Executive Summary

Snyder and Welch have recently posted a critique of our 2011 JPE paper, "Do Powerful Politicians Cause Corporate Downsizing," (*Journal of Political Economy*, 199, 1015-1060.). Here, we provide a comprehensive response to Snyder and Welch's critique, demonstrating why the claims of Snyder and Welch are entirely unfounded. A link to our published paper is here, while Snyder and Welch's critique document is appended to the end of this document.

In this document we detail why the main claims of Snyder and Welch are not supported in the actual data. In fact, the results presented in our original paper in many instances become stronger (economically and statistically) when we focus on the sub-periods suggested in the critique. This is an Executive Summary of the key focal points of the critique, along with our response. Following this Executive Summary in the document are: Section I - a more in-depth look at the main points of Snyder and Welch, along with tables showing empirical results in the data in response to these points; Section II – a point-by-point response to each issue brought up in Snyder and Welch; and Section III – a history of the ongoing interaction with Snyder and Welch.

1. Central critique – Snyder and Welch argue that the results of the paper are driven by the behavior of Texas oil firms during the late 1980s and early 1990s. In particular, Snyder and Welch argue that the single appointment of Lloyd Bentsen to a chair of the Senate Finance Committee in 1987 was directly preceded by an oil-price drop and the S&L crisis, and that firms in Texas were disproportionately impacted by these events (not the Bentsen chairmanship).
    a. Solution: We deal with this economic argument in the cleanest way possible - we completely remove the entire "problem" sample period and observations that are claimed to be driving our full-sample results. We run sub-period analyses post-problem period (post-1992) and pre-problem period (pre-1981), both including and excluding Texas, both including and excluding the oil industry, and the results are large, robust, and significant across a range of specifications and outcome variables.
    b. Put simply, if the main critique is that one time period is driving our results, and we completely remove this time period, re-run the results pre- and post-, with the results being strong and robust before and after (in fact stronger in the recent period), how can the results be "driven" by this time period? We were left asking the same question. In a nutshell, these simple facts neutralize the focal point of the critique document. We now go on to address some of the ancillary, smaller issues they bring up.

2. Minor critiques – Snyder and Welch then go on to criticize the clustering, coding choices, and coefficient magnitudes.
    a. Solution: We show that the results in the paper are robust to the clustering choice proposed by Snyder and Welch of state level along with state-shock level (given that results in the paper actually vary at a state-shock period level, not the state level). We also give the motivation, as we do in the paper, for our exogenous coding of the chairmanship shocks, as opposed to the endogenous coding proposed and used by Snyder and Welch.
    b. And finally, on the magnitude point, we completely agree that the results in the paper cannot be all driven by earmarks - which is why we make this same point, along with providing empirical evidence for multiple larger sources of government transfers (e.g., procurement contracts and state-level transfers), in the original published paper, which we point back to. More importantly, as we explicitly note in the discussion section of the original paper (on pg. 1056), the results in the paper cannot be used to compute a credible estimate of the government multiplier, since the effects in the paper 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.) as well. Why Snyder and Welch seem intent on performing an incorrect and

misleading calculation, when the original published paper already discusses the challenges around doing so, is a puzzle to us.

3. History of interaction with Snyder and Welch
    a. Snyder and Welch first contacted us with concerns regarding the paper 20 months ago. We spent a lot of time re-running our tests to ensure that they did not find some mistake in our implementation or results that so many others had missed. We recreated the data from scratch for them, and we presented Snyder and Welch with an exhaustive set of results. Snyder and Welch then returned 20 months later with another disjoint set of concerns, which are contained in their current critique. We append their initial critique, our response to their initial critique, and the current critique to the end of this document.

Lastly, we have made it a point throughout the writing of this paper to share our data and code with all researchers, including Snyder and Welch. The good news is that since the publishing of our paper, Compustat has come out with a new database called the Compustat Snapshot Database, which corrects some minor back-filling issues in the old versions of Compustat's data. We have obtained this data, re-created our datasets, and have now posted the new version of our dataset and code both on the JPE's site and on our personal web sites (along with the commented code that generates every result in this response, which uses the new, cleaner data). The results from our published paper actually get stronger with this new, less noisy data. We are excited about the new research questions other researchers can tackle with it, and look forward to hearing about them.

Section I: An In-Depth Analysis of the Key Points in Snyder and Welch (2014)

1. Response to the Main Critique of Snyder and Welch

The focal point of the critique is that the results in our paper were driven by the state of Texas during the late 1980s and early 1990s. This is the focus of the abstract, introduction, conclusion, and takes up the majority of the critique document. In particular, Snyder and Welch argue that the single appointment of Lloyd Bentsen to a chair of Senate Finance Committee in 1987 was directly preceded by an oil-price drop and the S&L crisis, and that firms in Texas were disproportionately impacted by these events (not the Bentsen chairmanship). Snyder and Welch claim: "Our paper shows that the CCM results were primarily due to the fact that existing Texas firms had high average capital expenditures around 1981, 4–7 years before Lloyd Bentsen assumed the U.S. Senate Finance Committee chairmanship."

We demonstrate clearly and unequivocally in this document that this episode does not drive the findings in our paper. We start with the simplest and most obvious test. If the concern is that the behavior of firms in Texas during this period is driving the results, one can simply kick out Texas over this time period from 1982-1992. Or, more conservatively, one could kick out ALL states over this time period, since the oil price collapse and S&L crisis may have impacted other firms and states as well. To illustrate that this time period has no impact on the inferences that can be drawn from our paper, we simply run our identical tests after 1992 (after the shock of Bentsen's appointment to the Senate Finance Chairmanship), and before 1982 (as the concern is that capital expenditures peaked in 1981 and then started falling in 1982 because of oil price declines starting in 1982). We also run a single pooled regression using all data, but excluding the 1982-1992 period.

As one can see from Table 1, Panels A-D, the results are remarkably robust. For all 5 outcome variables used in the paper (capital expenditures (capex), R&D, payout, employment, and sales growth), shocks to the Senate Finance Chairmanship lead to large and significant effects: post-1992 (1993-2008), pre-1982 (1968-1981), and over the entire time period excluding 1982-1992. In fact, the results are even stronger recently, which is not only likely more representative of the environment existing today, but has the most up to date and cleanest data.[1]

So what do Welch and Snyder do differently? They actually don't take the economic action implied by their claimed concern. They instead oddly decide to run a mis-specified regression where they kick out the entire state of Texas for the entire sample – including the majority of the sample from 1968-2008 for which they have no concern – and run tests only on the single outcome variable of capex for one single measure of top committees (the one that has the fewest number of shocks over the sample period). This puzzling approach reduces the coefficient on capex for Shocks to the Top 1 Committee; but interestingly, it also makes the R&D, payout, employment, and sales growth results all stronger, a point which is ignored in Snyder and Welch, and is inconsistent with the ex-post "alternative explanation" developed in the critique. Most

[1] We say "cleanest" data because we have gone back and obtained the recently available COMPUSTAT snapshot data, which has point-in-time data on firms' headquarters locations (i.e., it correctly codes BOEING as being in WA until they officially moved to IL), as opposed to the back-fit headquarters data found in the flagship COMPUSTAT data. As we explicitly mentioned in the published paper, all Compustat headquarters data is back-fit. Fortunately headquarter moves are fairly rare, and so the results we present here are very similar to those presented in the original JPE paper; some of the results from the paper are now statistically even stronger with this perfect headquarters location data (as opposed to the noisier data we had used in the published version). Note however that COMPUSTAT's Snapshot database only goes back to 1991, and so all data before 1991 is subject to this same back-filling issue. In the new version of data (posted on the JPE's site and our websites), we use the point-in-time perfectly accurate data back to 1991, and then use the 1991 listed headquarters for all firms from that point backwards in order to get the best measure of headquarters location .

importantly, this approach: i.) has very little to do with Texas firms over the 1982-1992 period, and ii.) does not actually address Snyder and Welch's underlying concern.

Again, to reiterate, our tests are designed to specifically address Snyder and Welch's alleged actual concern. We run our pre- and post-tests on a variety of sub-samples, for example kicking out: i.) Texas firms, ii) Texas oil firms, and iii.) all oil firms more generally (across all states). As Table 1 demonstrates, the results are strong, significant, and robust to any of these sample variations. We also run a single panel regression just kicking out this interim period (first with all firms, then kicking out Texas firms, Texas oil firms, all oil firms, etc.), and again the results are strong and robust.[2] And importantly, as noted earlier, we run these regressions for <u>all</u> of our main outcome variables (not just capex). As Table 1 shows (across all four Panels), these other outcome variables are by and large quite robust to time periods, to sub-samples kicking out oil firms, etc., even becoming larger and more significant in many cases. Further, all of these alternative outcome variables show the same larger and more significant impact from shocks in the more recent, and likely more representative, period.

So, in summation, the main critique of Snyder and Welch against the paper is that the results were driven by the behavior of Texas firms, and oil firms more generally, in the mid-1980s to early 1990s.

However, if one simply removes these firms and this time period altogether, the results are strong and robust before and after this time period. So, if the results are strong before and after this period, how can the results be "driven" by this period? We were left asking the same question. In a nutshell, these simple facts neutralize the focal point of the critique document. We now go on to address some of the ancillary, smaller issues they bring up.

2. Evidence Ignored in Snyder and Welch

One consistent aspect of the critique is its cherry-picked nature when it comes to the results and analyses that are reported (and the results not reported). One of the main examples is that the original paper explores a series of outcome variables, while the critique focuses almost exclusively on Capex. By contrast, for each analysis (for each sub-period and for all subsets of firms) that we report in Table 1 in this response, we report results across all of these outcome variables. As Table 1 shows, these results are robust across all permutations. In fact, in many sub-periods and sub-samples they are significantly larger (as mentioned, for instance, for nearly all outcomes in the most recent period the relationships appear to be getting stronger economically and statistically).

Below is a list of the analyses and aspects of the published paper that were largely or fully ignored in this critique:

a. Other outcome variables:
    i. As noted above, examining all outcome variables gives the broader picture of the impact of government spending on firms, and shows that the larger set of outcomes we examine in the paper are robust to the battery of sub-periods and sub-samples suggested by the critique.
b. Clustering of standard errors:
    i. Snyder and Welch argue that clustering the standard errors at the state level in the basic panel regressions is critical to the inferences that can be drawn. As we show in Table 1, the results are virtually identical across nearly every single specification,

[2] And even if one were to ignore the ex-post story developed in Snyder and Welch's critique and use the *entire* sample period (as they do), the results are still large and statistically significant when you exclude all oil firms, or all Texas oil firms, and only become insignificant in the capex regression if you remove all Texas firms (and even in that regression the *t*-statistic ranges between 1.37-1.48).

even after clustering by state. It is only in one specification (examining capex, for the full sample, for the Top 1 shock only dependent variable---coincidentally the only specification that Snyder and Welch focus on), where there is even a modest impact of clustering; and even in that specification, the *t*-statistic on the shock variable is still 1.86 and significant at the 10% level. In addition, although it leads to the same conclusions, we also include state-shock clustering of standard errors in Table 1 because we view this to be the more correct way to calculate standard errors for our estimates (we had these in the paper originally but were encouraged to remove them by discussants who thought they didn't add much, and were confusing to explain to readers).

c. Discussion of magnitudes (transfers and contracts)
   i. The critique has an entire section where it calculates the magnitude of a hypothetical multiplier using only earmark dollars as the shock denomination. We agree with the critique that this is an incorrect exercise – which is exactly why we have an entire section of the paper that says exactly this. Further, in addition to discussing the fact that other flows of government funds could be related to these shocks, we explicitly show empirically that broader measures of government transfers along with the awarding of government contracts are related to these same exogenous shocks (Table III in the published paper). Snyder and Welch ignore both the empirical results and the discussion section of the paper regarding these issues.
   ii. More importantly, as we explicitly note in the discussion section of the original paper (on pg. 1056), the results in the paper cannot be used to compute a credible estimate of the government multiplier, since the effects in the paper 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.) as well. Why Snyder and Welch seem intent on performing an incorrect and misleading calculation, when the original published paper already discusses the challenges around doing so, is a puzzle to us.

d. Placebo test for identification
   i. Snyder and Welch point out: *"For an exogenous shock to be plausible, it is important to test whether this exogenous shock also appeared to have an effect in placebo years before the event.*"
      1. We couldn't agree more, which is why we have this exact variable (Pre Shock) in Table V in the paper.
      2. In addition to this, in Figure A1 of the paper, we show impulse responses for all of our outcome variables rolling for a number of years following the shock. These results are also ignored.

e. State-by-state variation
   i. We were also quite interested in the state-by-state variation in the effect, and wanted to ensure that no one state was driving the results. We ran analyses for all of our outcome variables state-by-state (such that each state gets the exact same weight, nullifying any overweighting of states). We show that our results are strong and robust using this cross-sectional equal-weighting by state method. This is Appendix Table A4 in the published paper. It is completely ignored in Snyder and Welch.

An upshot of these omissions by Snyder and Welch is that the critique ends up as an unbalanced view of the complete picture. By contrast, in Table 1 of this response we report coefficients for all outcome variables run for each sub-sample and for each time-period in order to counterbalance the cherry-picked nature of the critique document. Additionally, we have posted the updated, cleaned dataset to our websites and the JPE website (along with the commented code) to generate all of the results reported in this Response.

3. Coding Issue

As we show definitively in our tests above, the behavior of firms in Texas cannot explain the findings in our paper. Snyder and Welch essentially agree that this statement is true for every single outcome variable in our paper, except for capex. Why their alternative story would only apply to capital expenditures and not to the other outcome variables we study in our paper is a puzzle. Rather than try to address this strange inconsistency, Snyder and Welch come up with a *new* explanation for why the results for the other outcome variables must be wrong. In particular, they claim that it is due to the way shocks are "coded" in the paper. Note of course that this coding issue, as they readily admit, does not impact the capex results (that is instead "due to Texas"), but only these other outcome variables. Again why this strange asymmetry would exist is never explained.

Regarding the coding of the shocks, one of the key contributions of our paper is to employ an exogenous measure of government spending. To do so, we compute shocks for a fixed length of time for all shocks (Top 1, Top 3, Top 5, Top 10, plus the shocks including the ranking members). The idea behind a fixed shock length was to ensure that the shock did not vary as a function of future election outcomes. Only ascendency is truly exogenous (does not rely on the ascending Senator's election). By contrast, continuation relies on re-election and departure - both of which are likely tied to endogenous economic conditions. We used a 6-year shock to match a Senate term, but in the online Appendix Figure A1 of the original paper, which was again ignored by Snyder and Welch, we showed that our results were not sensitive to the length of the shock period. In this Appendix Figure A1 we show an impulse response that shows response by shock-year so readers can understand the impact of shortening or lengthening the shock horizon. We would also note that we presented this paper at over a dozen schools, at many of the top conferences in the profession (NBER, WFA, FRA, etc.), and had 3 referees and 6 discussants carefully review the paper; clearly a large cross-section of the profession felt that our shock construction was a sensible approach.

By contrast, Snyder and Welch employ an endogenous coding approach that codes a shock as valid for a Senator's entire time as Chairman. Why Snyder and Welch felt it was important to show that some (but importantly not all) of the outcome variables we study are sensitive to using a more endogenous shock variable than the one we employ in our paper is again puzzling to us.

Finally, Snyder and Welch also claim in several instances throughout the paper that we mistakenly failed to code a particular shock, even though we explained to them 2 years ago (which they failed to mention in this "revised" draft) that we took the following approach across all our shock definitions: if the same state was already "being shocked" from another chairman event (e.g., a Top 3 or Top 5 shock) in the past 6 years, then we did not apply another shock starting in this year and lasting for another 6 years. The idea here being that we did not want to shock a state forever. Since Snyder and Welch only look at "Top 1" shocks, they failed to see these shocks that show up to these same states during an earlier year. Again this reinforces the problematic and highly selective nature of the Snyder and Welch critique, which consistently ignores the entire body of evidence presented in the paper. In summary, the shock definition we use in the paper was vetted by a huge cross-section of the profession, it is economically justifiable, and it is plausibly exogenous; by contrast, Snyder and Welch apparently prefer an endogenous coding scheme that impacts some variables but not others.

4. COMPUSTAT Corporate Headquarter (HQ) Backfilling and Cleaned Data

We use the COMPUSTAT database for our paper. One unfortunate component of this data that all papers using COMPUSTAT face - including ours - is that COMPUSTAT backfills corporate HQ location data. What this means is that if a firm has been publicly traded since 1960 and was headquartered in NY from 1960-2000, but changes its HQ location in 2000 to CA, COMPUSTAT goes back to the 1960-2000 data and backfills the HQ location to CA for all years that the firm was publicly traded (including 1960-2000). Fortunately, HQ moves happen rarely, and we mention this caveat in the published paper.

Since the time of our publication, COMPUSTAT has come out with a new product called the COMPUSTAT Snapshot Database. This data provides point-in-time reported data (not backfilled) going back to 1991 for all publicly traded firms. We obtained this full data going back to 1991, and created a cleaned-HQ version of our dataset. It is with this new, cleaner data that all analyses in this document are run. Fortunately, as headquarter moves are fairly rare, the results we present here are very similar to those presented in the original JPE paper; some of the results from the paper are now statistically even stronger with this perfect headquarters location data (as opposed to the noisier data we had used in the published version). Note again however that COMPUSTAT's Snapshot database only goes back to 1991, and so all data before 1991 is subject to this same back-filling issue. In the new version of data (posted on the JPE's site and our websites), we use the point-in-time, perfectly accurate data back to 1991, and then use the 1991-listed headquarters for all firms from that point backwards in order to get the best possible measure of headquarters location.

5. History of Interaction

Snyder and Welch first contacted us with concerns regarding our paper 20 months ago. We spent a lot of time re-running our tests to ensure that they did not find some mistake in our implementation or results that so many others had missed. We recreated the data from scratch for them, and we presented Snyder and Welch with an exhaustive set of results. We attach the first critique document from 20 months ago at the back of this response (in Section III), along with our responses point-by-point to that prior critique document.

Snyder and Welch then returned 20 months later with another disjoint set of concerns. We again have prepared a response showing simply why essentially all of their new critiques (much like the set 20 months ago) are incorrect, a function of completely subjective choice, or simply don't hold water in the actual data.

6. Summary

In summary, in this document we demonstrate why the main claims of Snyder and Welch are not supported by the data. Interested readers can engage further by reading our point-by-point rebuttal to Snyder and Welch's claims that we offer in the following Section II of this response, but most readers can surely stop here. To recap, the basic economic argument that the key findings in the paper are driven by the behavior of Texas firms (and oil firms more generally) in the mid-late 1980s is false. In addition, all of the ancillary claims made in their critique are either incorrect, selectively applied, or at best misleading. Fortunately, the one positive aspect to emerge from this critique is that we were able to update and improve the underlying data used in the study, by incorporating the new point-in-time headquarters data produced by Compustat. In doing so, many of the key results in the paper are estimated even more precisely, and the main findings look if anything stronger than in the published paper.

Table 1
The Impact of Senate Seniority Shocks on Corporate Investment, R&D, Payouts, Employment, and Sales Growth

This table reports panel regressions of firm-level capital expenditures (Capex), research and development (R&D), payouts (cash dividends plus repurchases), change in employment, and sales growth on Senate seniority shocks. Only shocks to the Top 1 committee (Senate Finance Committee) chairmanship are used. All models contain firm-fixed effects and year-fixed effects. Columns 1-3 also include controls for lagged Q, cash flow, and lagged leverage. Panels A-D report results from the same specifications, but for different time periods and sub-samples of firms. All standard errors are adjusted for clustering at the state or state-shock 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: Full Sample Period (1967-2008)				
	Capex	R&D	Payout	ChgEmp	SalesGr
	(1)	(2)	(3)	(4)	(5)
Including All Firms:					
Shock_Top1ChairOnly	-0.008	-0.006	0.003	-0.017	-0.019
(State Clustering)	(1.86)*	(3.23)***	(4.65)***	(2.34)**	(2.41)**
(State-Shock Clustering)	(2.26)**	(3.52)***	(4.62)***	(2.40)**	(3.03)***
No. of Observations	145,191	77,598	134,372	166,736	179,360
Pct. of Total Observations	100.0%	100.0%	100.0%	100.0%	100.0%
Removing Oil Firms:					
Shock_Top1ChairOnly	-0.003	-0.006	0.003	-0.015	-0.021
(State Clustering)	(2.94)***	(3.22)***	(5.88)***	(1.84)*	(2.16)**
(State-Shock Clustering)	(3.13)***	(3.51)**	(5.25)***	(1.97)*	(2.76)***
No. of Observations	138,114	76,086	127,734	159,626	171,462
Pct. of Total Observations	95.1%	98.1%	95.1%	95.7%	95.6%
Removing Texas Oil Firms:					
Shock_Top1ChairOnly	-0.003	-0.006	0.003	-0.016	-0.019
(State Clustering)	(2.61)**	(3.19)***	(6.32)***	(1.88)*	(1.98)*
(State-Shock Clustering)	(2.64)**	(3.49)***	(5.50)***	(2.02)**	(2.46)**
No. of Observations	142,153	76,916	131,531	163,676	176,011
Pct. of Total Observations	97.9%	99.1%	97.9%	98.2%	98.1%
Removing All Texas Firms:					
Shock_Top1ChairOnly	-0.002	-0.007	0.003	-0.021	-0.026
(State Clustering)	(1.37)	(3.51)***	(6.35)***	(2.60)**	(2.71)***
(State-Shock Clustering)	(1.48)	(3.22)***	(5.12)***	(2.93)***	(3.43)***
No. of Observations	132,921	72,918	122,849	153,594	164,838
Pct. of Total Observations	91.5%	94.0%	91.4%	92.1%	91.9%

