	Median	Std Dev	Max	Mean	Mean	Mean	Mean
Years as Senate Chair/Rank	7.6	6	5.7	24	5.9	9.0	9.1	6.7
Years of Senate Chamber Seniority	21.6	21	8.5	49	20.3	22.3	23.8	19.4
Years of Senate Comm. Seniority	18.2	17	9.2	49	17.1	18.8	21.2	15.3
Years as House Chair/Rank	6.3	6	4.3	18	5.7	7.2	6.7	5.8
Years of House Chamber Seniority	13.1	13	4.3	27	12.8	13.7	15.0	11.2
Years of House Comm. Seniority	11.6	12	4.8	26	11.4	12.1	13.7	9.5

Table A2: House Seniority Shocks

This table reports panel regressions of capital expenditures on House seniority shocks (from 1967-2008). All models contain firm-fixed effects and year-fixed effects, and controls for lagged Q, cash flow, and lagged leverage defined as in Table IV are included when indicated. All standard errors are adjusted for clustering at the state-year level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. ***Significant at 1%; **significant at 5%; *significant at 10%.

	Dependent Variable: Capital Expenditures$_{i,t}$/A$_{i,t-1}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Shock_Top1ChairOnly	-0.006**	-0.004**							
	(2.51)	(2.26)							
Shock_Top1Chair&Rank			-0.005***						
			(3.81)						
Shock_Top3ChairOnly				-0.001					
				(0.90)					
Shock_Top3Chair&Rank					-0.003***				
					(2.74)				
Shock_Top5ChairOnly						-0.002*			
						(1.93)			
Shock_Top10ChairOnly							-0.002***		
							(2.65)		
Drop_Top1ChairOnly								0.006*	
								(1.66)	
Drop_Top3ChairOnly									0.001
									(0.23)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.439	0.501	0.501	0.501	0.501	0.501	0.501	0.501	0.501
No. of Obs.	168977	139564	139564	139564	139564	139564	139564	139562	139562

Table A3: The Impact of Senate Seniority Shocks on R&D and Payouts, 1967-2008

This table reports panel regressions of firm research and development (R&D) and payouts (cash dividends plus repurchases) on Senate seniority shocks. Panel A reports results with firm-level R&D as the dependent variable, and Panel B reports results with firm-level payouts (cash dividends plus repurchases) as the dependent variable. All models contain firm-fixed effects and year-fixed effects, and include controls for lagged Q, cash flow, and lagged leverage as in Table IV. All standard errors are adjusted for clustering at the state-year level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. ***Significant at 1%; **significant at 5%; *significant at 10%.

Panel A: R&D	Dependent Variable: $R\&D_{i,t}/A_{i,t-1}$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock_Top1ChairOnly	-0.005*** (2.64)									
Shock_Top1Chair&Rank		-0.003** (1.98)								
Shock_Top3ChairOnly			-0.004*** (2.63)							
Shock_Top3Chair&Rank				-0.003*** (3.04)						
Drop_Top1ChairOnly					0.009*** (3.72)					
Drop_Top3ChairOnly						0.005** (2.13)				
Shock_Top1ChairOnly (House Shock)							-0.009*** (3.31)			
Shock_Top1Chair&Rank (House Shock)								-0.005*** (3.54)		
Shock_Top3ChairOnly (House Shock)									-0.005*** (2.61)	
Shock_Top3Chair&Rank (House Shock)										-0.001 (1.28)
Adjusted R^2	0.782	0.782	0.782	0.782	0.782	0.782	0.783	0.783	0.782	0.782
No. of Obs.	74842	74842	74842	74842	74842	74842	74841	74841	74841	74841

Panel B: Payouts	Dependent Variable: Total $Payout_{i,t}/A_{i,t-1}$									
	(1)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(11)	(11)
Shock_Top1ChairOnly	0.003***									
	(4.15)									
Shock_Top1Chair&Rank		0.002***								
		(3.70)								
Shock_Top3ChairOnly			0.002***							
			(4.23)							
Shock_Top3Chair&Rank				0.001***						
				(2.85)						
Drop_Top1ChairOnly					-0.001*					
					(1.71)					
Drop_Top3ChairOnly						-0.001				
						(1.44)				
Shock_Top1ChairOnly (House Shock)							0.001			
							(0.84)			
Shock_Top1Chair&Rank (House Shock)								0.001**		
								(2.51)		
Shock_Top3ChairOnly (House Shock)									0.002***	
									(3.14)	
Shock_Top3Chair&Rank (House Shock)										0.001***
										(3.59)
Adjusted R^2	0.392	0.392	0.392	0.392	0.392	0.392	0.392	0.392	0.392	0.392
No. of Obs.	129991	129991	129991	129991	129990	129990	129990	129990	129990	129990

Table A4: Breadth of Corporate Response to Seniority Shocks, 1967-2008

This table reports results for state-level regressions of various corporate response variables on senate seniority shocks, with firm and year fixed effects included; the Shock_Top10Chair&Rank variable is used as the independent variable here in order to maximize the number of states receiving a shock. Regression coefficients are averaged (equally) across the 48 states that have experienced such a shock, and t-stats computed using the standard-deviation of these coefficients across states are reported here. The %States Predicted Sign also shows significance level (in *) of a binomial test whether the state with the predicted sign is greater than a null of 0.5.