	Panel B: Full Period Excluding 1982-1992 (i.e., 1968-1981,1993-2008)				
	Capex	R&D	Payout	ChgEmp	SalesGr
	(1)	(2)	(3)	(4)	(5)
Including All Firms:					
Shock_Top1ChairOnly	-0.006	-0.012	0.004	-0.041	-0.047
(State Clustering)	(4.50)***	(3.97)***	(5.03)***	(3.86)***	(5.10)***
(State-Shock Clustering)	(4.86)***	(3.83)***	(4.34)***	(3.87)***	(4.88)***
No. of Observations	102,468	55,716	92,480	118,682	128,645
Pct. of Total Observations	70.6%	71.8%	68.8%	71.2%	71.7%
Removing Oil Firms:					
Shock_Top1ChairOnly	-0.007	-0.012	0.004	-0.043	-0.054
(State Clustering)	(5.17)***	(3.98)***	(4.86)***	(3.97)***	(6.17)***
(State-Shock Clustering)	(5.55)***	(3.84)***	(4.22)***	(4.08)***	(6.23)***
No. of Observations	98,173	54,762	88,577	114,241	123,782
Pct. of Total Observations	67.6%	70.6%	65.9%	68.5%	69.0%
Removing Texas Oil Firms:					
Shock_Top1ChairOnly	-0.006	-0.012	0.004	-0.042	-0.050
(State Clustering)	(4.22)***	(3.99)***	(4.89)***	(3.79)***	(5.37)***
(State-Shock Clustering)	(4.52)***	(3.85)***	(4.26)***	(3.83)***	(5.02)***
No. of Observations	100,442	55,242	90,639	116,614	126,408
Pct. of Total Observations	69.2%	71.2%	67.5%	69.9%	70.5%
Removing All Texas Firms:					
Shock_Top1ChairOnly	-0.006	-0.011	0.004	-0.040	-0.049
(State Clustering)	(4.44)***	(3.74)***	(4.45)***	(3.66)***	(5.02)***
(State-Shock Clustering)	(4.71)**	(3.63)***	(3.84)***	(3.67)***	(4.72)***
No. of Observations	93,873	52,350	84,581	109,345	118,368
Pct. of Total Observations	64.7%	67.5%	62.9%	65.6%	66.0%

	Panel C: Recent Period (1993-2008)				
	Capex	R&D	Payout	ChgEmp	SalesGr
	(1)	(2)	(3)	(4)	(5)
Including All Firms:					
Shock_Top1ChairOnly	-0.011	-0.014	0.005	-0.062	-0.071
(State Clustering)	(5.25)***	(3.97)***	(4.25)***	(4.08)***	(5.68)***
(State-Shock Clustering)	(5.93)***	(4.04)***	(3.27)***	(4.51)***	(5.25)***
No. of Observations	68,965	39,029	63,065	80,156	86,301
Pct. of Total Observations	47.5%	50.3%	46.9%	48.1%	48.1%
Removing Oil Firms:					
Shock_Top1ChairOnly	-0.011	-0.014	0.005	-0.063	-0.078
(State Clustering)	(5.07)***	(3.99)***	(4.26)***	(3.98)***	(5.86)***
(State-Shock Clustering)	(5.84)***	(4.06)***	(3.33)***	(4.48)***	(5.80)***
No. of Observations	66,147	38,590	60,454	77,226	83,137
Pct. of Total Observations	45.6%	49.7%	45.0%	46.3%	46.4%
Removing Texas Oil Firms:					
Shock_Top1ChairOnly	-0.010	-0.014	0.005	-0.063	-0.076
(State Clustering)	(4.97)***	(3.98)***	(4.21)***	(4.01)***	(5.90)***
(State-Shock Clustering)	(5.70)***	(4.05)***	(3.28)***	(4.45)***	(5.27)***
No. of Observations	67,492	38,801	61,723	78,626	84,664
Pct. of Total Observations	46.5%	50.0%	45.9%	47.2%	47.2%
Removing All Texas Firms:					
Shock_Top1ChairOnly	-0.010	-0.014	0.004	-0.060	-0.074
(State Clustering)	(5.00)***	(3.73)***	(3.84)***	(3.87)***	(5.58)***
(State-Shock Clustering)	(5.70)***	(3.81)***	(2.99)***	(4.27)***	(4.96)***
No. of Observations	62,895	36,761	57,431	73,598	79,153
Pct. of Total Observations	43.3%	47.4%	42.7%	44.1%	44.1%

	Panel D: Early Period (1968-1981)				
	Capex	R&D	Payout	ChgEmp	SalesGr
	(1)	(2)	(3)	(4)	(5)
Including All Firms:					
Shock_Top1ChairOnly	-0.020	0.002	0.002	-0.020	-0.023
(State Clustering)	(10.01)***	(3.23)***	(4.60)***	(4.63)***	(3.86)***
(State-Shock Clustering)	(10.00)***	(1.61)	(4.55)***	(2.00)**	(3.84)***
No. of Observations	33,503	16,687	29,415	38,526	42,344
Pct. of Total Observations	23.1%	21.5%	21.9%	23.1%	23.6%
Removing Oil Firms:					
Shock_Top1ChairOnly	-0.022	0.007	-0.003	-0.042	-0.093
(State Clustering)	(14.35)***	(10.16)***	(5.58)***	(10.00)***	(15.80)***
(State-Shock Clustering)	(5.74)***	(10.15)***	(5.56)***	(8.88)***	(14.79)***
No. of Observations	32,026	16,172	28,123	37,015	40,645
Pct. of Total Observations	22.1%	20.8%	20.9%	22.2%	22.7%
Removing Texas Oil Firms:					
Shock_Top1ChairOnly	-0.020	0.002	0.002	-0.020	-0.023
(State Clustering)	(13.30)***	(3.46)***	(4.48)***	(4.52)***	(3.86)***
(State-Shock Clustering)	(13.24)***	(1.71)*	(4.42)***	(1.94)*	(3.85)***
No. of Observations	32,950	16,441	28,916	37,988	41,744
Pct. of Total Observations	22.7%	21.2%	21.5%	22.8%	23.3%
Removing All Texas Firms:					
Shock_Top1ChairOnly	-0.021	0.002	0.002	-0.019	-0.023
(State Clustering)	(14.07)***	(3.44)***	(4.26)***	(4.23)***	(3.68)***
(State-Shock Clustering)	(13.99)***	(1.75)*	(4.22)***	(1.89)*	(3.67)***
No. of Observations	30,978	15,589	27,150	35,747	39,215
Pct. of Total Observations	21.3%	20.1%	20.2%	21.4%	21.9%

<u>Section II: Point-by-Point Analysis of the Key Claims in Snyder and Welch (2014)</u>

In this section, we lay-out a more in-depth, point-by-point response to each claim brought up in Snyder and Welch. The response to point 1) below contains a more detailed look at all of the analyses we ran in response to Snyder and Welch (which are encapsulated in Table 1). The responses to points 2-5) below are contained in the earlier Section I, but we include them here again for completeness.

1. The first (and main) point of the paper is the claim that the entire results in the paper are driven by the behavior of Texas oil firms during the mid-1980s to early 1990s time period, and by oil firms more generally over this time period, and perhaps even seeping into other firms (due to the S&L crisis). As this was coincident with the powerful committee ascension of Lloyd Bentsen in Texas, the results are driven (yet not caused) by this temporal relationship.
    a. We deal with this economic argument in the cleanest way possible - we completely remove the entire "problem" sample period and observations that are claimed to be driving our full sample results. We run sub-period analyses post-problem period (post-1992) and pre-problem period (pre-1981), both including and excluding Texas, both including and excluding the oil industry, and the results are large, robust, and significant across a range of specifications and outcome variables. These results are shown in Table 1. Table 1 contains multiple panels, including each outcome variable (not only Capex, as was focused on in the critique).
        i. Panel A shows pooled regression results for the full time period (1967-2008). The four separate sub-panels show results: 1.) For all firms, 2.) Removing all oil firms for the entire 41 year sample period, 3.) Removing all TX oil firms for the entire sample period, and 4.) Removing all TX firms for the entire sample period. First, focusing solely on capex (first column), the coefficient magnitude decreases when removing sub-samples of firms, but it only becomes insignificant for one of the four sub-panels (removing all Texas firms for the entire sample period, which is the specification focused on in Snyder and Welch). Further, and importantly, none of the other <u>four</u> outcome measures are significantly impacted by removing *any* of the sub-samples of firms. In fact, a number of the coefficient magnitudes get larger and more statistically significant for many of the sub-samples - for example spending on innovation (R&D) and Sales Growth. It is thus misleading to claim that because a coefficient on one of our five outcome measures decreases to insignificance when removing one particular sub-sample of firms - while many of the other outcome variables increase in magnitude and significance when removing that same sub-sample - that all of the results "go away." Further, Snyder and Welch would need a reason why the relationship they propose only impacts capex, while all of the other variables remain strong and significant (which they do not outline in the critique).
        ii. Panel B shows identical regressions as Panel A, but solely on the sample period outside of the "problem" time period. This panel thus runs pooled regressions for the entire sample period completely removing the problem period – from 1967-2008, excluding (1982-1992). Thus, if – as they claim - that time period were driving all of the relationships we document in the paper, we should find statistically zero coefficient estimates when we completely remove that period, and focus instead on the rest of the sample. As can be seen from Panel B, this is unequivocally not the case. In fact, for every one of the five outcome variables (including capex), the coefficient magnitudes are large and statistically significant. Further, removing any subset of firms (oil firms, TX oil firms, or all TX firms) over has no impact on the results, either in magnitude or statistical significance.
        iii. Panel C shows identical regressions as Panels A and B, but solely for the most recent time period (1993-2008). This time period is properly after the "problem" period proposed by Snyder and Welch. Thus, the same prediction applies here. If Snyder and

Welch are correct in their claim, then this time period that completely removes the period "driving" all of the results should have statistically zero estimated impacts. As can be seen from Panel B, this is again not the case. In fact, for every one of the five outcome variables (including capex), the coefficient magnitudes are larger and more statistically significant in this recent period. Given that this most recent time period is likely more representative of relationships today and that it utilizes the cleanest data (in terms of having perfectly clean HQ data, which we discuss in Point 3 below), we take comfort in that this is where the relationships are most strongly and cleanly empirically identified. Further, removing any subset of firms (oil firms, TX oil firms, or all TX firms) over this most recent time period has no impact on the results, either in magnitude or statistical significance.

iv. Panel D then shows identical regressions, but for the period completely pre-dating the "problem" time period (1968-1981). The same prediction applies here. If Snyder and Welch are correct in their claim, then this time period properly before the problem time period should have statistically zero estimated impacts. Again, as seen in Panel C, this is not the case. One caveat of this time period deserves note, however. As Snyder and Welch have chosen to focus on only one definition (the most narrow) of top committees we use in the paper (the Top1 Committee), there are only 6 exogenous shocks to this over the entire 41 year sample period. Further, only 1 of these shocks occurs in the pre-date period. Thus all of the coefficient estimation for each outcome variable for this pre-period is based on this single shock. This being said, from Panel C, the coefficient estimates are large and significant, and are again insensitive to removing any of the sub-samples (all oil firms, TX oil firms, or all firms in TX).

b. Taking these results as a whole, and putting it simply, if the main critique is that one time period is driving our results, and we completely remove this time period, re-run the results pre- and post-, with the results being strong and robust before and after (in fact stronger in the recent period), how can the results be "driven" by this time period? We were left asking the same question. Going further, even running the mis-specified regression from their critique only impacts one of the five outcome variables from the paper (some of the others actually become stronger and more statistically significant). In a nutshell, these simple facts neutralize the focal point of the critique document. The remainder of the point-by-point section will now go on to address some of the ancillary, smaller points they bring up.

2. Clustering
   a. Snyder and Welch argue that clustering the standard errors at the state level in the basic panel regressions is critical to the inferences that can be drawn. Their argument is that shocks occur at the state level (not state-year level, as we cluster in the paper), so that this is the right measure at which to assume independent variation. First, as we show in Table 1 (all panels), the statistical significance is robust, and virtually identical, across nearly every specification even after clustering by state. It is only in <u>one</u> specification (examining capex, for the full sample, for the Top 1 shock only dependent variable---coincidentally the only specification that Snyder and Welch focus on), where there is even a modest impact of clustering; and even in that specification, the *t*-statistic on the shock variable is still 1.86 and significant at the 10% level. In Table 1, for every regression run for each outcome variable, we show and compare this state level clustering to state-shock clustering of standard errors. We view this to be the more correct way to calculate standard errors for our estimates, as the state-shock period is the actual unit of variation in our analysis (we had included these in the paper originally along with state-year clustering but were encouraged to remove them by discussants who thought they didn't add much, and were confusing to explain to readers). In any case, as can be seen from Table 1, state level clustering, state-shock period level clustering, and state-year level clustering (shown in the original paper) lead to identical statistical inference in nearly all cases.

3. Coding of Shocks and Impulse Response
    a. As we show definitively in our tests above, the behavior of firms in Texas cannot explain the findings in our paper. Snyder and Welch essentially agree that this statement is true for every single outcome variable in our paper, except for capex. Why their alternative story would only apply to capital expenditures and not to the other outcome variables we study in our paper is a puzzle. Rather than try to address this strange inconsistency, Snyder and Welch come up with a new explanation for why the results for the other outcome variables must be wrong. In particular, they claim that it is due to the way shocks are "coded" in the paper. Note of course that this coding issue, as they readily admit, does not impact the capex results (that is instead "due to Texas"), but only these other outcome variables. Again why this strange asymmetry would exist is never explained.
    b. Regarding the coding of the shocks, one of the key contributions of our paper is to employ an exogenous measure of government spending. To do so, we compute shocks for a fixed length of time for all shocks (Top 1, Top 3, Top 5, Top 10, plus the shocks including the ranking members). The idea behind a fixed shock length was to ensure that the shock did not vary as a function of future election outcomes. Only ascendency is truly exogenous (does not rely on the ascending Senator's election). By contrast, continuation relies on re-election and departure can only occur if the Senator resigns or is voted out of office, and both are tied to endogenous economic conditions. We used a 6-year shock to match a Senate term, but in the online Appendix Figure A1 of the original paper, which was again ignored by Snyder and Welch, we showed that our results were not sensitive to the length of the shock period. In this Appendix Figure A1 we show an impulse response that shows response by shock-year so readers can understand the impact of shortening or lengthening the shock horizon. We would also note that we presented this paper at over a dozen schools, at many of the top conferences in the profession (NBER, WFA, FRA, etc.), and had 3 referees and 6 discussants carefully review the paper; clearly a large cross-section of the profession felt that our shock construction was a sensible approach.
    c. By contrast, Snyder and Welch employ an endogenous coding approach that codes a shock as valid for a Senator's entire time as Chairman. Why Snyder and Welch felt it was important to show that some (but importantly not all) of the outcome variables we study are sensitive to using a more endogenous shock variable than the one we employ in our paper is again puzzling to us.

4. Pre-shock placebo test and HQ Data
    a. Snyder and Welch point out: *"For an exogenous shock to be plausible, it is important to test whether this exogenous shock also appeared to have an effect in placebo years before the event.*"
        i. We couldn't agree more, which is why we have this exact variable (Pre Shock) in Table V in the paper.
        ii. In addition to this, in Figure A1 of the paper, we show impulse responses for all of our outcome variables rolling for a number of years following the shock.
    b. State-by-state variation
        i. In the published paper, we were also quite interested in the state-by-state variation in the effect, and wanted to ensure that no one state was driving the results. We ran analyses for all of our outcome variables state-by-state (such that each state gets the exact same weight, nullifying any overweighting of states). We show that our results are strong and robust using this cross-sectional equal-weighting by state method. This is Appendix Table A4 in the published paper. It is ignored in Snyder and Welch.
    c. HQ Data and Compustat
        i. We use the COMPUSTAT database for our paper. One unfortunate component of this data that all papers using COMPUSTAT face - including ours - is that COMPUSTAT backfills corporate HQ location data. What that means is that if a firm has been publicly traded since 1960 and was headquartered in NY from 1960-2000, but changes its HQ

location in 2000 to CA, COMPUSTAT goes back to the 1960-2000 data and backfills the HQ location to CA for all years that the firm was publicly traded (including 1960-2000). Fortunately, HQ moves happen rarely, and we mention this caveat in the published paper.

ii. Since the time of our publication, COMPUSTAT has come out with a new product called the COMPUSTAT Snapshot Database. This data provides point-in-time reported data (not backfilled) going back to 1991 for all publicly traded firms. We obtained this full data going back to 1991, and created a cleaned-HQ version of our dataset. It is with this new, cleaner data that all analyses in this document are run. Fortunately, as headquarter moves are fairly rare, the results we present here are very similar to those presented in the original JPE paper; some of the results from the paper are now statistically even stronger with this perfect headquarters location data (as opposed to the noisier data we had used in the published version). Note again however that COMPUSTAT's Snapshot database only goes back to 1991, and so all data before 1991 is subject to this same back-filling issue. In the new version of data (posted on the JPE's site and on our websites), we use the point-in-time, perfectly accurate data back to 1991, and then use the 1991-listed headquarters for all firms from that point backwards in order to get the best possible measure of headquarters location.

5. Economic Magnitudes
   a. The critique has an entire section where it calculates the magnitude of a hypothetical multiplier using only earmark dollars as the shock denomination. We agree with the critique that this is an incorrect exercise – which is exactly why we have an entire section of the paper that says exactly this. Further, in addition to discussing the fact that other flows of government funds could be related to these shocks, we explicitly show empirically that broader measures of government transfers along with the awarding of government contracts are related to these same exogenous shocks (Table III in the published paper). Snyder and Welch ignore both the empirical results and the discussion section of the paper regarding these issues.
   b. More importantly, as we explicitly note in the discussion section of the original paper (on pg. 1056), the results in the paper cannot be used to compute a credible estimate of the government multiplier, since the effects in the paper 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.) as well. Why Snyder and Welch seem intent on performing an incorrect and misleading calculation, when the original published paper already discusses the challenges around doing so, is a puzzle to us.

Section III: History of the Ongoing Interaction with Snyder and Welch

As noted above, this is not our first interaction with Snyder and Welch about our paper. They first contacted us with concerns regarding our paper 20 months ago. We spent a lot of time re-running our tests to ensure that they did not find some mistake in our implementation or results that so many others had missed. We recreated the data from scratch for them, and we presented Snyder and Welch with an exhaustive set of results and responses to their concerns.

However, Snyder and Welch then returned 20 months later with another disjoint set of concerns. Earlier in this document we articulated why essentially all of their new critiques (much like the set 20 months ago) are also incorrect, a function of completely subjective choice, or simply don't hold water in the actual data. But for completeness, and so readers can appreciate the history of interaction on this matter, we now attach in this section: a) our initial point-by-point response to their *prior* critique document; b) the prior critique document itself (dated January 2, 2013); and c) their newly *revised* critique document (dated September 12, 2014).

Original Response to Snyder and Welch (Part 1—January 2013)

Here are our responses to your concerns with the paper. We have included a table below that summarizes the results of the tests we conducted. In short, we re-downloaded Compustat, fixed the coding error for Louisiana that you pointed out, and re-ran all of our tests with and without Texas, with state-clustering of standard errors, and with state-shock clustering of standard errors.[1]

Table 3:

1. Original CAPEX result loses statistical significance when state clustering of standard errors is used.

   As you can see, all five major coefficients remain significant with state clustering of standard errors.

2. Coefficients become smaller in magnitude when historical coding of Senate chairmanship is used.

   We were strongly encouraged by our referees to use a fixed shock length (i.e. one that does not vary as a function of future election outcomes) because of endogeneity concerns. Only ascendency is truly exogenous (does not rely on the ascending Senator's election), whereas continuation relies on re-election and departure can only occur if the Senator resigns or is voted out of office. We used a 6-year shock to match a Senate term, but in the online Appendix Figure A1 we showed that our results were not sensitive to the length of the shock period.

[1] Although it leads to the same conclusions, we're including state-shock clustering of standard errors here because we view this to be the correct way to calculate standard errors for our estimates (we had these in the paper until the final version but were encouraged to remove them by referees who thought they didn't add much, and were confusing to explain to readers).

Table 4:

3. When states that had a Finance Committee chairman at some point are individually included in a regression along with the 41 states that have never had a chair, only 3 of the 8 states have a significant negative coefficient.

   On page 1052 of our paper, we discuss results of online Appendix Table A4, where state-level regressions are run for each of the 47 states that had at least one top-10 chair. The average coefficients are similar to those from the panel regressions and the fraction that are the correct sign is significantly greater than 0.5 for 4 of the 5 variables. In these regressions (as well as those that use only the Senate Finance committee), Texas is not the largest coefficient (in Table A4 it is similar to the average coefficient).

Table 5:

4. For Texas, coefficient suggests an implausibly large amount of crowding out. More generally, earmarks seem too low to have these kinds of effects and literature (e.g. Levitt and Poterba (1999)) finds little evidence that earmarks are the "tip of a bigger iceberg."

   Columns 10 and 11 of Table 3 suggest earmarks are indeed the "tip of a bigger iceberg." As we discuss in page 1032 of the paper, the coefficients suggest funding increases that are roughly 10x the earmark increases.

Figure 1:

5. Low CAPEX in Texas during Bentsen's chairmanship was preceded by a large spike in oil prices.

   In the state-level regressions employed in Table A4, Texas' CAPEX coefficient is not different than the average state's CAPEX coefficient.

Table 6:

6. Removing Texas from the original paper's regressions eliminates the CAPEX effect.

   While it is true that removing Texas lowers the magnitude of the CAPEX coefficient, it does not change the other 4 variables and, even so, the CAPEX coefficient remains statistically significant.

7. Removing Texas and using historical Senate coding of chairmanship flips sign of coefficient.

   Related to #2 regarding the use of historical Senate coding.

Table 7:

8. R&D result remains if Texas is removed but goes away if historical senate coding is used.

   Related to #2 regarding the use of historical Senate coding. See attached table for the R&D result after removing Texas.

Table 8:

9. Number of Employees result remains if Texas is removed but goes away if historical senate coding is used.

   Related to #2 regarding the use of historical Senate coding. See attached table for the Employees result after removing Texas.

Table 9:

10. Payout result becomes statistically insignificant when Texas is removed and historical senate coding is used.

    Related to #2 regarding the use of historical Senate coding. See attached table for the Payout result after removing Texas.

Table 10:

11. Sales growth result remains if Texas is removed but goes away if historical senate coding is used.

    Related to #2 regarding the use of historical Senate coding. See attached table for the Sales result after removing Texas.

Figure 2:

12. If historical senate coding is used but moved back in time, coefficients increase in magnitude, suggesting capex declines prior to chairmanship term.

    See above regarding the use of historical Senate coding. From the original paper's tables in the regressions that employ the pre-shock variable, if our original coding is used, no significant results obtain if the shock is moved back in time by 6 years (i.e. now covers the 6 years immediately prior to the Chairman's ascendency). The pre-shock coefficient after fixing the Louisiana shock, and with state-clustered standard errors, is -0.001 (t=0.18).

The Impact of Senate Seniority Shocks on Corporate Investment, Payouts, Employment, and Sales

This table reports panel regressions of firm-level capital expenditures (Capex), research and development (R&D), payouts (cash dividends plus repurchases), change in employment, and sales growth on Senate seniority shocks. Only shocks to the Top 1 committee (Senate Finance Committee) chairmanship are used. All models contain firm-fixed effects and year-fixed effects. Columns 1-3 also include controls for lagged Q, cash flow, and lagged leverage. All standard errors are adjusted for clustering at the state or state-shock 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: Full Sample Period (1967-2008)				
	Capex	R&D	Payout	ChgEmploy	SalesGrowth
	(1)	(2)	(3)	(4)	(5)
Including Texas:					
Shock_Top1ChairOnly	-0.010	-0.005	0.003	-0.012	-0.020
(State Clustering)	(1.90)*	(2.26)**	(5.55)***	(2.11)**	(4.09)***
(State Shock Clustering)	(2.37)**	(2.79)***	(5.79)***	(2.12)**	(5.02)***
Removing Texas:					
Shock_Top1ChairOnly	-0.002	-0.005	0.003	-0.018	-0.021
(State Clustering)	(2.00)*	(1.61)	(6.61)***	(3.71)***	(2.57)**
(State Shock Clustering)	(1.81)*	(2.01)**	(5.79)***	(3.85)***	(2.81)***

	Panel B: Earmark Sample Period (1991-2008)				
	Capex	R&D	Payout	ChgEmploy	SalesGrowth
	(1)	(2)	(3)	(4)	(5)
Including Texas:					
Shock_Top1ChairOnly	-0.010	-0.006	0.003	-0.029	-0.047
(State Clustering)	(4.36)***	(1.44)	(4.60)***	(6.46)***	(5.59)***
(State Shock Clustering)	(5.02)***	(1.84)*	(3.69)***	(4.98)***	(5.45)***
Removing Texas:					
Shock_Top1ChairOnly	-0.007	-0.005	0.003	-0.031	-0.039
(State Clustering)	(5.57)***	(1.00)	(4.57)***	(5.63)***	(7.81)***
(State Shock Clustering)	(5.80)***	(1.29)	(2.89)***	(4.60)***	(7.00)***

Original Response to Snyder and Welch (Part 2—January 2013)

Data Construction:

We looked into the data issues you mentioned.

1) With regard to the differences between the datafile we initially sent you (finaljpe.dta) and the file we sent yesterday (ivo_jason.dta), here is what is going on:
    a. We fixed the Louisiana mistake, which we had miscoded as not having a shock for the first few years of our sample. This error occurred because the shock occurred before our Compustat data began, and we didn't merge this in correctly. This is now fixed.
    b. We re-downloaded all of the data for the full period from Compustat. This resulted in more data for 1967 especially, since we had cut off our sample prematurely, and more data for 2008, since Compustat back-filled some additional data. The attached excel spreadsheets show the counts by year, so you can see where the differences come from. The new file actually has about 4,000 *less* observations, not more.
    c. Now, where do these differences come from? It turns out that most of the differences come from the raw CRSP/Compustat files we downloaded, back in 2009 and again this month. These counts are compared in columns G (new) and H (old). After downloading the files from WRDS, all we did was remove rows with missing STATE variable. Then we counted rows that have PERMNO by each year. As you can see, the differences between column B and column C are very similar to differences between column G and H. The large difference in 1967 is due to our mistake of truncating the early sample too much previously, and also the relatively large difference in 2008 is probably due to more observations added to the database later on. But there are some substantial differences in other years as well.
        i. Specifically, we used the same procedure to download the CRSP/Compustat merged files, back in 2009 and again this month. All we did was to go to the CRSP/Compustat merged database webpage, go to Fundamentals Annual and use the default linking options provided there. Perhaps there was a subtle change in the default linking options provided by WRDS since then. More importantly, the linking database is updated almost every month, practically altering the way the merging between CRSP and Compustat databases is conducted. It's been almost 3 years, so a substantial number of changes must have accumulated. Below are links to <u>just a few</u> of those updates:
            1. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2012/ccm_201201_quarterly.pdf
            2. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2012/ccm_201204_quarterly.pdf
            3. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2012/ccm_201207_quarterly.pdf
            4. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2012/ccm_201210_quarterly.pdf

5. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2011/ccm_201101.pdf
6. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2011/ccm_201104_quarterly.pdf
7. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2011/ccm_201107_quarterly.pdf
8. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/2011/ccm_201110_quarterly.pdf
9. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/annual/2011/data_edits.pdf
10. http://www.crsp.chicagobooth.edu/documentation/pdfs/relnotes/annual/2010/201007_annual_data_edits.pdf

ii. We don't think it's practical to go through all these files (listed above are just a fraction) and check how they affected our files.

iii. After taking account into differences that come from the CRSP/Compustat merged files themselves, the remaining differences are quite small. Even these remaining differences are again largely due to the change in the merged files themselves.

2) With regard to the coding of the shocks, here are the specific rules. Again, we preface this by saying that each of these rules was known to the (3) referees on this paper, and represent the preferred specifications of the referees:

   a. Shocks are applied in the year when a chairman ascends to the top spot.
   b. Shocks are applied for 6 years always.
      i. Again, as we noted earlier, we were strongly encouraged by our referees to use a fixed shock length (i.e. one that does not vary as a function of future election outcomes) because of endogeneity concerns. Only ascendency is truly exogenous (does not rely on the ascending Senator's election), whereas continuation relies on re-election and departure can only occur if the Senator resigns or is voted out of office. We used a 6-year shock to match a Senate term, but in the online Appendix Figure A1 we showed that our results were not sensitive to the length of the shock period.
   c. Shocks are only applied to firms alive in the year of the ascension.
      i. This explains why the percentage of firms shocked in a given state declines from 100% in the shock year to a lower number over time.
   d. If the same state is already "being shocked" from another chairman event (e.g., Top 3) in the past 6 years, then we don't apply another shock starting in this year and lasting for another 6 years. The idea here being that we don't want to shock a state forever. This helps to explain your remaining concerns:
      i. The KS shock is missing in 1981 and subsequent years because there's already an ongoing, lesser shock which started in 1979 for KS, when Bob Dole became the ranking member (but not the chair) of the Finance committee in 1979. In

such cases, we didn't upgrade the ranking member shock to a chair shock, nor did we extend the length of the shock.

ii. Again, consistent with how we treated KS in 1981, the IA shock wasn't initiated in 2003 because there was an ongoing ranking member shock for IA when Grassley became the ranking member of the Finance committee in 2001.

iii. And finally, we didn't initiate a shock for the state of Oregon in 1995 because Packwood resigned from the Senate in September of 1995 when "faced with the prospect of public Senate hearings and a vote to expel": (http://www.washingtonpost.com/wpsrv/politics/special/clinton/congress.htm#packwood) . We discovered this event because we had RAs go back and manually hand-code the reason for each Congressman's removal or "loss of chairmanship" for each shock in our sample. This allowed us to remove a few rare instances like this Packwood case, where we deemed that because of unusual circumstances, a particular shock was not valid in our sample. This represented many, many hours of hand-coding.

Do Powerful Politicians Cause Corporate Downsizing?

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January 2, 2013

Abstract

Although the identification approach and quasi-first-stage finding (that Senate finance committee chairmanships predict earmarks) are important contributions, Cohen, Coval and Malloy (2011) are incorrect in concluding that chairmanships have crowded out corporate capital expenditures. The evidence of conditional downsizing disappears if we cluster the standard errors by state. It disappears if we attribute the high capital expenditures of Texas firms in the early 1980s to the 1980s oil price rise and not to Lloyd Bentsen's Senate Finance chairmanship in 1987. And it disappears if we take into account that capital expenditures are a stable variable.

In a lead article in the JPE, Cohen et al. (2011), henceforth CCM, introduce a new empirical approach to identify shocks to Federal government spending through Congressional chairmanship changes. Their quasi[1]-first-stage (in their Table 3) shows that Congressional seniority shocks associate with more earmark spending. This is important in itself. CCM then show that the appointment of a state representative to a senior Congressional (and especially to the U.S. Senate finance chairmanship) caused a decline in private corporate capital expenditures, as well as in some other economic indicators (R&D, employment, etc). CCM interpret this as government crowding out of private corporate investment.

The authors were generous in sharing their data, thereby making it possible for us to take a second look at their results. Our own examination suggests that the results in the original paper do not hold up. In increasing order of importance, the critical issues are:

1. The CCM results are not robust to state-level clustering of standard errors. This is important because the treatment (the Senate chairmanship assumption) occurs at a level that affects all firms within the state. By not clustering at the state level, they are essentially assuming close-to-independent errors (see, e.g., Bertrand, Duflo and Mullainathan (2004), Friedman (2011), Siegel (2012)).

2. Of the eight states in which a Senator took over the finance committee, only three states have negative coefficients, and only one state had both a reasonably large number of firms and statistically significant results: Texas. In 1987, Lloyd Bentsen took over the chairmanship of the Senate Finance Committee. From 1980–1982, the average capital expenditures of publicly-traded Texas corporations (quoted as a function of assets) was an astonishingly high 30%. From about 1985 to the mid-1990s, capital expenditures settled around the 10% level. The estimated coefficient is astonishingly large, suggesting that Federal earmarks on the order of $300 million per year could have reduced capital expenditures by about $20 billion.

   However, the lower spending on capital expenditures in the late 1980s coincided not only with Bentsen's chairmanship assumption (and thus any possible Bentsen-caused Federal earmark-caused reduction in expenditures), but also with a decline in the world oil-price. Starting from $15 in 1979, the Texas Intermediate Crude jumped to $40 in 1980. In 1986, it collapsed from $30 to $12. Eliminating Texas from the regressions eliminates any significance.

[1] "Quasi" because they do not use the first stage in most of their subsequent regressions, but rely on the seniority shock itself.

3. (Asset-normalized) capital expenditures are a stable variable. High capital expenditures in one year are strongly associated with high capital expenditures the following year. We can show that corporate capital expenditures are not just low around the year in which the congressional committee chairmanship changed, but also years before. In fact, capital expenditures are lowest four years *before* a state fields the chair of the finance committee.

   Put differently, a CCM test would suggest even more strongly that corporations cut back their capital expenditures in anticipation of senatorial chairmanship four years into the future, than it suggests that corporations cut back in response to congressional chairmanships. Put differently yet again, asset-adjusted capital expenditures *changes* were not negatively effected by senatorial chairmanships.

Our paper would not have been possible without the generous collaboration of the CCM authors. And we are first to admit that it is much easier to critique a paper—having the advantage of working off a specification—than it is to write a novel paper. However, our paper documents that each of these three issues is sufficient to conclude now that there is no relationship between Congressional chairmanships and corporate capital expenditures in the CCM sample. The remainder of our paper intends to provide the best inference on their key specifications in a straightforward manner.

I Data

Our data is mostly identical to that used in Cohen et al. (2011). Thus, the reader can consult their descriptive statistical tables (Table 1-3). Our abbreviated recap is in Table 1. Of course, CCM have to make a number of choices, like any other empirical paper. Three aspects are worth noting. First, various data constraints eliminate about two-thirds of all CRSP firms. Second, the headquarter of firms is a crude proxy for the location of all the firms' operations. The proxy error-in-variables problem makes it all the more surprising that their findings turned out significant. Unfortunately, there is no good publicly accessible data set that allows us to improve on their variable—they have used the best variable that can be reasonably constructed. Third, we corrected Senate chairmanship coding, as indicated in Table 2. This coding matters in some results, but not in others.

[Insert Table 2 here: Senate Chairman Recoding]

II Results

A Residual Clustering

[Insert Table 3 here: The Effect of State-Level Standard Error Clustering]

In Table 3, we replicate the key result in Cohen et al. (2011, Table 4, models (1) and (2)). The left two regressions use the same data as the original paper and thus have identical coefficients . When we use our own corrected chairmanship indicator (in the right two regressions), the model (1) coefficient drops from –0.012 to –0.009, the model (2) coefficient drops from –0.009 to –0.008. The statistical significance with state-year level clustering of standard errors remains. It also remains with year-level clustering and becomes stronger with firm-level clustering.

However, the statistical significance disappears if we allow for broader clustering, specifically state-level clustering.[2] This suggests that the fact that firms in some states always have higher capital expenditures than firms other states influences the reliability of the interesting coefficient on Senate chairmanship. Although we would argue that state-level clustering is appropriate, this is a judgment that we can leave to the reader.

B Texas

[Insert Table 4 here: States with Senate Chairmanships]

In Table 4, we break out the eight states that had Senate chairmanships in the sample period, and compare each to the 41 states that never had the chairmanship. Five states had positive coefficients, with Louisiana having significantly positive coefficients and Kansas having marginally significantly positive coefficients. The biggest coefficient in absolute magnitude occurred in Louisana.

Of the eight states, three states have the negative coefficient emphasized in CCM, with Montana and Texas having significantly negative coefficients. Not surprisingly, Montana loses this significance with firm-specific clustering, because Montana is home to only 16 companies in the data. Only Texas has a strong robust negative coefficient based on the behavior of many firms. Table 5 helps assess the economic magnitude of the coefficient,

[2]Cohen et al. (2011) did use state-level clustering in their quasi-first stage earmark regressions, but not in their key second-stage regressions.

admittedly generous mixing of expectations with ratios and levels and changes. The coefficient implies a reduction in capital expenditures for Texas firms of about 1.85 percent of assets (with controls). Corporate expenditures were typically about 8% of assets among Texas firms, which means that the assumption of the chairmanship roughly reduced capital expenditures by about one quarter (–1.85/8). Table 4 shows that Texas firms had about $80 billion in capital expenditures per year and annual earmarks of about $321 million (about 0.4% of Texas' firms total capital expenditures). The coefficient implied reduction of $20 billion in private capital expenditures with changes in earmarks that averaged $0.3 billion per year seems completely out of proportion. One might wonder whether earmarks could be the tip of a bigger iceberg, proxying for other Federal flows. Earmarks were small (Carroll (2010)) until the mid 1990s. However, other academic papers do not suggest that earmarks and Federal flows are similar. Levitt and Poterba (1999) find that chairmenships do not have a strong influence on Federal flows, although having senior senators is correlated positively (not negatively) with economic growth.

[Insert Figure 1 here: Time-Series Plot of Texas Firms' Asset-Normalized Capital Expenditures]

The upper plot in Figure 1 plots the asset-normalized capital expenditures of Texas and of all other states from 1980 to 2000. Texas corporations had unusually large capital expenditures from 1980 to 1982. By about 1985, Texas corporations had just come more in line with other states. Conveniently, in 1986, Texas corporations had exactly the same average capital expenditures as other states. In the years in which Lloyd Bentsen held the chairmanship, 1987–1992, Texas corporate capital expenditures again edged above those of corporations from other states. The positive coefficient in the CCM regression is primarily due to the fact that capital expenditures were high from 1980 through 1982.

The question is then whether there is another possible explanation for the decline in Texas capital ependitures. The lower plot shows that the price of West Texas Crude rose from from $14 in 1977 to $39.50 by 1980, remaining above $25 until 1986, when it collapsed back to $15. We leave it to the reader to judge whether Texas firms spent more on asset-normalized capital expenditures in 1980–1982 than after 1986 because the oil price had retreated from its earlier doubling, or whether Texas firms spent more on asset-normalized capital expenditures in 1980–1982 than after 1986 because Lloyd Bentsen took over the chairmanship of the Senate finance committee in 1987.

[Insert Table 6 here: CCM Regression without Texas]

In Table 6, we repeat the regression in Table 3 without Texas. The coefficient without Texas drops from –0.94 to –0.03. With our own historical U.S. senate coding, the coefficient even switches sign. Without Texas, the CCM specification suggests no crowding out. Not reported, if we run the regression excluding the years of the oil boom (1979 to 1985), the results in Table 6 are unchanged.

C Other Dependent Variables

Similar problems apply to other specifications in CCM that find that government involvement crowds out R&D, employment, and sales growth:

- In Table 7, we can replicate the CCM coefficient of –0.45 (their Table 6A), with statistical significance at state-year level clustering. This coefficient flips sign when we use the historical U.S. senate coding, although it is not significant.

[Insert Table 7 here: Explaining Research and Development]

- In Table 8, we can replicate the CCM coefficient of –0.90 (their Table 6C). This coefficient flips sign when we use the historical U.S. senate coding, although its statistical significance depends on the clustering method for the standard errors.

[Insert Table 8 here: Explaining Changes in Employment]

- In Table 10, we can replicate the CCM coefficient of –1.49 (their Table 6D). This coefficient flips sign when we use the historical U.S. senate coding, although its statistical significance depends on the clustering method for the standard errors. Moreover, R&D is a stable variable, an issue that we will address next.

[Insert Table 10 here: Explaining Sales Growth]

[Insert Table 9 here: Explaining Total Payout]

We can confirm that corporate payouts in our Table 9 remain positive, regardless of specification (their Table 6C). However, without Texas and with historical U.S. senate coding, the statistical significance disappears. Moreover, it is not clear whether corporate payouts in any given year are good or bad, or why government earmarks would crowd out corporate payouts.

D Autocorrelation of Asset-Normalized Capital Expenditures

[Insert Figure 2 here: CCM Regression Coefficient Using Different Leads and Lags]

We have left the most important problem for last. Asset-normalized capital expenditures are a stable variable. Firms that have high capital expenditures in one year tend to have high capital expenditures in the following year. The dependent variable in CCM is the asset-normalized capital-expenditure level, not its change. In Figure 2, we repeat the CCM test, except that we lag and lead the chair assumption event by a number of years. On the left of year 0, the coefficient measures the capital expenditure response in anticipation of future Senate chairmanship. On the right of year 0, the coefficient measures the capital expenditure response with a lag of future Senate chairmanship. The CCM coefficient is its most negative four years prior to the assumption of Senate chairmanship. This suggests that there is no evidence that the assumption of chairmanship had any unusually negative effect on corporate capital expenditures. If we predict (unexpected) changes in capital expenditures, Senatorial chairmanship plays no role.

III Conclusion

Cohen et al. (2011) supported the argument that government intervention can be harmful. It offered the appealing finding that government, as proxied by Congressional finance chairmanships, crowded out the capital expenditures of publicly traded corporations. This evidence was all the more surprising, because few would have expected the chairmanships of a single Congressional committee to have such a strong impact on the capital expenditures of publicly-traded corporations. Many of these firms have operations in states beyond that of their headquarter and the chair senator. Capital expenditures are also highly variable, suggesting that the effect would have had to be huge to make a difference. A casual reading of the text of the earmarks suggests that additional funding, if any, often went to such causes as smoking reduction—hardly the stuff that induces private firms to cut back their investments.

Fortunately (or unfortunately), our paper has shown that the evidence Cohen et al. (2011) does not hold up on second examination. Government intervention may well be harmful (or not), and may well crowd out corporate capital expenditures (or not). But Congressional chairmanship assumptions would seem not to have had any important influence on the capital expenditures of the publicly-traded corporations, one way or another.

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Table 1: Summary Statistics

Variable	Observations	Mean	Sdv	Min	Max
Capital Expenditures / Lagged Assets	168,975	0.0784	0.1006	0	1.0254
Research and Development / Lagged Assets	87,865	0.0777	0.1334	0	1.2750
w/ valid capex	86,870	0.0779	0.1336	0	1.2750
Total Payouts / Lagged Assets	156,724	0.0233	0.0440	0	0.4566
w/ valid capex	154,832	0.0234	0.0440	0	0.4566
Sales Growth	181,489	0.1773	0.4414	–0.7217	3.7279
w/ valid capex	167,000	0.1804	0.4511	–0.7217	3.7279
CCM Finance Chair	217,610	0.0256	0.1579	0	1
w/ valid capex	168,975	0.0253	0.1570	0	1
Real Finance Chair	217,610	0.0310	0.1734	0	1
w/ valid capex	168,975	0.0301	0.1710	0	1
Lagged Q	157,276	1.8223	1.8369	0.3872	30.3963
w/ valid capex	153,348	1.8224	1.8256	0.3872	30.3962
Cash Flow / Lagged Assets	153,626	0.0366	0.2418	–2.4891	0.5739
w/ valid capex	151,482	0.0363	0.2417	–2.4891	0.5739
Lagged Leverage	174,209	0.4461	0.2784	0.0044	0.9865
w/ valid capex	159,833	0.4158	0.2610	0.0044	0.9865

Explanations: The variables were originally from Compustat and generously provided by Cohen, Coval, and Malloy (CCM). Because our data is the same, more statistics can be found in the original paper, Cohen et al. (2011). Firm-level variables start in 1967, state-level variables in 1991. Both data sets end in 2008. We also report statistics for firm-years without valid capital expenditures, because the regressions in our Tables 7–10 follow CCM and do not impose the capital expenditure availability constraint.

Table 2: Coding of Chairmanship of the U.S. Senate Finance Committee

Congress	Year	CCM Coding	U.S. Historical Coding	
90th	1967	None	LA	Long (D)
90th	1968	None	LA	Long (D)
91th	1969	None	LA	Long (D)
91th	1970	None	LA	Long (D)
92th	1971	LA	LA	Long (D)
92th	1972	None	LA	Long (D)
93th	1973	None	LA	Long (D)
93th	1974	None	LA	Long (D)
94th	1975	None	LA	Long (D)
94th	1976	None	LA	Long (D)
95th	1977	None	LA	Long (D)
95th	1978	None	LA	Long (D)
96th	1979	None	LA	Long (D)
96th	1980	None	LA	Long (D)
97th	1981	None	KS	Dole (R)
97th	1982	None	KS	Dole (R)
98th	1983	None	KS	Dole (R)
98th	1984	None	KS	Dole (R)
99th	1985	OR	OR	Packwood (R)
99th	1986	OR(79%/97%)	OR	Packwood (R)
100th	1987	OR(72%/77%) and TX	TX	Bentsen (D)
100th	1988	OR(66%/69%) and TX(91%/100%)	TX	Bentsen (D)
101th	1989	OR(62%/64%) and TX(83%/90%)	TX	Bentsen (D)
101th	1990	OR(52%/59%) and TX(76%/83%)	TX	Bentsen (D)
102nd	1991	TX(71%/76%)	TX	Bentsen (D)
102nd	1992	TX(65%/71%)	TX	Bentsen (D)
103rd	1993	NY	NY	Moynihan (D)
103rd	1994	NY(89%/100%)	NY	Moynihan (D)
104th	1995	NY(85%/90%)	OR	Packwood (R)
104th	1996	NY(74%/81%) and DE	OR	Packwood (R)
105th	1997	NY(69%/72%) and DE	DE	Roth (R)
105th	1998	NY(65%/67%) and DE(94%)	DE	Roth (R)
106th	1999	DE(94%/90%)	DE	Roth (R)
106th	2000	DE(90%/88%)	DE	Roth (R)
107th	2001	DE(84%/88%) and MT	MT	Baucus (D)
107th	2002	MT	MT	Baucus (D)
108th	2003	MT	IA	Grassley (R)
108th	2004	MT(83%/100%)	IA	Grassley (R)
109th	2005	MT(83%/83%)	IA	Grassley (R)
109th	2006	MT(80%/80%)	IA	Grassley (R)

Explanations: The CCM state dummies are backed out from the CCM panel data. When present, the percentage figures behind the state name are the number of firms from this state in this year that have the Senate Chair dummy coded as 1. The first number is without the filter that capital expenditures are available, the second number imposes the filter. No percentage behind a state name means 100%, i.e., all firms from this state are coded as 1 in this year. The historical U.S. Senate data is from the *History of the Committee on Finance, United States Senate, May 21, 1981, with updates,* http://www.finance.senate.gov/about/history/.