	State-Level Cross-Sectional Average Regression Coefficients (Shock_Top10Chair&Rank)			
Dependent Var:	Coeff:	t-stat	%States Predicted Sign	Individual States with Predicted Sign Coefficient
Capital $Expenditures_{i,t}/A_{i,t-1}$	-0.026***	3.48	73.9%***	AK,AL,AZ,CO,CT,FL,GA,HI,IL,KS,MA,MD,MI,MO,MS,MT,ND,NE,NY,NJ,NV,NY,OK, OR,PA,RI,SC,TN,TX,UT,VA,WA,WI,WY
$R\&D_{i,t}/A_{i,t-1}$	-0.003	-0.51	53.3%	AZ,CA,FL,GA,HI,ID,IL,IN,KS,KY,LA,MD,MT,ND,NH,NJ,NM,NY,OR,PA,SC,SD,TX,WA
Total $Payout_{i,t}/A_{i,t-1}$	0.004**	1.98	67.4%***	AK,AL,AZ,CO,CT,FL,IL,IN,KY,LA,MA,MO,MS,ND,NE,NH,NJ,NM,NV,NY,OK,OR,PA, RI,SC,TN,TX,UT,VT,WA,WI
ChgEmployees	-0.091***	4.58	82.6%***	AK,AR,AZ,CA,CO,CT,FL,GA,HI,IA,IL,KS,KY,MA,MD,MI,MO,MS,MT,NC,NE,NH,NJ, NV,NY,OK,OR,PA,RI,SD,TN,TX,UT,VA,WA,WI,WV,WY
SalesGrowth	-0.130***	4.81	78.3%***	AK,AL,AZ,CO,CT,FL,GA,Hi,IA,ID,IL,KS,KY,LA,MA,MD,MI,MO,MS,MT,NC,NE,NH,NJ, NY,OK,OR,PA,RI,SC,TN,TX,UT,VA,WA,WY

Table A5: State-Level Responses: GDP, Employment, and Personal Income

This table reports panel regressions of a number of state-level dependent variables on US Senate seniority shocks (defined as in Table I). In columns 1-3, the dependent variables are ln(GDP), ln(Employment), and ln(Personal Income), respectively. These data items are state-level aggregates drawn from the Bureau of Economic Analysis (BEA). The regressions are run at the state level as in Table III. The model contains both state-fixed effects and year-fixed effects, standard errors are adjusted for clustering at the state-shock level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. Significance levels are denoted by: *** for the 1%; ** for the 5%; and * for the 10% level.

State-Level Responses to Senate Seniority Shocks			
	(1)	(2)	(3)
	Log(GDP) 1964-2008	Log(Employment) 1969-2008	Log(Personal Income) 1958-2008
Total	-0.052	-0.048*	-0.058*
	(1.45)	(1.70)	(1.83)
Private	-0.056	-0.052	-0.062*
	(1.43)	(1.41)	(1.89)
Manufacturing	-0.156***	-0.111*	-0.104*
	(3.19)	(1.98)	(1.79)
Construction	-0.096*	-0.069	-0.074*
	(1.91)	(1.28)	(1.67)
Transportation	-0.076*	-0.053	-0.080
	(1.96)	(1.10)	(1.60)
Wholesale Trade	-0.051	-0.024	-0.067
	(1.03)	(0.67)	(1.38)
Retail Trade	-0.076**	-0.053*	-0.079**
	(2.28)	(1.78)	(2.27)
Finance and Real Estate	0.025	-0.036	-0.019
	(0.42)	(0.77)	(0.44)
Services	-0.040	-0.039	-0.061
	(0.85)	(0.98)	(1.40)
Fixed Effects	Year	Year	Year
Fixed Effects	State	State	State
No. of Obs.	2300	2000	2550

Figure A1: The Impact of Seniority Shocks across Horizons

This figure reports various horizon definitions of the post-chair ascendancy shock used throughout the paper. The shock reported in all tables in the paper is the six-year post shock. Below is shown shock horizons defined from 1-10 years, and the resultant coefficients in the impacts of the shocks on: capital expenditures (Cap Ex), measured as capital expenditures scaled by lagged assets, research and development expenditures (R&D), measured as R&D expenditures scaled by lagged assets, payouts (Payout), measured as total dividends and repurchases scaled by lagged assets, sales growth (Sales), measured as growth in sales, and employment growth (Employment), measured as the growth in employment.

Figure A2: Earmarks in Alabama vs. Rest of US

This figure shows the annual earmarks (in millions of dollars) for the state of Alabama and for the average state in the United States excluding Alabama (Rest of US), from 1991-2003.