Interpretation: The state-year level correlation (with 2,050 observations) between the CCM coding and the U.S. Senate history coding is 47%. The remainder of our paper will entertain both definitions.

Table 3: The Impact of Seniority Shocks on Corporate Investments (Capital Expenditures / Lagged Assets), in Percent, 1967–2008

	Original CCM Regs		Hist. Senate Coding	
	(1)	(2)	(3)	(4)
Senate Finance Committee Chair	**–1.22**	**–0.94**	–0.87	–0.82
Standard Errors, Clustered by				
State-Year level (as in CCM)	**(0.35)*****	**(0.30)*****	(0.42)**	(0.34)**
Year level	(0.36)***	(0.31)***	(0.44)**	(0.38)**
Firm level	(0.23)***	(0.23)***	(0.26)***	(0.26)***
State level	(0.69)*	(0.62)	(0.73)	(0.63)
Controls	-No	Yes	-No	Yes
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	168,975	139,564	168,975	139,564
CCM Senate Coding	Yes	Yes		
Historical US Senate Coding			Yes	Yes

Explanations: The left two regressions replicate the CCM regressions (although our own figures are quoted in percent [multiplied by 100]). Because we are using the CCM data, the coefficients and (bold-faced) standard errors are not just similar but identical. CCM report state-year clustered standard errors. We boldface the coefficient and standard errors that correspond to those in CCM. We add three more standard error clustering variants. The right two regressions switch to use of the historical U.S. Senate coding of chairmanship, as in Table 2. The controls are lagged Q, cash flow, and lagged leverage. *, ** , and *** indicate significance at the 10%, 5%, and 1% confidence levels, respectively. Gray cells indicate coefficients that drop below statistical significance at the conventional 5% level when the cell's standard error is used.

Interpretation: Because the effect of U.S. Senate Chairmanship is coded into all firms from a state (it is a state-wide effect), standard errors should be clustered at the state level. When they are, the CCM coefficients of –1.22 and –0.94 become insignificant. The coefficients are smaller when we use the U.S. Senate historical coding of chairmanship instead of the CCM coding (Table 2), but the statistical significance levels remain similar.

Table 4: Explaining Capital Expenditures / Lagged Assets, in Percent, By State of Chair, 1967–2008

Panel A: Without Controls

			1986-86					2003-06
	-1996	1981-84	1995-96	1987-92	1993-94	1997-00	2001-02	2007-10
State:	Louisiana	Kansas	Oregon	Texas	New York	Delaware	Iowa	Montana
Senate Finance Committee Chair	+5.05	+1.85	**–0.60**	**–2.10**	+0.16	+0.73	+0.43	**–4.08**
Standard Errors, Clustered by								
State-Year level (as in CCM)	(1.18)***	(0.99)*	(0.73)	(0.58)***	(0.25)	(0.61)	(0.42)	(1.44)***
Year level	(1.28)***	(1.02)*	(0.76)	(0.61)***	(0.27)	(0.58)	(0.45)	(1.44)***
Firm level	(1.74)***	(2.02)	(0.80)	(0.39)***	(0.31)	(1.59)	(0.57)	(3.31)
State level	(0.14)***	(1.85)***	(0.16)***	(0.13)***	(0.18)	(0.10)***	(0.13)***	(0.13)***
Adjusted R^2	48.79%	48.74%	48.61%	49.67%	48.75%	48.54%	48.59%	48.55%
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	131,149	131,233	131,658	145,459	148,183	130,683	131,117	130,249

Panel B: With Controls

State:	Louisiana	Kansas	Oregon	Texas	New York	Delaware	Iowa	Montana
Senate Finance Committee Chair	+5.22	+1.71	**–0.98**	**–1.85**	+0.07	+0.88	+0.38	**–4.58**
Standard Errors, Clustered by								
State-Year level (as in CCM)	(1.22)***	(1.09)*	(0.49)**	(0.44)***	(0.30)	(0.71)	(0.39)	(1.59)***
Year	(1.32)***	(1.07)*	(0.51)*	(0.48)***	(0.36)	(0.71)	(0.40)	(1.62)***
Firm	(2.05)**	(1.66)	(0.76)	(0.40)***	(0.30)	(1.44)	(0.81)	(3.47)
State	(0.18)***	(0.17)***	(0.15)***	(0.10)***	(0.13)	(0.09)***	(0.16)***	(0.10)***
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	48.79%	48.74%	48.61%	49.67%	48.75%	48.54%	48.59%	48.55%
Observations	108,324	108,437	108,809	120,428	122,226	107,992	108,321	107,632

Explanations: These regressions break out the states that had a U.S. Senate Finance Committee Chairman (using the historical U.S. Senate coding, although the CCM definition leads to similar results) and include only the remaining 41 states that never had such a chair. Otherwise, these regression imitate Table 3.

Interpretation: Only three out of eight states (OR, TX, MT) have the negative coefficient described in CCM. Controls do not change the conclusions.

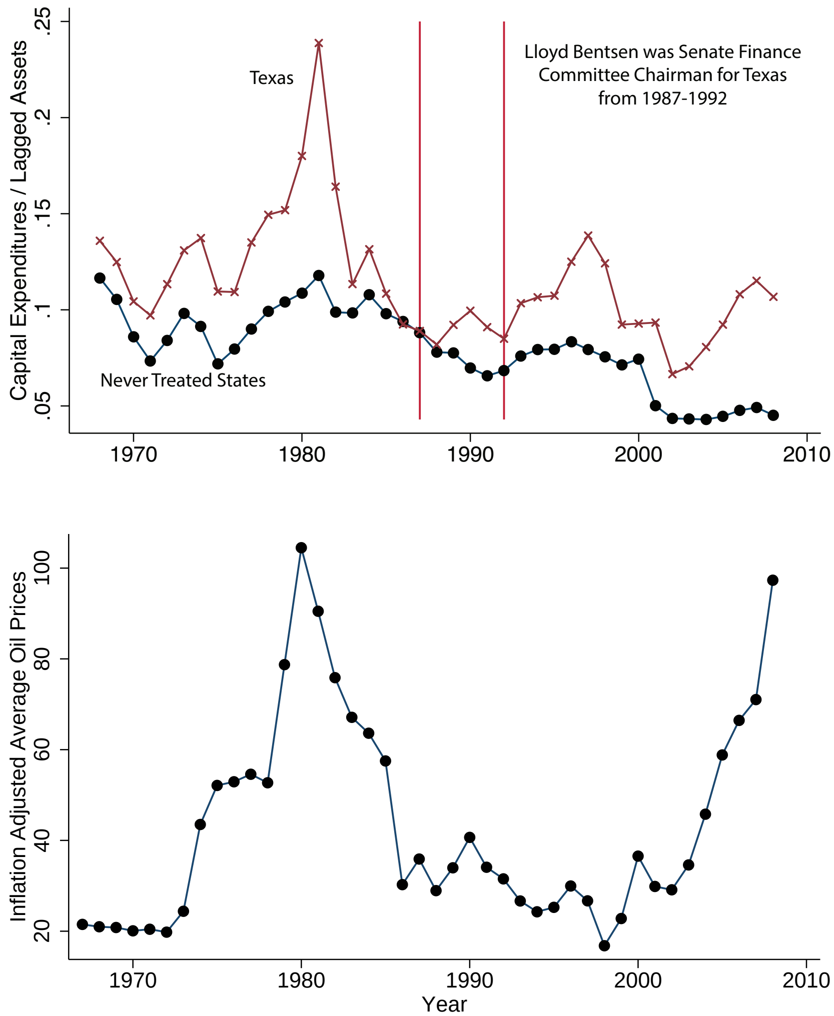
Table 5: Explaining Capital Expenditures / Lagged Assets, in Percent, By State of Chair, 1967–2008

State:	Louisiana	Kansas	Oregon	Texas	New York	Delaware	Iowa	Montana
			1986-86					2003-06
	-1996	1981-84	1995-96	1987-92	1993-94	1997-00	2001-02	2007-10
Senate Finance Committee Chair Coefficient w/o Controls Asset Normalized	+5.05	+1.85	**–0.60**	**–2.10**	+0.16	+0.73	+0.43	**–4.08**
Senate Finance Committee Chair Coefficient w/ Controls Asset Normalized	+5.22	+1.71	**–0.98**	**–1.85**	+0.07	+0.88	+0.38	**–4.58**
Avg Ann Earmarks, in mill 2008 $	153	74	101	321	287	21	101	74
Per-Capita	35.1	28.9	32.3	16.9	15.5	29.6	35.6	86.9
Avg Ann Capex, in mill 2008 $	4,110	3,799	2,406	79,670	60,059	5,451	1,182	256.6
$\overline{\text{Earmarks}}$ / $\overline{\text{Capex}}$	3.7%	1.9%	4.2%	0.4%	0.5%	0.4%	8.5%	29%

Explanations: The first two data lines are the coefficients with controls from Table 4. The following three data lines were copied from CCM, Table 2. The final line is calculated from the relevant lines above.

Interpretation: For Texas, despite Federal earmarks accounting for only $0.321 billion, on average, the implied coefficient suggests a reduction on the other of $20 billion. This seems implausibly large.

Figure 1: Asset-normalized capital expenditures of Texas firms compared to firms in 41 states that never had a Senate Finance Committee Chair



Explanations: The top graph plos the time series of the capital expenditures of Texas firms and of firms from the 41 other states that never had a Senate chair. The bottom graph plots the Western Texas crude oil price.

Interpretation: Texas firms tended to have modestly higher capital expenditures than other states throughout the sample. However, much of the large capital expenditures before the Senate Chairmanship Assumption by Lloyd Bentsen occurred from 1979 to 1983. This was preceded by a large spike in oil prices—a prominent industry in Texas.

Table 6: Explaining Capital Expenditures / Lagged Assets, in Percent, By State of Chair, 1967–2008, With and Without Texas

	Original CCM Regs	Without Texas	
Senate Finance Committee Chair	**–0.94**	–0.03	+0.48
Standard Errors, Clustered by			
State-Year level (as in CCM)	**(0.30)*****	(0.21)	(0.46)
Year level	(0.31)***	(0.24)	(0.36)
Firm level	(0.23)***	(0.23)	(0.31)
State level	(0.62)	(0.08)	(0.47)
Controls	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Adjusted R^2	50.08%	49.18%	49.18%
Observations	139,564	126,651	126,651
CCM Senate Coding	Yes	Yes	
Historical US Senate Coding			Yes

Explanations: These regressions repeat those in Table 3, except that the right-side two regressions omit the state of Texas.

Interpretation: Without the state of Texas, the coefficient is zero. Without the state of Texas *and* the historical Senate coding of chairmanship, the coefficient is positive though not statistically significant.

Table 7: Explaining Research and Development / Lagged Assets, in Percent, By State of Chair, 1967–2008

	With Texas		Without Texas	
	CCM Orig	Historical Sen Cod	CCM Orig	Historical Sen Cod
Senate Finance Committee Chair	–0.45	+0.03	–0.41	+0.32
Standard Errors, Clustered by				
State-Year level (as in CCM)	(0.17)***	(0.17)	(0.24)*	(0.30)
Year level	(0.15)***	(0.18)	(0.21)*	(0.33)
Firm level	(0.30)*	(0.28)	(0.41)	(0.31)
State level	(0.30)	(0.28)	(0.41)	(0.41)
Controls	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	78.24%	78.24%	78.09%	78.09%
Observations	74,842	74,842	70,017	70,017
CCM Senate Coding	Yes		Yes	
Historical US Senate Coding		Yes		Yes

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now research and development (as in CCM, Table 6, Panel A). Following CCM, these regressions use controls.

Interpretation: The coding of who is Senate chair is critical when explaining R&D. With the historical Senate coding, the coefficient is positive.

Jason: Your email says 0.03 is pos. But your excel table shows it is negative. can you please double-check all numbers, not just in this table, but everywhere before we send it?

Table 8: Explaining Change in Number of Employees, in Percent, By State of Chair, 1967–2008

	With Texas		Without Texas	
		Historical		Historical
	CCM Orig	Sen Cod	CCM Orig	Sen Cod
Senate Finance Committee Chair	—0.90	+1.05	–1.02	+1.64
Standard Errors, Clustered by				
State-Year level (as in CCM)	(0.80)	(0.68)	(1.18)	(1.01)
Year level	(0.77)	(0.68)	(1.04)	(1.08)
Firm level	(0.70)	(0.75)	(0.96)	(1.12)
State level	(0.33)***	(0.49)**	(0.43)**	(1.01)
Controls	-No	-No	-No	-No
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	13.51%	13.52%	14.14%	14.14%
Observations	168,267	168,267	153,618	153,618
CCM Senate Coding	Yes		Yes	
Historical US Senate Coding		Yes		Yes

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now the percent change in employment (as in CCM, Table 6, Panel C). Following CCM, these regressions do *not* use controls.

Interpretation: The coding of who is Senate chair is critical when explaining the change in the number of employees. With the historical Senate coding, the coefficient is positive, though not statistically significant.

Table 9: Explaining Total Payout / Lagged Assets, in Percent, By State of Chair, 1967–2008

	With Texas		Without Texas	
		Historical		Historical
	CCM Orig	Sen Cod	CCM Orig	Sen Cod
Senate Finance Committee Chair	+0.27	+0.17	+0.33	+0.16
Standard Errors, Clustered by				
State-Year level (as in CCM)	(0.07)***	(0.06)***	(0.09)***	(0.10)
Year level	(0.07)***	(0.07)***	(0.10)***	(0.10)
Firm level	(0.10)***	(0.09)**	(0.14)**	(0.13)
State level	(0.07)***	(0.07)*	(0.05)***	(0.14)
Controls	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	39.20%	39.20%	38.95%	38.94%
Observations	129,991	129,991	117,845	117,845
CCM Senate Coding	Yes		Yes	
Historical US Senate Coding		Yes		Yes

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now total payout (as in CCM, Table 6, Panel B). Following CCM, these regressions use controls.

Interpretation: Firms seem to pay out more during the chairmanship, although this is not significant if the historical Senate coding is used and Texas is excluded.

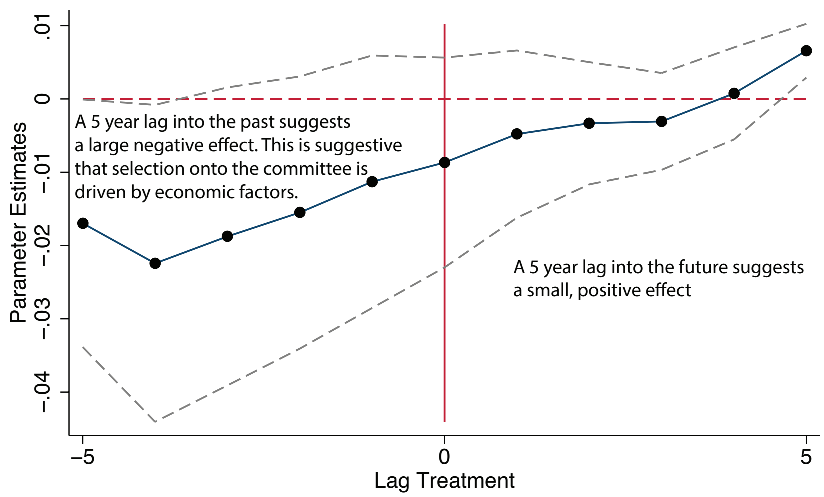
Table 10: Explaining Sales Growth, in Percent, By State of Chair, 1967–2008

	With Texas		Without Texas	
	CCM Orig	Historical Sen Cod	CCM Orig	Historical Sen Cod
Senate Finance Committee Chair	–1.49	+1.72	–1.34	+3.65
Standard Errors, Clustered by				
State-Year level (as in CCM)	(1.15)	(1.32)	(1.21)	(1.39)***
Year level	(1.24)	(1.38)	(1.21)	(1.31)***
Firm level	(0.98)	(1.00)*	(1.32)	(1.40)***
State level	(0.41)***	(0.94)*	(0.84)	(1.41)**
Controls	-No	-No	-No	-No
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	18.14%	18.14%	18.78%	18.78%
Observations	181,489	181,489	165,337	165,337
CCM Senate Coding	Yes		Yes	
Historical US Senate Coding		Yes		Yes

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now sales growth (as in CCM, Table 6, Panel D). Following CCM, these regressions do *not* use controls.

Interpretation: The coding of who is Senate chair is critical when explaining sales growth. With the historical Senate coding, the coefficient is positive. The statistical significance is misleading, because it ignores the variable persistence problem discussed in the next section. We consider it spurious.

Figure 2: Treatment Coefficient With Placebo Leads and Lags



	Before Chair Assumption							After Chair Assumption			
Year	−5	−4	−3	−2	−1	0	1	2	3	4	5
Coef	−1.70	−2.24	−1.88	−1.55	−1.13	−0.87	−0.48	−0.33	−0.31	0.08	0.66
Std Err	0.86	1.10	1.04	0.95	0.88	0.73	0.58	0.43	0.34	0.32	0.19

Explanations: The figure illustrates the CCM coefficients from Table 3 for eleven regressions, where we lead or lag the Senate chairmanship assumption timing. The table below it switches specification, using the historical Senate coding with clustering and without controls, but leads to the same conclusion. In the figure, the coefficient at zero is the original regression coefficient on the Senate Finance Committee Chair variable in Table 3, explaining corporate capital expenditures from 1967–2008. The dashed line indicate the two standard deviation range with state clustering. To the left of zero are identical regressions, except that the chairmanship assumption is pretended to have been x years earlier. To its right are identical regressions, except that the chairmanship assumption is pretended to have been x years later.

Interpretation: The strongest negative coefficient is at –4. If the specification is interpreted as in CCM, this would suggest that capital expenditures declined four years *before* the chairmanship assumption. This is also consistent with Figure 1, where Texas firms had the highest capital expenditures four years *before* Bentsen's assumption of the chairmanship.

Do Powerful Politicians Really Cause Corporate Downsizing?

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September 15, 2014

Abstract

Cohen, Coval and Malloy (2011) suggested that increased government spending crowded out private corporate investment by publicly-traded corporations, as identified by changes in Congressional chairmanships. Our paper shows that this was incorrect. The magnitude of their reported crowding-out is implausibly large. Instead, their inference was due to an omitted variable. The Chairmanship of Texas Senator Lloyd Bentsen from 1987 to 1992 followed a large decline in oil prices from 1980 to 1986. Similar investment reductions also occurred contemporaneously in oil firms and oil states beyond Texas. Our paper also discusses issues, such as standard-error clustering, Senate coding choices, and temporal alignment diagnostics, which carry importance in many of their regressions. The answer to the question in the title is that there is no evidence that powerful politicians caused corporate downsizing.

In their lead article in the *Journal of Political Economy*, Cohen et al. (2011), henceforth CCM, introduce a new empirical approach to identify shocks to Federal government spending and the effects of these shocks on non-state corporate capital expenditures.

CCM assume that being appointed the Chair of a Congressional Committee is a serendipitous (exogenous) event. Changes in chairmanship then become a good shock (instrument) for measuring the influence of government actions on corporate behavior. CCM begin by showing that the appointment of a U.S. state's member of Congress to a committee leadership role associated with more earmark spending to their home states. This leads them to interpret Congressional committee appointment as an exogenous shock for Federal earmark spending.

CCM then use this identification strategy to investigate their key question: Did the appointment of a U.S. Senator to become the Chair of the U.S. Senate Finance Committee (and a Congressman to become the Chair the House Means and Ways Committee) cause a decline in corporate investment in their home states? CCM interpret their data to suggest that this was indeed so. They assume that the link was that government earmarks crowded out corporate investment. Their analysis is based on behavior of publicly-traded Compustat firms, primarily of their capital expenditures and secondarily of their R&D spending, corporate employment, payout, and sales growth. Their findings are all the more remarkable because this channel is not the usual crowding-out channel, in which the taxation necessary to fund government expenses reduces the profitability and thus quantity of private investment. A priori, theory could have allowed government earmarks and corporate expenditures to be *in-state* complements rather than substitutes. Thus, CCM must have identified an even more direct and potentially harmful channel. In turn, the CCM findings have served as the basis of testimony before Congress, and continue to circulate in various blogs and commentaries on the Internet.[1] The peer-reviewed academic answer to this question in CCM remains relevant and influential.

Our own second look at the CCM data finds no evidence of crowding out.

The magnitudes of the estimated coefficients in CCM imply that publicly-traded firms headquartered in the Senator's home state cut back their (world-wide) capital expenditures by orders of magnitude more than the amount of the earmarks. For example, in 1991, *Citizens against Goverment Waste* reported that earmarks were $7 billion to all 50 states,

[1]The congressional testimony on Sep 29, 2010 is in the public record. On August 1, 2014, a google search for "Do powerful politicians cause corporate downsizing" and "blog" suggested at least 4,300 hits, including blogs on heartland.org, reason.org, economist.org, etc.

of which about \$75-100 million (\$170 in 2008-\$) were to Texas. A Senate appointment shock should have accounted for half this. In the CCM model, the coefficient on the appointment dummy implies that Texas firms, with assets of about \$800 billion (in 2008-\$), reduced their capital expenditures by an extra \$10 to \$20 billion (in 2008-\$) due to Lloyd Bentsen's chairmanship. Even if the entire million earmark spending had been due to Bentsen, a reasonable coefficient should not predict a \$10,000 million reduction in capital expenditures for a \$100 million earmark increase. It is simply too large to allow for a plausible interpretation of the coefficient as a measure of causal crowding-out by Senatorial earmark shocks of corporate investment.

Instead, our paper finds that the primary cause of the large negative association between Senate Finance chairmanship appointments and corporate capital expenditures lay elsewhere. It was the fact that Texas Senator Lloyd Bentsen's appointment to the Chair of the U.S. Senate Finance Committee in 1987 just succeeded the oil-price collapse in the mid-1980s (and coincided with the S&L crisis).

We show that the key coefficient is zero without Texas. While only about 5% of U.S. firms on Compustat are in the oil industry, about 32% of the firms in Texas are. The Bentsen appointment just happened to follow the oil price decline and thus just happened to associate with a relatively large reduction in Texas firms' corporate capital expenditures. Because the oil-price collapse began two years earlier, because not all firms are in the oil industry, and because Texas is not the only oil-producing U.S. state, it is possible to disentangle the effect of the Senate chairmanship from an oil-related macroeconomic effect. Our paper documents that during the 1987–1992 Bentsen years (1) Texas oil firms behaved largely like other oil-firms, and (2) Texas averages behaved largely like those from other states with many oil firms. Our paper shows this both visually and parametrically.

Our paper also points out a number of other issues that contributed to the CCM findings and subsequent interpretation, specifically (1) the way standard-errors were clustered, (2) how Senate appointments were coded, and (3) how relatively stable capital expenditures in one year spilled over into the next year (rendereing the identification less of a "shock" as it seemed to be). For an exogenous shock to be plausible, one can test whether this exogenous shock also appeared to have an effect in placebo years *before* the event.[2] Correcting for these three issues also regularly reduces the results reported in CCM to statistical significances that are below conventional levels. The Congressional appointment coding method in (2)

[2] In this context, we consider it implausible that Bentsen's appointment would have had an influence on the world oil price or corporate capital expenditures *two years before his appointment*.

is particularly important for explaining variables other than capital expenditures and for explaining the influence of House leadership appointments.

It is important to reassess existing empirical evidence independently. This is especially the case for CCM, because this paper has not only had continuing academic influence (and publication prestige), but also the potential to influence future economic policy. Our paper contributes to the understanding of the influence of government by documenting that there is *no* evidence of a relationship between Congressional chairmanships and private corporate investment. Moreover, it is implausible that there could be a detectable earmark channel. Our paper takes no stance on the more general question of whether government intervention is harmful or not.

Our paper proceeds as follows. In Section I, we recap the data used in CCM and generously provided to us by CCM. In addition, this section spells out some more details that are required for replication. In particular, we explain the coding of Senate chairmanship appointments in CCM, describe a more parsimonious simple historical coding, and compare the two. This choice matters in some but not all specifications. In Section II, we focus on the association of Senate appointments and corporate capital expenditures. We explain why state-level clustering is appropriate, and why the reported CCM coefficients are orders of magnitude larger than one could reasonably have expected. When we look at the effects of the individual Senate appointments, we find that the CCM results were largely driven by the appointment of Senator Lloyd Bentsen of Texas in 1987. When we investigate the behavior of firms around this time period, we find that Texas was more exposed to the coincidental decline in oil prices than the average state. If we control for oil-industry composition within state, the effect completely disappears. Moreover, the timing difference between the decline in oil prices and the appointment allows us to show that the reduction in capital expenditures began before Bentsen's appointment (which can be viewed as placebo years), and firms from other oil-states cut back investment in the same way that Texas firms did. In Section III, we investigate the CCM secondary variables: R&D expenses, employment, sales growth, and payout. In all cases except corporate payouts, we can replicate the CCM reported coefficients but we find no effect if we apply simple corrections (especially but not only for the historical Senate coding). We also briefly look at their other key independent variables, appointments to other Congressional leadership positions. The results disappear if we use historical coding which does not omit William Reynold Archer's (TX) House Ways and Means Committee Chairmanship from 1995 to 2001. In Section IV, we conclude.

I Data

[Insert Table 1 here: Summary Statistics]

Most of our data was obtained from and is thus identical to that used in CCM. Thus, the reader can consult their descriptive statistical tables (Table 1-3). Our abbreviated recapitulation of the key summary statistics appears in Table 1.

Of course, like any other empirical paper, the CCM article had to make a number of implementation choices. Most (but not all) turn out to be just neutral mutations. Understanding the choices and tradeoffs thus is required to help the reader interpret the CCM study. In particular, a more detailed explanation for their method of Senate coding is necessary to explain the coding alternatives that our own paper is entertaining (in addition to the original CCM coding).

A Chairmanships

The most important finding in CCM regards the effect of the appointment of a Senate Finance Committee Chair on corporate behavior.

The CCM study relies on an unconventional Senate coding system:[3]

- Shocks are applied when a chairman ascends.
- Shocks last for six years (to reduce endogeneity in chairmanship demotions).
- No second senatorial shock is applied when one is already in place.
- Oregon was excluded because Senator Packwood faced Senate sanctions during his chairmanship.
- Louisiana was excluded because the original Compustat data began later.

In addition, they excluded new firms and (firms from) Kansas from 1981 to 1984.

[3] CCM communicated to us privately that some of these choices were strongly suggested by their JPE referees.

The actual historical Senate chairmanships were quite different:

State	Senator	Historical	CCM Coding
Delaware	Roth (R)	1996-2000	1996-2001
Iowa	Grassley (R)	2001,2003-2006	
Kansas	Dole (R)	1981-1984	
Louisiana	Long (D)	1968-1980	1971
Montana	Baucus (D)	2002,2007-2008	2001-2006
New York	Moynihan (D)	1993-1994	1993-1998
Oregon	Packwood (R)	1985-1986, 1995	1985-1990
Texas	Bentsen (D)	1987-1992	1987-1992

Our own paper therefore entertains two coding alternatives and shows both results to let the reader choose:

1. We use the original CCM coding to confirm exact correspondence with the published CCM results.

2. We use what we shall refer to as "U.S. historical coding." This considers *any* firm in the CCM data set to have been treated in years in which its headquarter was located in the U.S. state that fielded the Chair of the Committee.[4]

[Insert Table 2 here: Senate Chairman Coding]

Table 2 shows the resulting coding differences in the firm-year Compustat data set. Our results below will show that the choice of Senate coding plays an important role when explaining R&D, corporate employment, and sales growth, but not when explaining corporate payout. When explaining corporate capital expenditures, the choice of Senate coding plays a contributory but not a central role.

There are similar differences between the CCM coding and a historical coding of House Ways and Means Committee chairmanships. We only briefly consider the House regression in one table in Section III, so we omit the details. They are available upon request.

[4] In the "historical" version, we coded 2001 to Grassley, because he was the chair during the first half of 2001 (when most budget decisions were made), although Baucus then assumed the chairmanship in the second half. This coding choice has little influence on the results we are reporting.

B Corporate Data

The headquarter of a firm is only a crude proxy for the location of all the firms' operations. The measurement-error problem makes it all the more surprising that the CCM findings turned out to be so significant. Because there is no good publicly accessible data set that allows us to improve on the corporate-location variable, we adopted CCM's coding.

One noteworthy aspect in their coding concerns their treatment of new firms (i.e., firms that appeared with new identifiers on Compustat). In their paper, CCM applied chairmanship shocks only to firms that were alive in the year of ascension. Such new firms commonly have larger capital expenditure ratios. For instance, consider Lloyd Bentsen (TX) appointment of the Chairmanship in 1987. The 399 firm-years from (more recently founded) Texas firms had capital expenditure ratios of 12%. In contrast, the 2,247 firm-years that had existed prior to Lloyd Bentsen's chairmanship had capital expenditure ratios of 9%. As a specific example, in 1986, Kinetic Concepts of Texas entered the data set with capital expenditures of $41 million and assets of $163 million. Entering in the same year as Bentsen, neither Kinetic Concepts' 25% ratio in 1986, nor its subsequent ratios were therefore included as Texas firms subject to potential crowding out. Thus, the CCM test hypothesis can be clarified from "government crowds out private capital expenditures" to "government crowds out private capital expenditures of pre-existing firms." However, even in this case, firms like Kinetic Concepts should not only not be coded as treated, but also not be coded as controls. In our own "historical Senate coding," we thus also took the opportunity to code new firms into the state in which they were founded. This can create small differences in results, but does not greatly affect the inference.[5]

Although we can and do replicate the coefficients in their data set perfectly and use it as the base of our analysis, it was impossible to recreate the CCM data set from first principles (and thus the exact same coefficient estimates). This is no fault of their's (or our's). Since the CCM publication in 2011, more data (and possibly corrections) in the Compustat data and in the CRSP/Compustat linkage have become available. These changes have made it impossible to trace individual firms and data points and/or impossible to replicate exact coefficient estimates. When we attempted to construct a new data set from first principles, our self-constructed data broadly suggested that such changes would not greatly alter the

[5]It did not seem wise to remove new firm observations altogether. Although this would have solved the problem for the treated states, it then becomes unclear how to handle new firms in the Compustat data in the untreated states.

inference. Thus, we rely primarily on the original CCM data in order to make the results directly comparable.

II Capital Expenditures

[Insert Table 3 here: Senate Appointments and Capital Expenditures]

In Table 3, we replicate the key result in CCM, Table 4, models (1) and (2). The left two regressions, models (1) and (2), use the same data and coding as the original paper and thus have identical coefficients and standard errors.

A Standard Errors and Senate Appointment Coding

The treatment effect (the Senate chairmanship appointment) occurs at a level that affects all firms within the state. Examining whether firms are affected therefore requires clustering at the state level (see, e.g., Bertrand, Duflo and Mullainathan (2004), Friedman (2011), Siegel (2012), or Serrato and Wingender (2014)). Put differently, because the hypothetical effect is state-wide and persistent (coded into all firms from one state as it is a state-wide effect), the standard errors should be clustered at the state level, not just at the state-year level. A firm that is different in one year from a regression-predicted mean (e.g., during the chairmanship) is not an independently new observation in the next year.

Table 3 shows that the statistical significance on the treatment dummy in Table 3 disappears in the with-controls regression if we do not use their state-year-level clustering but state-level clustering.[6] This suggests that the fact that firms in the same state move together can influence the reliability and interpretation of the coefficient estimates on Senate chairmanship.

Although state-level clustering suggests in a classical inference sense that the coefficient in model (1) is significant while the coefficient in model (2) is not, this should not be over-interpreted. With state-level standard errors, the T-statistic drops from a modestly-significant $-1.22/0.69 \approx -1.8$ in model (1) without controls, to a modestly-insignificant $-0.94/0.62 \approx -1.5$ in model (2) with controls. It is only with the historical (actual)

[6] CCM did use state-level clustering in their quasi-first stage earmark regressions in their Table 3, but not in the crowding-out capital expenditure regressions in Tables 4 and 5.

Senate coding that the T-statistics drop to below –1.2, in both the with-control and the without-control regressions, both clearly below conventional statistical significance levels.

B Coefficient Magnitudes

The economic meaning of coefficients that are about –0.01 is shockingly large. Roughly, the implied statement here is that Senate appointments reduced capital-expenditures from 7.5% of assets to 6.5% of assets.

In their Table 2, CCM report earmarks and capital expenditures by state. In their Table 3, CCM report coefficient estimates of seniority shocks on state-level earmarks of about 0.45. They report that "From table 1, 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)."

The coefficient magnitude of –0.01 is easiest to understand if we consider the U.S., both earmarks and capital expenditures, as a whole. This is to consider the ratio of earmarks to investment reductions. The numbers here are just hypothetical for the purpose of coefficient magnitude assessment, applicable to the ratio, not to be taken literally.

In general, earmarks are widely considered to have been small until the mid-1990s (Carroll (2010)). The "Citizen's against Government Waste" (CAGW) state-disaggregated earmark[7] data that is used in CCM starts in 1991. In the time-series, total U.S. earmarks rose from about $3 billion (nominal) in 1991 to $29 billion in 2008. CCM Table 2 shows that states received on average about $7 billion[8] per year in earmarks from 1991 to 2008. Thus, if one were to have applied Senate shocks to all U.S. states, total aggregate earmarks would have increased by an extra $0.45 \times \$7 \approx \$4$ billion per year.

Although this is not an economically insignificant amount, it pales in comparison with corporate capital expenditures and assets. The U.S. firms in CCM Table 2 had about $527 billion (2008-$) per year in capital expenditures. Not in their sample but easily obtainable from Compustat, the sample firms had about $8 trillion in assets in the early 1990s, rising

[7] CCM rely on the same CAGW data to establish that appointments increase earmarks. CAGW defines pork as spending requested by only one chamber of Congress; not specifically authorized; not competitively awarded; not requested by the President; greatly exceeding the President's budget request or the previous year's funding; not the subject of congressional hearings; or serving only a local or special interest.

[8] Following CCM, we quote all relevant dollar figures in this section adjusted to 2008 dollars.

to about $30 trillion in 2006 (both in 2008-$). The estimated CCM coefficient is on a Senatorial appointment dummy, explaining a dependent variable that is quoted in per-asset units.

We can use these facts to judge the coefficient magnitude in approximate terms. A regression coefficient of –0.01 in the CCM regressions implies a reduction in capital expenditures of about $0.01 \times \$20$ trillion $= \$200$ billion. (Again, for intuition, the coefficient states a reduction in the investment-asset ratio from 7.5% to 6.5%.) Thus, the model coefficient suggests an *aggregate-investment-decrease-to-earmark-increase* effectiveness of $200-to-$4.

The average of ratios is not the average ratio. We can use firms' own reported assets to improve this estimate. Given model (2) in Table 3, the model fits capital expenditures on a per-firm-year year to sum on average up to $610 billion (in 2008-$) from 1991-2008, obtained by multiplying each firm-year fitted capital-expenditure ratio *given an appointment dummy of 0* by actual own lagged firm assets for each firm. Given a dummy of 1, the model fits capital expenditures to be on average $396 billion per firm-year (in 2008-$). The difference between $610 and $396 billion, i.e., $215 billion, is the reduction attributed by the model to the Senate appointment dummy. Similar calculations yield total expected investment reductions due to a hypothetical Senate appointment for all firms in a given year:

Model-Implied Aggregate Investment Reductions, in Billion Dollars

	1970	1975	1980	1985	1990	1995	2000	2005	$\overline{1991-2008}$
Nominal	$7	$14	$24	$37	$62	$90	$151	$325	$181
Real (2008-$)	$39	$57	$62	$74	$103	$127	$188	$353	$215

The $215 billion per year investment reduction is conservative. It assumes zero assets on all other firms and thus no capital-expenditure reductions.[9] With this more detailed data, the model suggests an *aggregate-investment-decrease-to-earmark-increase* of $215-to-$4 from 1991 to 2008.

[9] Asker, Farre-Mensa and Ljunqvist (2013) suggest that the publicly-traded and non-publicly-traded corporate sectors account for about equal amounts in terms of aggregate non-residential fixed investment in 2010. If the CCM conjecture is correct, and if privately-traded firms reacted like publicly-traded firms, then the loss in investment could have been twice this amount! Asker et al do not examine whether the spending reductions are concentrated primarily in larger and smaller firms. Their regressions are equal-weighted, not value-weighted. If the CCM results came primarily from small firms, the coefficient could possibly become more plausible, but the conclusion that government spending crowds out *aggregate* investment would then seem incorrect.

Crowding-out of more than one-to-one does not strike us as plausible. Government earmarks were not associated with higher state taxes (the usual crowding-out channel); and, a priori, earmarks could have been complements rather than substitutes for in-state corporate expenditures. There is no known mechanism that can predict this large a crowding-out coefficient. Anything more than two-to-one strikes us as highly implausible. Even a five-to-one ratio would have struck us as absurd. The order of magnitude of the coefficients in CCM is simply wrong. The ratio is too large to be attributable to a crowding-out effect caused by earmarks.

These calculations further suggest that one could not have expected both reasonable economic and statistical significance in a sample of less than a few centuries. The crowding-out effect would have had to be considerably more than one-to-one to allow a researcher to expect statistical significance. After all, even at the $200-to-$4 crowding-out ratio, the observed coefficients are just hovering around conventional statistical significance levels, depending on the standard error clustering method. If the coefficient estimates had been, say, only one-to-one—which would still have been a remarkably efficient crowding-out "accomplishment" by a government—the smaller coefficient could then not have met conventional statistical significance levels. In sum, to detect statistical significance requires either implausible large economic magnitudes (for earmarks and mechanism) or a much longer data set to overcome the noise.

This reasoning will be more convincing if there is an available alternative explanation for the coefficients. If it was not earmarks, then what else could possibly have been responsible for the CCM results?

C Specific U.S. States

[Insert Table 4 here: Explaining Capital Expenditures By U.S. State of Chair]

In Table 4, we break the regressions into eight individual-state panel regressions, one for each state that had a Senate Finance Committee chairman. Each regressions compares one such state to the other 42 states which never had a chair (in the sample period). That is, in each of these regressions, we exclude all firms from the seven other states that ever had a Senate Finance chairman, either before or after. The regressions use the historical coding, although the (unreported) results with CCM coding are similar.

Five out of eight states have positive coefficients, with Louisiana having a significantly positive coefficient estimate and Kansas having a marginally significant positive coefficient. The biggest coefficient in absolute magnitude in all eight regressions was positive and occurred in Louisiana.

Three states had the negative "crowding-out" coefficients. Montana had only 14 firm-year observations. Thus, Montana is not a key driver of the CCM inference. Oregon had not only a smaller coefficient estimate than Texas, it also had only 1/20th of the observations of Texas. Again, Oregon is not a key driver. It is Texas that is worth investigating further.

[Insert Table 5 here: Explaining Capital Expenditures with and without Texas]

We first confirm that Texas was largely responsible for the negative CCM coefficient. In Table 5, we repeat the regression in Table 3 without Texas. The coefficient on having the Senate Chair drops from –0.0094 with Texas to –0.0003 without, using CCM coding. With our own historical U.S. senate coding, the coefficient even switches sign.[10]

D Texas Earmarks

As noted earlier, the "Citizen's against Government Waste" (CAGW) state-disaggregated earmark data that is used in CCM starts only in 1991. This means that we cannot look at the specific earmarks to Texas from 1987 to 1990. We do not know whether the Bentsen years were associated with an increase in earmarks because the data is not available before 1991, but it is unlikely that they were larger before 1991 than after. To give its report readers a background about the kinds of projects that were classified as earmarks, the 1991 CAGW report highlights mentioned that Texas received under $100 million, with $92.6 million earmarked for extending the Red River waterway to Shreveport (attributed in the report to be pork on behalf of the other Texas Senator, Bennett Johnston) and a $75,000 expense for Plant Stress Research. Attributing all multi-state projects that included Texas solely to Texas and summing over all projects yields earmarks of

	1991	1992	1993	1994	1995	1996	1997
TX	$82	$122	$331	$112	$132	$75	$179
US	$3,110	$2,664	$6,621	$7,776	$10,778	$12,508	$14,514

[10] If we do not use the CCM controls but we continue to use CCM coding, the coefficient drops only from –0.0122 to –0.0022, still statistically significant with state-level clustering (but not state-year clustering). The table shows the more relevant result with CCM controls.

in nominal million dollars. In 1991, Exxon-Mobil alone had (nominal) assets of $87 billion and capital expenditures of $7.3 billion. A coefficient of –0.01 implies a decrease in $73 million in investment. The model thus implies that Exxon-Mobil alone would have reduced its capital expenditures by more than two times the (45% × $82 ≈) $37 million increase in more earmarks to Texas attributable to Bentsen.

In 1991, the 447 Texas firms with Compustat data together had capital expenditures of $40.8 billion ($64.6 billion in 2008-$) on assets of $525 billion ($830 billion in 2008-$). At a coefficient of –0.0094, repeating the calculations from Page 9, the model suggests that predicted capital expenditures in Texas should have declined from $62 billion to $52 billion, i.e., by an aggregate $10 billion (all in 2008-$). This is more three times the total earmarks to all U.S. states combined, not just to Texas; and not just the Senator-implied earmark change, but in total. The coefficient estimate of –2% for Texas in Table 4 more than doubles this model-implied capital expenditure reduction to over $20 billion.

[Insert Figure 1 here: Federal Per-Capita Outlays]

Total Federal Outlays: However, we can ask whether these earmarks were only the tip of a bigger iceberg, proxying for Federal flows more generally. If Bentsen's chairmanship crowded out private capital expenditures with other Federal fund flows instead of earmarks, then it might be possible to detect an increase in Federal outlays to Texas during the Bentsen years. Figure 1 plots the federal outlays of Texas relative to the 42 non-Chair states. (The data are from Elis, Malhotra and Meredith (2009).) The lower panel shows that the Federal outlays to Texas remained fairly similar to outlays to other states from about 1980 to 2000.

The figure does show that the Bentsen years did not associate with an unusual increase in *total* Federal outlays to Texas. This lack of an unusual change is probably because a large part of Federal outlays are mandatory and not discretionary expenses. This is also consistent with research by Levitt and Poterba (1999) that finds that chairmanships do not have a strong influence on Federal flows, although having senior senators is correlated positively (not negatively) with economic growth.

It is unlikely that a smaller Federal account transfer flow to Texas could be identified that could reasonably explain a corporate capital expenditure reduction of $10 to $20 billion. Even all Federal outlays together do not seem large enough. In 1990, Texas had a population of about 17 million. Figure 1 shows that Federal outlays increased by about $1,000 per capita from 1987 to 1992 (just as they did in other states). Thus, even if the entire change in Federal outlays to Texas had been due to the Bentsen appointment—and the evidence from other states suggests that this was not the case—the CCM model would still suggest that corporate capital expenditures would have declined by about one-to-one for each additional dollar transferred to a Texan. There is no known mechanism that can explain such a large crowding-out coefficient without the taxation channel.

E Alternative Explanations For Texas

The key question remaining now is *if not earmarks, then what could have driven the behavior of Texas firms during the years before, during, and after Bentsen's Chairmanship?*

[Insert Figure 2 here: Macroeconomic Conditions: Oil Price and Bank Failures]

The obvious first line of inquiry where Texas is concerned relates to the oil and gas sector. Figure 2 plots the crude oil price, PPI-adjusted. This plot shows that the price of oil rose from \$14 in 1977 to \$39.50 by 1980, remained above \$25 until 1986, and then fell back to \$15. It is a plausible hypothesis that the low oil price during the Bentsen years, following years of higher oil prices, could have been responsible for reducing corporate investment expenses.[11]

The lower figure plots the number of bank failures per million inhabitants. The Bentsen years coincided with the Savings&Loan (S&L) Crisis, which hit Texas especially hard in the wake of the reduction in oil prices and real-estate values in Texas. Lack of bank capital could itself also have contributed to lower corporate capital expenditures.[12]

[11] A part of firms' investments, especially in real-estate, is thought not only to be unmovable and irreversible, but to have to occur in large indivisible pieces.

[12] We do not use bank failures as a control, because this would require an additional caveat: Being roughly contemporaneous, we then could not exclude the hypothesis that Bentsen's ascension to the Senate Chair increased the number of bank failures in Texas, and that Bentsen's negative effect on corporate capital expenditures more generally worked through this banking channel. By controlling for bank failures, we would not attribute this channel to the Chairmanship. (There is annual variation in bank failures that means that the two variables are not collinear.) Again, we consider it no more plausible that the decline in real estate and mortgage values associated with the decline in oil prices induced bank failures than did increased government earmark spending. This is further suggested by the timing of events: Texas banks had already started to fail before Bentsen's ascension and stopped to fail towards the middle of his Chairmanship. Unreported, controlling for bank failures would indeed increase the Texas coefficient further.

[Insert Table 6 here: Oil and Petroleum Sector Classifications]

To explore the behavior of firms in the oil and gas sector, and especially those in Texas, we first need to classify Compustat firms into oil and non-oil firms. Panel A of Table 6 lists the criteria that we applied. A firm was considered to be in the oil sector if its Compustat SIC code was 1300-1399 (Oil and Gas Extraction), 2911 (Petroleum Refining), 3533 (Oil and Gas Field Machinery), 4613 (Refined Petroleum Pipelines), 4925 (Mixed, Manufactured or Liquified Petroleum Gas Production and Distribution), and 5171-2 (Petroleum and Petroleum Products) in the year before we measured its capital expenditure. Panel B lists the states with the highest fraction of oil firms in 1970. There were eight states in which the oil sector was about 25% on average or more in 1970: North Dakota, Oklahoma, Texas, Louisiana, Wyoming, Colorado, Alabama, and Mississippi. Over the entire sample, the first six states had more than 15% of their publicly-traded firms in the oil sector. In some specifications below, we use a dummy that measures whether a firm was headquartered in one of these eight oil states.[13] About two-thirds of the firm-year observations from these eight states were from Texas. The other seven states accounted for the remaining one-third. Their firms make it possible to distinguish between a Texas effect and an oil state effect.

[Insert Figure 3 here: Capital Expenditures of Texas Oil Firms vs. Oil Firms from 42 Other States]

In Figure 3, we plot the capital expenditures of Texas firms relative to those of firms in the 42 non-Chair states.[14] The upper capital-expenditure figure shows that Texas firms tended to be more capital-expenditure intensive than those in the other 42 states. The figure further shows that Texas firms' capital expenditures were especially high from 1976 to 1982 and then again from 2002 on, the years when oil prices were relatively high.[15]

More importantly, the plot allows visualizing the source of the negative coefficient estimate in CCM. The average distance between Texas firms and the other 42 states from 1987 to 1992 (Bentsen) was less than the average distance between them in other years.

[13] Our results reported below hold regardless of whether we use all eight states or just six states. We did not have many observations in 1967, but industry membership is a very stable variable. For our important investigation, 1970 is early enough, because the appointment of Bentsen occurred only in the late 1980s.

[14] The composition of firms in these graphs can change from year to year, as firms can enter and exit the Compustat data base. That is, the capital expenditures increase in years in which new firms enter and decrease in years when they exit. This is not the critical driver of the result. When lagged capital expenditures is an independent variable (as on Page 21), by necessity, the one-year-to-one-year composition cannot change.

[15] We have no explanation why Texas firms increased their capital expenditures in 1997 when oil price increased only modestly.

However, the visual pattern raises a concern. It suggests that the capital expenditure narrowing began already two to four years prior to the Bentsen appointment. (It also lasted another year or two beyond it.)

As noted, not all oil firms were headquartered in Texas. The middle and lower graphs separate out firms in the oil industry vs. those not in the oil industry. Again, the visual task is to judge the average distance between the capital expenditures of firms from Texas and firms from the 42 other states, during the 1987–1992 Bentsen years vs. other years:

- The middle graph shows that Texas oil firms invested relatively more than other U.S. states' oil firms from 1995 to 1998 (and less than those firms from other oil states after 2000). Texas oil firms did not seem to reduce capital expenditures in an unusual manner in the Bentsen years.[16]

- In the lower graph, we plot the capital expenditures of non-oil firms. Oil is a relatively capital-intensive industry, so the magnitudes of their capital expenditures are lower. Before 1983, their capital expenditures were smaller than those of the Texas oil firms, but Texas non-oil firms still partly mimicked Texas oil firms in their response to oil price changes. That is, Texas non-oil firms also expanded with their oil-firm neighbors when oil prices were high (i.e., until about 1982).

  After 1983, there is little difference between non-oil firms from Texas and non-oil firms from other states. If Bentsen's appointment had a capital-expenditure depressing effect on non-oil firms, it would have seemed to have begun around 1983 and would have lasted for decades after his departure.

The two lower graphs together suggest that the top graph contains a composition aspect: Texas had relatively many more oil firms. The figure thus suggests that a part of the reduction in capital expenditures in Texas during the Bentsen years was due to the fact that all oil firms scaled back their capital expenditures, but Texas simply had more of them than the average other state. Consequently, these graphs suggest that controlling for oil-industry-membership in different years will eliminate most of the Bentsen effect. Any remaining effect would likely be due to the behavior of non-oil industries prior to 1983.

[16]We do not know why Texas firms invested relatively more from 1996–1998 than their peers, but any assessment that Texas capital expenditures from 1987–1992 were low relative to those in other years would seem to have to rest on these years. From about 2000 on, Texas oil-firms spent even less than their peers. Before 1997, they spent almost exactly like their peers.

[Insert Figure 4 here: Texas vs. Other Oil Firms]

Unfortunately, we have no good empirical model for the investment dynamics of oil- and/or non-oil firms, for the path-dependence of investment (due to the sunk-cost aspect of non-movable capital investments), for the optimal response to declines in the crude oil price, and for changing oil-extraction technologies.[17] However, we have a next-best alternative: Texas was not the only state with a high concentration of firms in the oil sector. Table 6 documented that there were other states with unusually large oil sectors. In Figure 4, we repeat the graphs from Figure 3 that compare the behavior of firms from Texas to that of firms from other states. The top figure shows that Texas firms invested relatively less during the 1987–1992 Bentsen years. The middle figure shows that a similar pattern was also the case for firms from the other six oil states (with no Senate Chair ever), i.e. those from ND, OK, WY, CO, AL, and MS. Just like firms from Texas, their firms invested relatively less (than non-oil state firms) during the Bentsen years. The bottom figure shows that Texas firms were not at all unusual compared to firms from the other oil states. If anything, during the Bentsen years, the capital expenditures of Texas firms relative to the capital expenditures of firms from the five other oil states were relatively higher (less red) than they were in the rest of the sample.

F Parametric Estimation With Oil-Industry Controls

Together, Figure 3 and Figure 4 suggest additional controls: first, a control for whether a firm is in the oil industry (in a given year); second, a control for how oil-dependent a state is in terms of percent of its publicly-traded firms that were in the oil industry in the previous year; third, a control for whether a firm was from an oil-state, where oil states are

[17] These are also the principal reasons why we cannot replace the set of year dummies with the prevailing oil-price. That is, our new controls are for the capital-expenditure behavior of oil firms in different years, not for the capital-expenditure behavior of oil firms as a function of the oil price. Figure 3 shows that Texas firms invested more than their peers from 1996-8 despite a low oil-price. Replacing the year dummies with the oil price itself, thereby omitting other oil-industry-related supply and demand effects, is not enough to reverse the coefficient.

The U.S. oil&gas sector is large enough to cause a meaningful reduction in investment. A PWC report on behalf of the American Petroleum Institute from July 2013 claims that about 5.6% of the U.S. employment, 6.3% of labor income, and 8% of value added in 2011 was oil&gas, accounting for about $600 billion of GDP. Halving the price of oil (which is not as mean-reverting as a Senate Chairmanship), as occurred in the mid-1990s, could well have caused a reduction in affected oil&gas states in corporate capital expenditures as large as that what was observed.

classified by frequency of oil firms in 1970. Thus, we introduce the following new control variables in Table 7:

- A full set of 40 year-dummies,[18] each multiplied by a dummy that is 1 if the firm was in the oil sector and 0 otherwise. This is *not* the same as separate year and industry (or firm) dummies, but measures how oil firms invested differently in different years relative to non-oil firms.
- A full set of 40 year-dummies, each multiplied by whether the firm's headquarter was in one of the eight oil states: ND, OK, WY, LA, CO, AL, MS, and TX.
- A full set of 40 year-dummies, each multiplied for each firm by the fraction of oil firms in the state in which the firm is headquartered. Most but not all of the variation in these variables comes from the eight oil-states vs the other 42 states.

Not reported in the tables, F-tests suggest that whenever we include these independent variables, they are highly statistically significant. Obviously, the variables are also correlated with the Senatorial appointment. (Otherwise, they could not influence its coefficient.) They are not correlated to the point where multicollinearity makes it impossible for the regression to disentangle the effects. The fact that there were non-Texas oil firm-years and other oil states allows the regression to allocate power to the Chairmanship vs the oil-sector aspects.[19]

[Insert Table 7 here: With Oil-Year Dummies and Per-Capital Bank Failures, Texas vs Others]

In Table 7, we add these three new variables. The left two regressions again compare Texas firms to those in the other 42 states which never had a chair, as we did in Table 4. As before, in models (1) and (2), the Bentsen coefficient is again highly statistically significant. Relative to other firms, Texas firms invested less during the Bentsen years. The strong negative coefficient reflects our focus on the single state with the strongest effect.

However, model (3) shows that including the first set of new controls—the year-dummies that also control for oil-industry membership—reduce the estimated coefficient on the

[18] See also footnote 17.

[19] Moreover, even if there had been perfect multicollinearity, one could argue that extraordinary claims—that small Federal government earmarks managed to reduce much larger corporate capital expenditures—require extraordinary proof. If alternative events occurred exactly at the same time and with perfect or near-perfect overlap, it would then be impossible to interpret the observed behavior as conclusive evidence of crowding out. (Instead, they would merely be one among other possible explanations.)

Bentsen's years from –0.0185 to –0.0075. The standard error also declines, leaving the coefficient estimate smaller but statistically significant. Model (4) shows that the coefficient estimate practically descends to zero when we offer being headquartered in an oil-state as a control. Texas firms did not invest any less from 1987 to 1992 when compared to firms from other oil states. Finally, model (5) shows that the coefficient estimate is very small and statistically insignificant if we use the fraction of oil-firms in the same state one year before the capital expenditure measure, instead of simply a dummy whether the state was one of our seven oil firms.[20]

[Insert Table 8 here: With Oil-Year Dummies and Per-Capital Bank Failures, Entire Sample]

In Table 8, we no longer compare Texas against 42 other states, but we return to the original CCM specification with fifty U.S. states and all eight treated Senate-Chair states. In models (1) and (2), the Senate Chair coefficient is again highly statistically significant: relative to other firms, treated firms invested less during the year of their home Senator's appointment. In model (3), we again add the yearly oil-sector dummy for each firm. This is sufficient to reduce the coefficient estimate to about one-third of its original magnitude (from –0.0087 to –0.0026) and eliminate all statistical significance. Model (4) shows that the coefficient estimate switches sign when we offer being headquartered in an oil-state as a control. Firms with the Senate Chairmanship did not invest any less when we control whether a firm was in one of the eight oil states (1970). Finally, model (5) shows that the coefficient estimate is also positive if we use the fraction of oil-firms in the same state one year before the capital expenditure measure, instead of simply a dummy whether the state was one of our eight oil firms.

We do not view the positive coefficient estimates to be indicative of a positive influence. They are statistically insignificant. Based on the magnitude of the earmarks vs. the magnitude of corporate capital expenditures, our priors and posteriors were that Chairmanships should not have played a major role (at least not through the earmark channel).

[20] We had to remove LA in this regression to avoid benchmarking Texas to another Senate-chaired state. This would bias the regression against finding Texas being important.

G Stability of Asset-Normalized Capital Expenditures

Asset-normalized capital expenditures are a relatively stable variable. Firms that have high capital expenditures in one year tend to have high capital expenditures in the following year.

G.1 Pre-Event Placebo Years

It is good practice to use a quick diagnostic test for whether an exogenous-shock identification worked well: If a shock presents sharp identification, it should not show a response in placebo years.

Years after the shock are not clean placebos. This is because it may take a while for government earmarks to crowd out capital expenditures. A delayed or extended-duration response can be reasonable under the alternative hypothesis.

Years before the shock are (relatively) clean placebos.[21] Any non-zero effect would suggest either anticipation of the shock or misspecification. In our application, it is simply not plausible that the actual Senatorial appointment would induce firms to reduce their capital expenditures years before in anticipation of a Senatorial appointment years later. Consequently, a negative coefficient estimate explaining corporate capital expenditures with Senate appointments years later would be indicative of incorrect identification. For a quasi-randomized experimental shock, a pre-response should just not be the case. This is not only the case if the associated impact would seem to be stronger in prior years than in the actual year, but also if there seems to be *any* effect in earlier years. In our case, such a finding would mean that it would become more difficult to associate and therefore attribute a decline in private capital expenditures to government intervention rather than to other (more gradual or earlier) contemporaneous happenings.

[Insert Figure 5 here: Regression coefficients with different leads and lags]

In Figure 5, we repeat two of the CCM regressions from Table 3, except that we lag and lead the chair appointment event by a number of years. (The results remain the same when we use other specifications from earlier tables.) On the left of year 0, the coefficient measures the capital expenditure response in anticipation of a future Senate chairmanship.

[21] Interestingly, in their Table 5, CCM do provide one a test for a quasi-placebo period before the appointment.

On the right of year 0, the coefficient measures the capital expenditure response with a lag of Senate chairmanship.[22]

The CCM coefficient is not only also negative at the appointment of Senate chairmanship but also negative four years prior to the appointment. (In fact, although it is not statistically significant, the placebo response is even *more negative* than the actually observed year-0 response.

G.2 Overlap on Placebo

Importantly, one can object that the placebo years show an effect because they contain overlap with the actual years. This is an appropriate objection, but it also suggests that the quality of the experimental identification is not as sharp as one may wish.

The first alternative is to assess whether coefficient estimates increase or decrease given year shifts. This means that we look whether the inference becomes stronger or weaker, as we showed that they did in Table 5. The other alternative is to specifically consider samples without overlap. We can compare the coefficient in the first two years of the appointment to the first two years *before* the appointment, only. Naturally, two-years coefficients do not capture the full effect. With controls and starring based on state clustering, for the overall sample and the three states with negative coefficients

	Full Tenure	First 2 True Years	Placebo 2 Prior Years
	1985–86,95	1985–1986	1983–1984
Oregon	–0.0098	+0.0043***	–0.0082**
	1987–92	1987–1988	1984–1986
Texas	–0.0185***	–0.0192***	–0.0223***
	2002,07–08	2002,07–08	2000, 05–06
Montana	–0.0458***	–0.0342***	+0.0243***
	Year 0–T	Year 0,+1	Year –2,–1
All	–0.0082	–0.0071	–0.0048

[22] In this table, we corrected three minor firm-year coding changes, which can explain why the coefficient at lag 0 is not –0.0122 as in Table 3, Model 1, but –0.0121.

Although there seems to have been a stronger effect in the first two years of the appointment, compared to the two placebo years, in the overall sample (principally Texas), the fact that there was already some coefficient in the direction of the effect preceding the appointment is an indication that the shock itself was not fully responsible in itself, i.e., sharp enough.

G.3 Auto-Coefficient

The dependent variable in CCM is the asset-normalized capital-expenditure level, not its year-to-year or multi-year change. There is no control for lagged capital expenditures in the regression, either. It is thus a good question, related to stability of the variable to be explained, whether controlling for lagged capital expenditures can be used to improve the identification—to better isolate the shock component.[23] For illustration, we use as specification the model that had the most negative coefficient estimate in year 0, the –1.22% from Table 3, Model (1). (The results are similar in the other specifications.)

	Placebo		Actual	Actual
Year-Lag	–2	–1	0	0
Senate Appointment Coefficient	–0.0089**	–0.0063*	–0.0056*	–0.0122***
Cap Exp Auto Coefficient	0.27	0.27	0.27	Forced 0

Controlling for lagged capital expenditures reduces the estimated coefficient to less than half of its previously-reported value. Again, the placebo year –2 shows a more negative response (–0.0089) than the actual Senate appointment year 0 (–0.0056). This suggests that the Senate appointment was not a shock to capital expenditures, even after we control for lagged capital expenditures in our attempt to obtain a better estimate of "unusual changes" in capital expenditures.

[23] Including such a variable reduces some of the composition effect that firm entry can have on capital expenditures. The concern here is that a state's average capital expenditures can change when a firm in a very high-capital expenditure intensive industry comes on-line or goes off-line. Some composition effect is taken care of in CCM by the requirement of asset data availability in the year before the firm enters. However, this does not hold over longer time intervals.

H Conclusion on Capital Expenditure Response to Senate Appointments

In sum, the data suggests that the primary reason for the negative association between chairmanships and capital expenditures was that (Texas) oil firms scaled back their capital expenditures in the wake of the oil price collapse from 1982 to 1986, just as Texas Senator Lloyd Bentsen took over the Senate Finance Committee Chair. This incidental timing caused a spurious indication in CCM that such appointments crowded out corporate-capital expenditures. Secondary contributors were the lack of clustering standard errors by state and the lack of sharp shock time identification for a variable of interest that has both some persistence and non-linear dynamics.

III Other Variables

CCM investigate other corporate behavior patterns, too, specifically R&D expenses, employment, and sales growth. As with capital expenditures, they conclude that when their home state took over the Senate Finance Committee Chairmanship, publicly-traded firms reduced their R&D expenses, reduced their employment, suffered lower sales growth, and paid out more. In this section, we show that these results are not robust.

A Research and Development Expenditures

[Insert Table 9 here: Explaining Research and Development]

In Table 9, we replicate the CCM coefficient of –0.0045 (CCM, Table 6, Panel A, Model 1). The coefficients on the appointment variables are statistically significant with state-year level clustering, but not with state-level clustering. Unlike for capital expenditures, for R&D, it does not matter whether Texas firms are included or excluded, or whether oil-industry membership is controlled for.

However, it does matter how the Senate appointment years are coded. This coefficient estimate flips sign when we use the historical U.S. senate coding (it is not statistically significantly positive). In hindsight, the difference between capital expenditures and R&D can perhaps be attributed to the fact that oil firms have unusually large capital expenditures, but not unusually large R&D expenses. This had a large effect on Texas.

We do not report the detailed analysis, but the R&D results are driven by an outlier, too. The negative coefficient estimate in the original CCM regression is not driven by firms from Texas, but largely driven by firms from New York after 1993. Table 2 shows that even though Senator Moynihan's Senate Chairmanship ran only from 1993 to 1994, New York firms were coded as treated by CCM as late as 1998 (the early years of the Tech boom with larger R&D investments). The (NY-based) results depended critically on one year, 1996. This can also explain why the sign of the coefficient estimate depends on the Senate coding.

A placebo-year appointment experiment cannot explain (much of) the effect:

	Placebo		Actual
Year	–2	–1	0
Senate Appointment Coefficient	–0.0013	–0.0023	–0.0043*

On the other hand, –0.0043 was not a particularly strong coefficient estimate to begin with.

Conclusion: The R&D response is not robust to historical coding of Senate chairmanship.

B Payout

It is not clear whether corporate payouts to their investors in any given year are good or bad. CCM interpret this as remittance of funds that would otherwise have been used for investment.

[Insert Table 10 here: Explaining Total Payout]

In Table 10, we replicate the CCM coefficient of +0.0027 (CCM, Table 6, Panel B, Model 1). We can confirm that corporate payouts have a positive coefficient estimate, regardless of specification or Senate coding. (The historical method that omits Texas is not a fair test. It is a diagnostic.) The coefficient estimate is robust with respect to the standard error clustering method. It is also robust with respect to the Senate coding. It is also robust to whether the regressions are run with or without Texas (which is not a [fair] test, but a diagnostic).

The main problem in interpreting the Senate appointment to have caused an increase in payout is the persistence of payouts. When we use the placebo prior years, i.e., examining whether a future Senate appointment associates with an increase in payout, the coefficient estimate is still positive:

	Placebo		Actual
Year	–2	–1	0
Senate Appointment Coefficient	0.0024***	0.0025***	0.0024***

(Not reported, even five years prior, the coefficient remained at about 0.001.) Thus, the data suggest that it is incorrect to attribute the increase in corporate payouts to the Senate appointment.

> **Conclusion:** Although payouts do seem to be higher, the effect seems to be equally strong two years before the appointment.

C Employment

[Insert Table 11 here: Explaining Changes in Employment]

In Table 11, we replicate the CCM coefficient of –0.0090 (CCM, Table 6, Panel C, Model 1). Its statistical significance improves when we use state-level clustering. This negative coefficient estimate has also been a puzzle to Ramey (2011). She concludes that the literature that estimate the effect of state or region multipliers on employment and income effects has generally (though not always) found positive employment effects, except for CCM: "A notable exception is the Cohen, Coval and Malloy (2011) paper, which finds that an increase in earmarks (induced by shifts in political power) lead to a decline in corporate employment in the state."

Table 11 shows that Texas plays no unusual role for explaining employment changes. However, the employment change coefficient on the Senate appointment switches sign when we use the historical Senate coding. Thus, the CCM inference depends critically on their coding.

Moreover, the CCM-coded response coefficient is also negative in placebo years before the Senate appointment.

	Placebo		Actual
Year	–2	–1	0
Senate Appointment Coefficient	–0.0193*	–0.0199*	–0.0083

Thus, the data suggest that it is incorrect to attribute a decrease in corporate employment to the Senate appointment. This resolves the Ramey puzzle.

> **Conclusion:** The employment response is not robust to historical coding of Senate chairmanship.

D Sales Growth

[Insert Table 12 here: Explaining Sales Growth]

In Table 12, we replicate the CCM coefficient of –0.0149 (CCM, Table 6, Panel D, Model 1). The table shows that Texas plays no unusual role in explaining sales growth. However, the sales-growth coefficient on the Senate appointment switches sign when we use the historical U.S. senate coding.

Moreover, its statistical significance depends on the clustering method for the standard errors.

Finally, the CCM-coded response coefficient is also negative in placebo years before the Senate appointment.

	Placebo		Actual
Year	–2	–1	0
Senate Appointment Coefficient	–0.0335*	–0.0311*	–0.0136

Thus, the data suggest that it is incorrect to attribute the decrease in sales growth to the Senate appointment.

> **Conclusion:** The sales growth response is not robust to historical coding of Senate chairmanship.

E House Ways and Means Leadership Appointment

[Insert Table 13 here: House Ways and Means Chairmanship and Capital Expenditures]

In their Table 5, CCM show how seniority shocks in other Congressional appointments have similar though lesser effects. Thus, we first focus on the most prominent, the appointment to Chairmanship of the House Ways and Means Committee. Table 13 replicates the reported –0.004 coefficient in CCM.

An examination of the underlying data reveals that CCM chose a similarly unusual coding for House Chairmanship appointments as they did for Senate Chairmanship appointments. Thus, models (3) and (4) in Table 13 also consider a plain historical coding of House Chairmanship. Under this historical coding, the coefficient estimates reverse sign to become positive. Not reported, an analysis of the data can explain this: The CCM coding omitted the House Ways and Means Committee Chair William Reynolds Archer of Texas from 1995–2001, a period during which Figures 3 and 4 suggested that Texas and oil firms out-invested their peers.

Moreover, repeating their Table 5, the inclusion of oil-state year effects in the regression suggests that this is again a (Texas) oil-firm phenomenon. Doing so reduces the coefficients for their independent variables even using the CCM coding (not the historical coding, i.e., in which Archer is never considered a chair). Using the CCM variable names,

Independent Variable	CCM	w/ Control for Oil State×Year
Shock_Top1ChairOnly	–0.0082	–0.0005
Shock_Top3ChairOnly	–0.0063	–0.0006
Shock_Top3ChairPlusRank	–0.0057	–0.0015
Shock_Top5ChairOnly	–0.0045	–0.0004
Shock_Top10ChairOnly	–0.0025	0.0000

For more detailed description of these independent variables, the reader can refer to the original CCM article.

Conclusion: The investment response is not robust to historical coding of House chairmanship and/or control for oil state.

IV Conclusion

Cohen et al. (2011) seemed to show that extra government expenditures and earmarks, as proxied by Congressional Finance chairmanships, crowded out the capital expenditures of publicly-traded corporations. This evidence was all the more surprising, because we would not have expected the usual crowding-out channel (taxation) to function and we would not have expected the chairmanships of single Congressional committees to have such a strong impact on the capital expenditures of often geographically diffuse publicly-traded corporations. Many of these firms have operations in states beyond that of their headquarters (and the Senate Chair). Capital expenditures are also large and volatile, which means that any measurable effect would have had to have been huge to make a difference. A casual reading of the text of the earmarks suggests that additional funding, if any, often went to such causes as waterway expansions and smoking reductions—hardly the policies that would induce many private firms to cut back their investments. The order of magnitude of the reported coefficients in CCM was implausibly large: it suggested that every dollar of Federal earmarks translated into private investment reductions that would have had to be hundreds of times larger than the earmarks themselves.

Our paper shows that the CCM results were primarily due to the fact that existing Texas firms had high average capital expenditures around 1981, four to seven years before Lloyd Bentsen assumed the U.S. Senate Finance Committee chairmanship in 1988. The early 1980s were a time of high world oil prices. By the mid-1980s, in the wake of the global oil price decline, the average capital expenditure levels of Texas firms had already begun to return to ordinary levels, years before Bentsen took the chair. There is no remarkable change in the two years before, during, or after Bentsen's chairmanship. The evidence does not suggest that it was Bentsen-induced earmark-spending that induced Texas firms to scale back capital expenditures.

Our paper also points out that many other results in CCM hinge on their somewhat unusual non-historical chairmanship coding and choice of standard error clustering. Placebo regressions on the key findings years *before* the appointment often report spurious significance, too.

Primarily, our paper provides the correct answer to the important question posed both in the CCM title and our own title: *Did powerful politicians cause corporate downsizing?* The correct answer is that there neither was nor could there have been evidence that they did. The magnitudes of the earmark interventions were too small to have made a detectable impact on a variable as large and noisy as corporate investment. The evidence cannot tell us whether, on the tiniest margin, Congressional-appointment-induced government earmarks crowded out a little private investment (or not). And it surely cannot answer the broader question whether government intervention is socially harmful (or not). But the evidence can tell us that the earlier-reported affirmative answer was wrong.

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Table 1: Summary Statistics

Variable	Observations	Mean	Sdv	Min	Max
Capital Expenditures / Lagged Assets	168,975	0.0784	0.1006	0	1.0254
Research and Development / Lagged Assets	87,865	0.0777	0.1334	0	1.2750
w/ valid capex	86,870	0.0779	0.1336	0	1.2750
Total Payouts / Lagged Assets	156,724	0.0233	0.0440	0	0.4566
w/ valid capex	154,832	0.0234	0.0440	0	0.4566
Sales Growth	181,489	0.1773	0.4414	–0.7217	3.7279
w/ valid capex	167,000	0.1804	0.4511	–0.7217	3.7279
CCM Finance Chair	217,610	0.0256	0.1579	0	1
w/ valid capex	168,975	0.0253	0.1570	0	1
Real Finance Chair	217,610	0.0310	0.1734	0	1
w/ valid capex	168,975	0.0301	0.1710	0	1
Lagged Q	157,276	1.8223	1.8369	0.3872	30.3963
w/ valid capex	153,348	1.8224	1.8256	0.3872	30.3962
Cash Flow / Lagged Assets	153,626	0.0366	0.2418	–2.4891	0.5739
w/ valid capex	151,482	0.0363	0.2417	–2.4891	0.5739
Lagged Leverage	174,209	0.4461	0.2784	0.0044	0.9865
w/ valid capex	159,833	0.4158	0.2610	0.0044	0.9865

Explanations: The variables were originally from Compustat and generously provided by Cohen, Coval, and Malloy (CCM). Because our data is the same, more statistics can be found in the original paper, Cohen et al. (2011). Firm-level variables start in 1967, state-level variables in 1991. Both data sets end in 2008. We also report statistics for firm-years without valid capital expenditures, because the regressions in our Tables 9–12 follow CCM and do not impose the capital expenditure availability constraint.

Table 2: Coding of Chairmanship of the U.S. Senate Finance Committee

Congress	Year	Hist	CCM Coding
90th	1967	LA	None
90th	1968	LA	None
91th	1969	LA	None
91th	1970	LA	None
92th	1971	LA	LA(100%)
92th	1972	LA	None
93th	1973	LA	None
93th	1974	LA	None
94th	1975	LA	None
94th	1976	LA	None
95th	1977	LA	None
95th	1978	LA	None
96th	1979	LA	None
96th	1980	LA	None
97th	1981	KS	None
97th	1982	KS	None
98th	1983	KS	None
98th	1984	KS	None
99th	1985	OR	OR(100%)
99th	1986	OR	OR(79%/97%)
100th	1987	TX	OR(72%/77%), TX(100%)
100th	1988	TX	OR(66%/69%), TX(91%/100%)

Congress	Year	Hist	CCM Coding
101th	1989	TX	OR(62%/64%), TX(83%/90%)
101th	1990	TX	OR(52%/59%), TX(76%/83%)
102nd	1991	TX	TX(71%/77%)
102nd	1992	TX	TX(65%/71%)
103rd	1993	NY	NY(100%)
103rd	1994	NY	NY(89%/100%)
104th	1995	OR	NY(81%/90%)
104th	1996	DE	NY(74%/81%), DE(100%)
105th	1997	DE	NY(69%/72%), DE(100%)
105th	1998	DE	NY(65%/67%), DE(99%/100%)
106th	1999	DE	DE(94%/90%)
106th	2000	DE	DE(90%/88%)
107th	2001	IA	DE(84%/88%), MT(100%)
107th	2002	MT	MT(100%)
108th	2003	IA	MT(100%)
108th	2004	IA	MT(83%/100%)
109th	2005	IA	MT(83%/83%)
109th	2006	IA	MT(80%/80%)
110th	2007	MT	None
110th	2008	MT	None

Explanations: The CCM state coding (dummies) are backed out from the CCM panel data. When present, the percentage figures behind the state name are the number of firms from this state in this year that have the Senate Chair dummy coded as 1. The first number is without the filter that capital expenditures are available, the second number imposes the filter. No percentage behind a state name means 100%, i.e., all firms from this state are coded as 1 in this year. The historical U.S. Senate data is from the *History of the Committee on Finance, United States Senate, May 21, 1981, with updates,* http://www.finance.senate.gov/about/history/. Note that we also correct the coding of new firms when we use "historical Coding" in subsequent tables.

Interpretation: The state-year level correlation (with 2,086 observations) between the CCM coding and the U.S. Senate history coding is 47%. Our paper uses both the CCM and the historical coding.

Table 3: The Impact of Senate Chairmanship and Corporate Capital Expenditures by Firms in the Senator's Homestate, 1968–2008

Dependent Variable: $CapExp_{i,t}/Assets_{i,t-1}$				
	CCM Senate Coding		Hist. Senate Coding	
	(1)	(2)	(3)	(4)
Senate Finance Committee Chair	–0.0122†	–0.0094†	–0.0087	–0.0082
Standard Errors, Clustered by				
State-Year level (as in CCM)	(0.0035†)***	(0.0030†)***	(0.0042)**	(0.0034)**
State level	(0.0069)*	~~(0.0062)~~	~~(0.0073)~~	~~(0.0063)~~
CCM Controls	No	Yes	No	Yes
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	44.0%†	50.1%†	44.0%	50.1%
Observations	168,975†	139,564†	168,975†	139,564†
Senate-Year Coding	CCM	CCM	Hist	Hist

† indicates values that are identical to those reported in CCM (Table 4, Models (1) and (2)).
*, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: The corporate data is from CCM and originally from Compustat. The dependent variable are firm-year capital expenditures normalized by lagged firm assets. The "CCM controls" are lagged Q, contemporaneous asset-adjusted cash flow, and lagged leverage. The left two regressions replicate the CCM regressions, where the Senate Finance Committee Chair is coded as it was original, explained in Table 2. Because we are using the original CCM data, the coefficients and state-year level standard errors are not just similar but identical. The right two regressions instead use the historical U.S. Senate coding method, again as explained in Table 2.

Interpretation: With state-level clustered standard errors, the CCM coefficient of –0.0094 in the "with-controls" specification becomes insignificant. Both coefficients are smaller when we use the historical method instead of the CCM method for Senate coding (see Table 2). Both become insignificant when standard errors are clustered at the state level.

Table 4: Explaining Capital Expenditures / Lagged Assets By U.S. State of Chair, 1968–2008

Dependent Variable: $\text{CapExp}_{i,t}/\text{Assets}_{i,t-1}$

Panel A: Without Controls

State:	1966 -1980 Louisiana	1981 -1984 Kansas	1985-86 1995 Oregon	1987 -1992 Texas	1993 -1994 New York	1996 -2000 Delaware	2001, 2003-06 Iowa	2002 2007-08 Montana	All
Senate Finance Committee Chair	+0.0505	+0.0185	–0.0060	–0.0210	+0.0016	+0.0073	+0.0043	–0.0408	–0.0087
Standard Errors, Clustered by									
State-Year level (as in CCM)	(0.0118)***	(0.0099)*	~~(0.0073)~~	(0.0058)***	~~(0.0025)~~	~~(0.0061)~~	~~(0.0042)~~	(0.0144)***	
State level	(0.0014)***	(0.0028)***	(0.0016)***	(0.0013)***	~~(0.0018)~~	(0.0010)***	(0.0013)***	(0.0013)***	
Adjusted R^2	42.9%	42.8%	42.7%	43.6%	42.9%	42.6%	42.6%	42.6%	
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Affected-State Firm-Years	285	168	150	3,097	1,536	167	152	14	
Observations	131,149	131,233	131,658	145,459	148,183	130,683	131,117	130,249	

Panel B: With Controls for lagged Q, contemporaneous asset-adjusted cash flow, and lagged leverage

State:	Louisiana	Kansas	Oregon	Texas	New York	Delaware	Iowa	Montana	All
Senate Finance Committee Chair	+0.0522	+0.0171	–0.0098	–0.0185	+0.0007	+0.0088	+0.0038	–0.0458	—0.0082
Standard Errors, Clustered by									
State-Year level (as in CCM)	(0.0122)***	(0.0109)*	(0.0049)**	(0.0044)***	~~(0.0030)~~	~~(0.0071)~~	~~(0.0039)~~	(0.0159)***	
State	(0.0018)***	(0.0017)***	(0.0015)***	(0.0010)***	~~(0.0013)~~	(0.0009)***	(0.0016)***	(0.0010)***	
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Adjusted R^2	48.8%	48.7%	48.6%	49.7%	48.8%	48.5%	48.6%	48.6%	
Affected-State Firm-Years	161	123	102	2,273	813	93	103	9	
Observations	108,324	108,437	108,809	120,428	122,226	107,992	108,321	107,632	

*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions break out the states that had a U.S. Senate Finance Committee Chairman using the historical U.S. Senate coding and includes as controls only the remaining 42 states that had no Chair in the sample. Otherwise, these regression imitate those in Table 3.

Interpretation: Only three out of eight states (OR, TX, MT) have the negative coefficient described in CCM. The CCM Controls do not change the conclusions.

Table 5: Explaining Capital Expenditures / Lagged Assets By State of Chair, 1968–2008, With and Without Texas

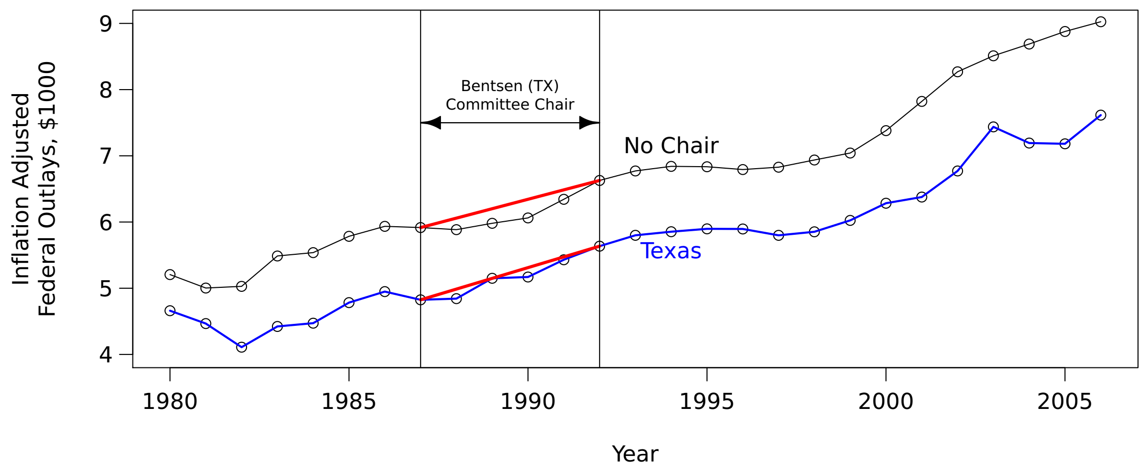
Dependent Variable: $\text{CapExp}_{i,t}/\text{Assets}_{i,t-1}$			
	All States Original	Without Texas	
Senate-Year Coding	CCM	CCM	Hist
Senate Finance Committee Chair	–0.0094†	–0.0003	+0.0048
Standard Errors, Clustered by			
State-Year level (as in CCM)	(0.0030†)***	~~(0.0021)~~	~~(0.0030)~~
State level	~~(0.0062)~~	~~(0.0008)~~	~~(0.0047)~~
CCM Controls	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Adjusted R^2	50.1%†	49.2%	49.2%
Observations	139,564†	126,651	126,651

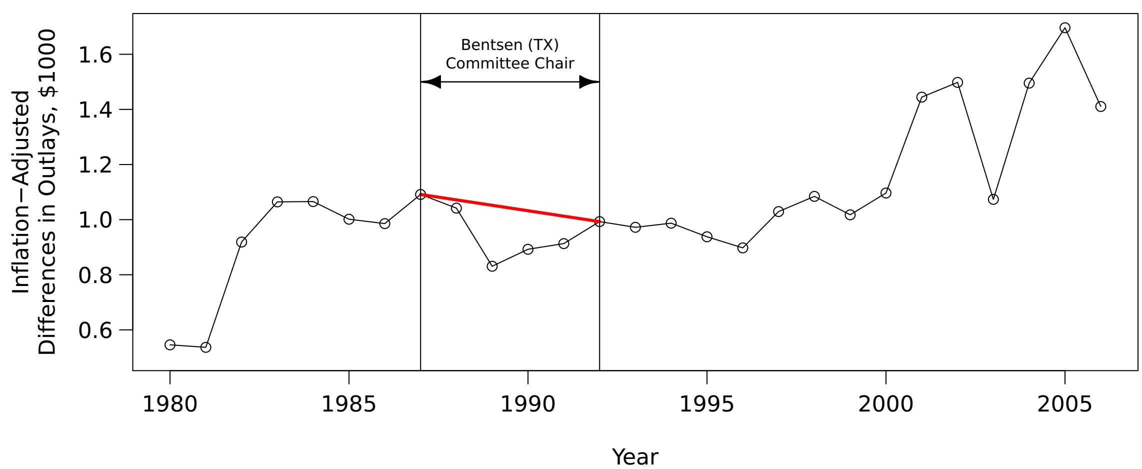
† indicates values that are identical to those reported in CCM (Table 4, Model (2)).
*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions are the equivalent of Model (2) in Table 3, except that the right two regressions omit the state of Texas.

Interpretation: Without the state of Texas, the coefficient estimate is no longer reliably negative.

Figure 1: Federal Per-Capita Outlays in Inflation-Adjusted Thousand Dollars, TX vs. 42 states that never had a Senate Finance Committee Chair



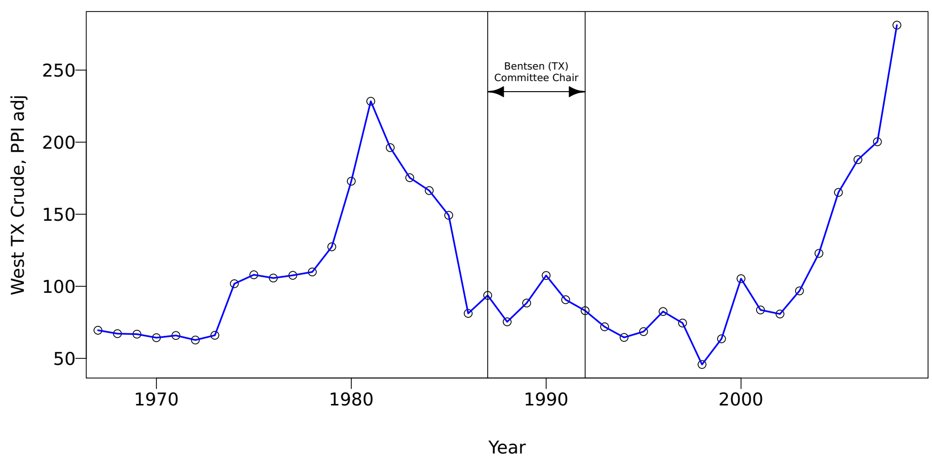


Explanations: The top graph plots the inflation-adjusted per-capita level of Federal Outlays for Texas and the 42 states that had no Chair in the sample, as in Elis et al. (2009). The bottom graph plots the per-capita difference between them.

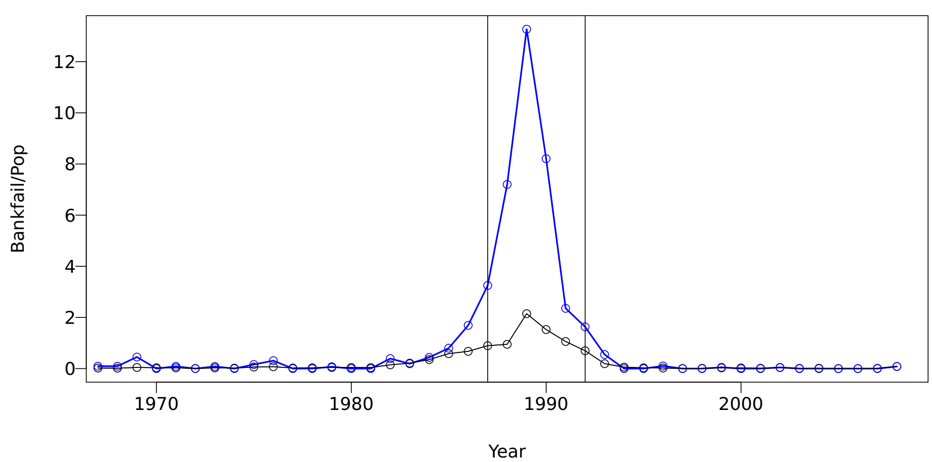
Interpretation: Although there was modest increase in federal outlays to Texas in 1989, over Bentsen's full 1987–1992 tenure, the was increase in per-capita outlays to Texas was not significant. Relative per-capita outlays remained essentially constant from about 1983 to 2000.

Figure 2: Macroeconomic Conditions: Oil Price and Bank Failures

Crude Oil Price



Bank Failures Per Million Inhabitants



Explanations: The top graph plots the inflation-adjusted oil price. Oil prices are from the Bureau of Labor Statistics (PPI-Adjusted Crude Petroleum), WPU0561 *NSA, Crude Domestic-Production Petroleum—part of PPI-Commodities*. The bottom graph plots the number of bank failures in the U.S. and in Texas(blue), per million inhabitants. Bank failure data is from the FDIC (`http://www.fdic.gov/bank/individual/failed/`), population estimates are from the Bureau of Economic Analysis.

Interpretation: The oil price was relatively low from 1987 to 1992, after a period of high oil-prices. It seems plausible that this could have induced oil firms to scale back capital expenditures. Bank failures were relatively high from 1987 to 1992, especially in Texas, but the failure rate had started to increase before Bentsen assumed the Chair, and had peaked before he left the Chair.

Table 6: Oil and Petroleum Classifications

Panel A: Oil Sector Classification

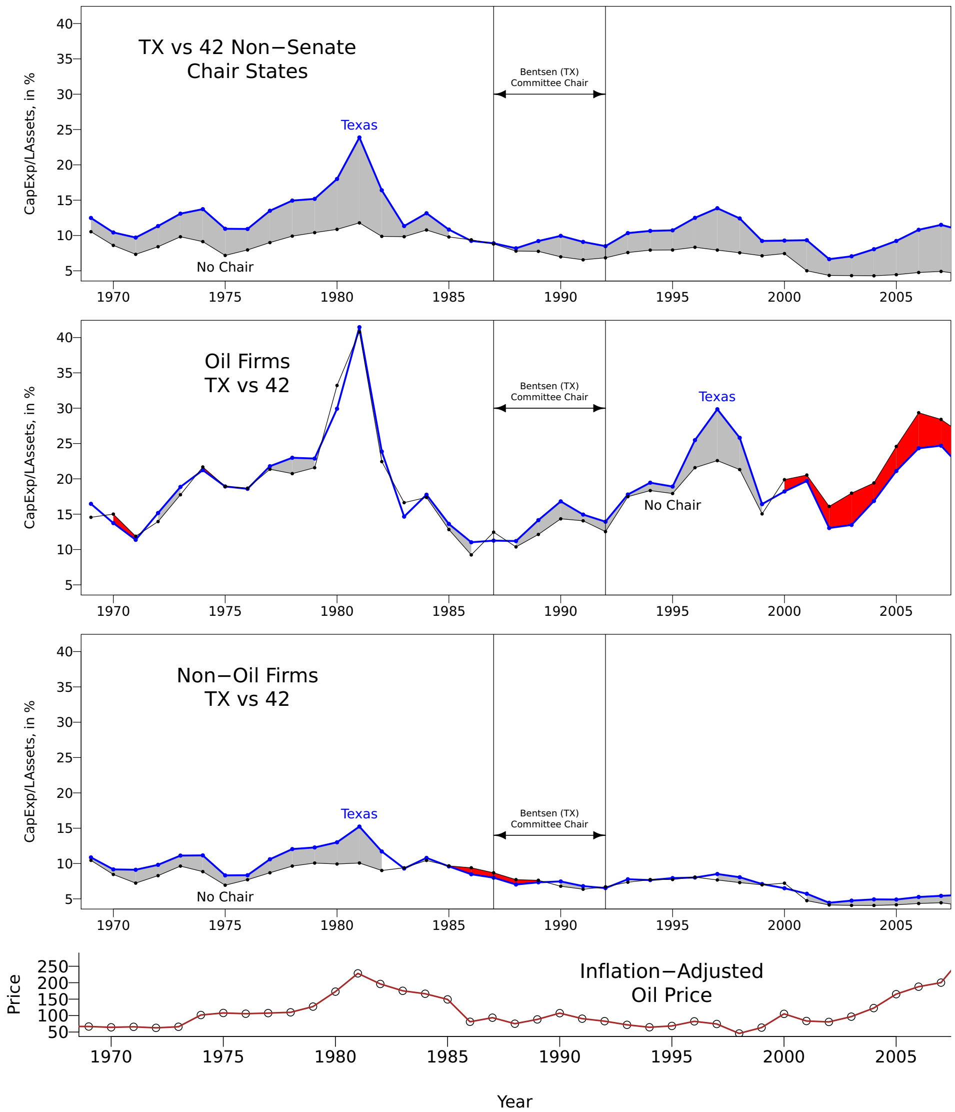
SIC	Industry	Texas Firm Years
1311	Crude Petro & Natural Gas	2,657
1381	Drilling Oil & Gas Wells	573
1382	Oil & Gas Field Exploration	226
1389	Oil & Gas Field Services	357
2911	Petroleum Refining	443
2990	Miscellaneous Products of Petroleum & Coal	2
3533	Oil & Gas Field Machinery & Equipment	354
4610	Pipelines (No Natural Gas)	20
4922	Natural Gas Transmission	394
4923	Natural Gas Transmission and Distribution	197
4924	Natural Gas Distribution	81
4932	Gas and Other Services Combined	–
5171	Wholesale Petroleum Bulk Stations & Terminals	53
5172	Wholesale Petroleum	118
5500	Retail Auto Dealers and Gasoline Stations	67
6792	Oil royalty traders	390

Panel B: Percent of Publicly-Traded Firms In U.S. States in the Oil&Gas Sector

State	In 1970		Over Entire Sample	
ND	100%	(1 in 1)	59%	(78 in 132)
OK	44%	(8 in 18)	47%	(884 in 1,869)
MS	33%	(1 in 3)	10%	(71 in 686)
TX	31%	(53 in 173)	32%	(5,932 in 18,374)
LA	29%	(4 in 14)	29%	(377 in 1,294)
CO	25%	(8 in 32)	24%	(1,316 in 5,517)
AL	25%	(3 in 12)	9%	(118 in 1,379)
WY	0%	(0 in 1)	29%	(45 in 157)
All Others	$\leq$ 15%		$\leq$ 20%	

Explanations: In Panel B, the sort order is by percent of publicly-traded firms of this state that are in the Oil Sector in 1970. About one-third of publicly-traded oil&gas firms are not from Texas.

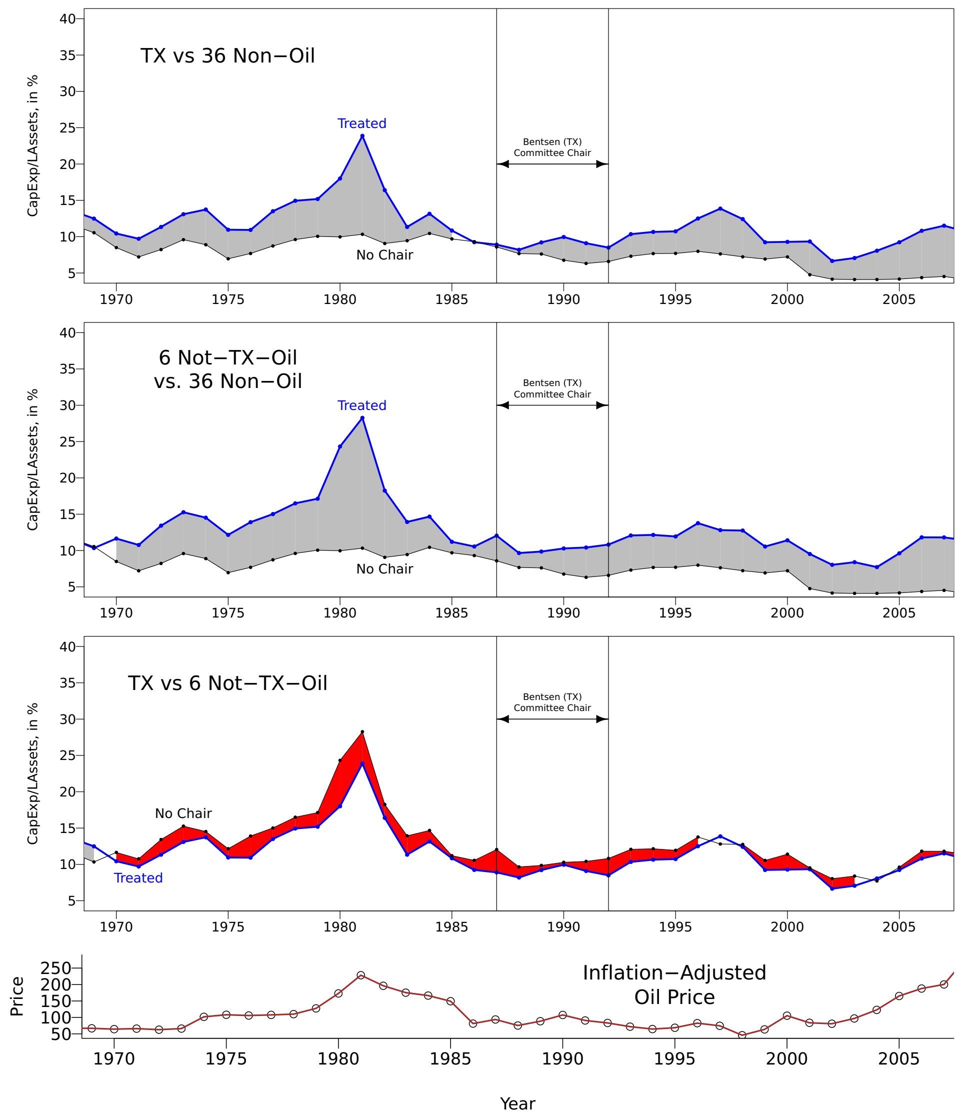
Figure 3: Asset-normalized capital expenditures of Texas firms compared to firms in 42 states that had no Senate Finance Committee Chair



Explanations: The top three graphs plot the time-series of the asset-normalized capital expenditures of Texas firms vs. those of firms from the 42 other states that had no Chair in the sample. The top graph includes all firms, the middle graph includes only firms in the oil sector (see Table 6), the bottom graph includes only firms not in the oil sector. Areas where TX firms out-invested non-TX firms are in gray; areas where non-TX firms out-invested are in red; areas where the two lines crossed mid-year are uncolored. The bottom graph plots the inflation-adjusted oil price.

Interpretation: Much of the smaller distances between Texas and the other 42 states during the Bentsen years is an oil-sector composition effect.

Figure 4: Asset-normalized capital expenditures of Texas firms and Other Oil States



Explanations: These graphs compare states within the set of 42 states that had no Chair in the sample. The top graph is the same as the top graph in Figure 3, i.e., the time-series of the asset-normalized capital expenditures of Texas firms vs. those of firms from the 36 other states that never had a Senate chair and were not oil states. The middle graph repeats this graph for firms from the six other oil states (ND, OK, WY, CO, AL, and MS) vs firms from the other 36 states. The bottom graph compares the capital expenditures of Texas firms to the capital expenditures of firms from the six other oil states.

Interpretation: Firms from other oil states had similar capital-expenditures patterns, before and after Bentsen's 1987–1992 chairmanship, as firms from Texas.

Table 7: Explaining Capital Expenditures / Lagged Assets By State of Chair, 1968–2008, Texas vs. 42 Non-Chair States

Dependent Variable: $\text{CapExp}_{i,t}/\text{Assets}_{i,t-1}$					
	(CCM)	Hist	Various Year-Oil Controls		
	(1)	(2)	(3)	(4)	(5)
Bentsen Senate Finance Chair	–0.0204	–0.0185	–0.0075	–0.0011	+0.0004
Standard Errors, Clustered by					
State-year level	(0.0046)***	(0.0044)***	(0.0026)***	~~(0.0048)~~	~~(0.0044)~~
State level	(0.0011)***	(0.0010)***	(0.0033)**	~~(0.0056)~~	~~(0.0080)~~
Year × in Oil-Industry	No	No	Yes	No	No
Year × in ND,OK,TX,WY,CO,AL,MS	No	No	No	Yes	No
Year × Own State's Oil Percentage	No	No	No	No	Yes
CCM Controls	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	49.7%	49.7%	50.6%	50.1%	50.1%
Observations	120,428	120,428	120,428	120,428	120,428
U.S. Senate Coding	CCM	Hist	Hist	Hist	Hist

*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions compare Texas firms' capital expenditures to those of firms in 42 states that had no Chair. The first two columns show that Texas firms reduced their capital expenditures when Bentsen was appointed. The last three columns show that controlling for the performance of oil states and firms in different years, the reduced capital expenditures were relatively smaller. The "Year × in Oil-Industry" consists of a full set of 40 year dummies, each multiplied by an indicator of whether the firm was in the oil-industry in year t. The "Year × in Oil-Industry" consists of a full set of 40 year dummies, each multiplied by a 1 if the firm was headquartered in ND,OK,TX,WY,CO,AL,MS in year t and 0 otherwise. (LA is dropped because it had a Senate Chair in an earlier year.) The "Year × Own State's Oil Percentage" consists of a full set of 40 year dummies, each multiplied by the fraction of firms in the oil-sector in the firm's home state in year t.

Interpretation: The negative Bentsen "effect" can instead be attributed to the year-dependent behavior of oil firms.

Table 8: Explaining Capital Expenditures / Lagged Assets By State of Chair, 1968–2008, All 50 States

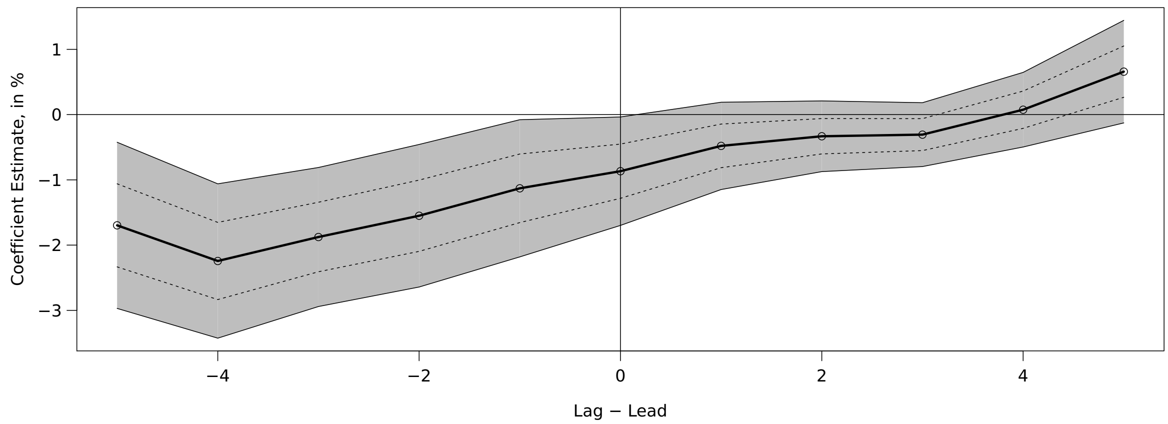
Dependent Variable: $\text{CapExp}_{i,t}/\text{Assets}_{i,t-1}$					
	(CCM)	Hist	Various Year-Oil Controls		
	(1)	(2)	(3)	(4)	(5)
Senate Finance Committee Chair	–0.0094[†]	–0.0082	–0.0026	+0.0016	+0.0019
Standard Errors, Clustered by					
State-year level	(0.0030[†])***	(0.0034)**	~~(0.0019)~~	~~(0.0022)~~	~~(0.0022)~~
State level	~~(0.0062)~~	~~(0.0063)~~	~~(0.0028)~~	~~(0.0023)~~	~~(0.0029)~~
Year × in Oil-Industry	No	No	Yes	No	No
Year × in ND,OK,TX,LA,CO,AL,MS,WY	No	No	No	Yes	No
Year × Own State's Oil Percentage	No	No	No	No	Yes
CCM Controls	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	50.1%[†]	50.1%	51.0%	50.5%	50.6%
Observations	139,564[†]	139,564	139,564	139,564	139,564
U.S. Senate Coding	CCM	Hist	Hist	Hist	Hist

[†] indicates values that are identical to those reported in CCM (Table 4, Model (2)).
*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions rerun the original regressions with all 50 U.S. states, but control for yearly oil-industry effects. Variables are defined in Table 7.

Interpretation: The negative coefficient is not robust when we control for annual effects and oil-sector composition.

Figure 5: Treatment Coefficient With Placebo Leads and Lags



Dependent Variable: $\text{CapExp}_{i,t+x}/\text{Assets}_{i,t+x-1}$									
	Before Chair Appointment					After Chair Appointment			
Lead-Lag	–4	–3	–2	–1	0	1	2	3	4
Table 3 (1): Coef, CCM	–0.0113	–0.0125	–0.0138	–0.0137	**–0.0121**	–0.0092	–0.0064	–0.0032	–0.0001
Table 3 (3): Coef, Hist	–0.0224	–0.0188	–0.0155	–0.0113	**–0.0087**	–0.0048	–0.0033	–0.0031	0.0008
State-Year Cluster SE	0.0059	0.0053	0.0055	0.0052	**0.0042**	0.0033	0.0027	0.0024	0.0029
State Cluster SE	0.0110	0.0104	0.0095	0.0088	**0.0073**	0.0058	0.0043	0.0034	0.0032

Explanations: The figure imitates the with-control coefficient in Table 3 (3), but for eleven regressions in which we either lead or lag the Senate chairmanship appointment timing. In the figure, the coefficient at zero is the original regression coefficient on the Senate Finance Committee Chair variable in Table 3 (1), explaining corporate capital expenditures from 1968–2008. The dashed line indicate the two standard deviation range with state clustering. To the left of zero are identical regression specifications, except that the chairmanship appointment is pretended to have been x years earlier. To its right are identical regressions, except that the chairmanship appointment is pretended to have been x years later. The table below gives equivalent information, but also for Table 3 model (3).

Interpretation: Coefficients in Placebo years, i.e., years before the chairmanship appointment, are not zero, but similarly negative. This is also broadly consistent with Figure 3, in which Texas firms had the highest capital expenditures four years *before* Bentsen's 1987–1992 appointment. Note that long-term tenure can induce this pattern, too, although this clarifies that the "shock" identification is weak.

Table 9: Explaining Research and Development / Lagged Assets By State of Chair, 1968–2008

Dependent Variable: $R\&D_{i,t}/Assets_{i,t-1}$				
	With Texas		Without Texas	
Senate-Year Coding	CCM	Hist	CCM	Hist
Senate Finance Committee Chair	–0.0045†	+0.0003	–0.0041	+0.0032
Standard Errors, Clustered by				
State-Year level (as in CCM)	(0.0017†)***	~~(0.0017)~~	(0.0024)*	~~(0.0030)~~
State level	(0.0023)*	~~(0.0029)~~	~~(0.0037)~~	~~(0.0053)~~
Controls	Yes†	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	78.2%†	78.2%	78.1%	78.1%
Observations	74,842†	74,842	70,017	70,017

† indicates values that are identical to those reported in CCM (Table 6, Panel A, Model (1)).
*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now research and development (as in CCM, Table 6, Panel A). Following CCM, these regressions use controls, even though the caption states that, like the regressions below, there are no control for R&D. This makes little difference.

Interpretation: The coding of Senate appointments is critical when explaining R&D. With the historical Senate coding, the coefficient is positive. Texas is unimportant.

Table 10: Explaining Total Payout / Lagged Assets By State of Chair, 1968–2008

Dependent Variable: $\text{Payout}_{i,t}/\text{Assets}_{i,t-1}$				
	With Texas		Without Texas	
Senate-Year Coding	CCM	Hist	CCM	Hist
Senate Finance Committee Chair	$+0.0027^{\dagger}$	$+0.0017$	$+0.0033$	$+0.0016$
Standard Errors, Clustered by				
State-Year level (as in CCM)	$(0.0007^{\dagger})^{***}$	$(0.0006)^{***}$	$(0.0009)^{***}$	~~(0.0010)~~
State level	$(0.0007)^{***}$	$(0.0007)^{**}$	$(0.0005)^{***}$	~~(0.0014)~~
Controls	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	$39.2\%^{\dagger}$	39.2%	39.0%	38.9%
Observations	$129{,}991^{\dagger}$	129,991	117,845	117,845

† indicates values that are identical to those reported in CCM (Table 6, Panel B, Model (1)).
*, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now total payout (as in CCM, Table 6, Panel B). Following CCM, these regressions use controls, even though the caption states that, like the regressions below, there are no control for R&D. This makes little difference.

Interpretation: Firms seem to pay out more during the chairmanship, although this is not significant if the historical Senate coding is used and Texas is excluded.

Table 11: Explaining Change in Number of Employees By State of Chair, 1968–2008

Dependent Variable: Δ Employees$_{i,t}$				
	With Texas		Without Texas	
Senate-Year Coding	CCM	Hist	CCM	Hist
Senate Finance Committee Chair	–0.0089†	+0.0105	–0.0102	+0.0164
Standard Errors, Clustered by				
State-Year level (as in CCM)	~~(0.0080†)~~	~~(0.0068)~~	~~(0.0118)~~	~~(0.0101)~~
State level	(0.0033)***	(0.0049)**	(0.0043)**	~~(0.0101)~~
Controls	No†	No	No	No
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	13.5%†	13.5%	14.1%	14.1%
Observations	168,267†	168,267	153,618	153,618

† indicates values that are identical to those reported in CCM (Table 6, Panel C, Model (1)).
*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now the percent change in employment (as in CCM, Table 6, Panel C). Following CCM, these regressions do *not* use controls, as stated in their table caption.

Interpretation: The coding of Senate appointments is critical when explaining changes in employment. With the historical Senate coding, the coefficient is positive. Texas is unimportant.

Table 12: Explaining Sales Growth By State of Chair, 1968–2008

Dependent Variable: $\%\Delta Sales_{i,t}$				
	With Texas		Without Texas	
Senate-Year Coding	CCM	Hist	CCM	Hist
Senate Finance Committee Chair	–0.0149†	+0.0172	–0.0134	+0.0365
Standard Errors, Clustered by				
State-Year level (as in CCM)	~~(0.0115†)~~	~~(0.0132)~~	~~(0.0121)~~	(0.0139)***
State level	(0.0041)***	(0.0094)*	~~(0.0084)~~	(0.0141)**
Controls	No	No	No	No
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	18.1%†	18.1%	18.8%	18.8%
Observations	181,489†	181,489	165,337	165,337

† indicates values that are identical to those reported in CCM (Table 6, Panel D, Model (1)).
*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: These regressions repeat those in Table 3, except that the dependent variable is now sales growth (as in CCM, Table 6, Panel D). Following CCM, these regressions do *not* use controls, as stated in their table caption.

Interpretation: The coding of Senate appointments is critical when explaining sales growth. With the historical Senate coding, the coefficient is positive. Texas is unimportant.

Table 13: The Impact of House Ways and Means Chairmanship and Corporate Capital Expenditures by Firms in the Congressman's Homestate, 1968–2008

Dependent Variable: $CapExp_{i,t}/Assets_{i,t-1}$				
	CCM House Coding		Hist. House Coding	
	(1)	(2)	(3)	(4)
House Ways&Means Committee Chair	–0.0060	–0.0039†	+0.0018	+0.0029
Standard Errors, Clustered by				
State-Year level (as in CCM)	(0.0024)**	(0.0018†)**	~~(0.0023)~~	~~(0.0019)~~
State level	(0.0017)***	(0.0015)**	~~(0.0032)~~	~~(0.0034)~~
CCM Controls	No	Yes	No	Yes
Year Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R^2	44.0%†	50.1%†	44.0%	50.1%
Observations	168,975†	139,564†	168,975†	139,564†
House Coding	CCM	CCM	Hist	Hist

† indicates values that are identical to those reported in CCM (Table 5, Model (1)).
*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels. Struck-out cells indicate when the estimated coefficient drops below significance at the 5% level given this s.e.

Explanations: This is similar to Table 3, except that the independent variable is not Senate Finance Committee Chairmanship, but House Ways and Means Committee Chairmanship.

Interpretation: The unusual coding in CCM induced negative coefficient estimates.