Do Powerful Politicians Really Cause Corporate Downsizing?

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Abstract

Cohen, Coval, and Malloy (2011) suggest that increased government spending crowded out private corporate investment by publicly-traded corporations, as identified by changes in Congressional chairmanships. Yet, the same inference would obtain in a placebo that begins years earlier, the capital-expenditure decline was primarily a 1987-92 Texas effect (following a 1980-86 oil price decline), and the standard errors were not clustered by state.

We thank Lauren Cohen, Josh Coval, and Chris Malloy for making their data available to interested researchers and for their feedback. Voluntary sharing of data is prima-facie evidence of academic integrity of the highest kind. Their data sharing made our critique possible in the first place.

Cohen, Coval, and Malloy (2011), henceforth CCM, introduced a novel empirical approach to identify shocks to congressional seniority and the effects of these shocks on corporate investment. If chair appointments to the U.S. Senate Finance Committee (or the House Ways and Means Committee) were serendipitous events, then changes in chairmanships could be good shocks (instruments) for measuring the effects of government intervention. CCM first showed that earmark spending to the home state increased when a new chair ascended, thus establishing one such channel—an increase in public government spending.

CCM then reported that new chairmanships caused a *decline* in the consolidated world-wide corporate investment for publicly-traded Compustat firms in the Senators' home states, as proxied primarily by capital expenditures (capex) and secondarily by R&D spending, corporate employment, payout, and sales growth. For perspective, their point estimate of 10% for the capex decline is of the same order of magnitude as the investment declines typically observed in recessions. Their findings have served as the basis of testimony before Congress and continue to circulate in various blogs and commentaries on the Internet. The peer-reviewed academic evidence in CCM remains relevant and influential.

Our paper takes a second look at the CCM data and finds no evidence that changes in congressional seniority influenced corporate investment. Our extended analysis is available online.

I Ascension Coding

The CCM variable, which our paper also used for replication purposes, is based on a novel Senate coding system that readers should be aware of: (1) Shocks were applied when a chairman ascended. (2) Shocks lasted for six years; (3) No second senatorial shock was applied to a state when one was already in place *for this state*. (4) Oregon 1995 was excluded because Senator Packwood faced Senate sanctions during his second chairmanship; (5) Louisiana was excluded because the original Compustat data began later; (6) When a state had a ranking member (KS in 1979 and IA in 2003) who then became chair, the ranking member was not upgraded, nor was the length of the shock extended. Table 1 highlights the differences between the resulting CCM coding and the actual historical Senate chairmanships.

[Insert Table 1 here: CCM-Historical Chairman Coding With Explanations]

CCM believe that their coding system is superior, because Senators could have been more likely to be deposed *when/because* in-state conditions were good. Table 1 suggests that the only plausible election-induced removal from a Senate chair occurred in 2000 (and only would have reduced their specific Senate dummy coding by one year). More commonly, chairmanships changed for reasons beyond conditions in the Senator's state—usually, through Senate majority changes in other election cycles and not through in-state related events.[1,2]

II Placebos, Texas, and Clustering Issues

Most of our data was obtained from and is thus identical to that used in CCM 2011. The reader can consult their descriptive statistical tables (Table 1-3).

[Insert Table 2 here: Treatment Coefficient In Placebo Years Prior to Ascenscion]

In the middle column of our Table 2, we replicate the key CCM regressions. They explain the corporate variables in the left columns with controls and the variable of interest: a dummy that is related to the Senate chairmanship appointment, as coded by CCM. With the original CCM data, our regressions can report identical coefficients and standard errors.

If an effect is attributed to a Senate ascension and firms did not anticipate it, advancing the ascension coding should diminish the measured effect. Thus, the columns to the left of the original CCM coefficients recode the treatment dummy by starting them 4 years or 1 year earlier, respectively. In effect, this allows testing the absurd hypothesis that firms began throttling their investment in anticipation of future CCM Senate changes. Yet, the

[1] Even if endogenous election outcomes had been pervasive, we could not understand the *specific* CCM rules. In Monte-Carlo experiments on some possible underlying models, the bias worsened when we applied the CCM rules (1), (2), (3). To us, their recoding seems unsuitable to improve a test for the influence of *earmarks* on investment, based on a *Senate chair* (not earmark) coefficient. In effect, the CCM coding attributes corporate investment declines to a Senator's earmarks when this Senator could not possibly have given them.

[2] CCM also always coded new firms as control observations, even when in treated states. For example, CCM coded the 464 firm-years from NY (not in the data set at the time of the first Senatorial appointment) as *control* observations. These NY firm-years had average capex of 7.3%, leaving only the 2,545 NY firm-years with average capex of 5.3% as *treated* observations. Because new firms have higher capex, this recoding of all new firms into the *control* set necessarily biases coefficients in favor of the CCM hypothesis. We note that with when new firms in treatment states are not coded as controls and with historical ascension coding, the R&D, change-in-employee coding, and sales growth coefficients switch signs. The employment coefficient sign switch resolves a puzzle in Ramey (2011): "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."

coefficients are similarly negative as those reported in CCM. The table thus suggests that the CCM year identification is not sharp. An objection is that with the same chairmanship durations, our recoding still picks up some overlap from the actual chairmanships. In the columns to the right of the original CCM coefficients, we always recode the CCM ascension and post-CCM ascension years as controls (0), and code only pre-ascension years as treatment (1). In effect, this allows testing the hypothesis that firms anticipated Senate ascension, reacted only pre-ascension, and then returned to normal levels beginning with ascension. Table 2 shows that if we use only one year, the standard errors are much higher, but the point estimates are similarly negative for capital expenditures, employee reductions, and sales declines. For the four years preceding the CCM ascension coding, the coefficient estimates and standard errors are similar to those reported in CCM. The R&D results moderate, though the CCM results are modestly stronger. Measured relative to our placebo null, the incremental R&D reduction on the CCM-coded ascension is insignificant.

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

When we looked at the data state by state, we realized that 94% of treated firm-years were from Texas and New York. In unreported separate regressions, we found that only Texas (with 3,097 firm-years) and Montana (with 14 firm-years) had large negative coefficients (–0.02 and –0.04) in capex regressions, whereas five out of eight chairmanship states had positive coefficients. Table 3 shows capex regressions that eliminate Texas from the sample (1987-92, Lloyd Bentsen, see Table 1). They suggest that the reported CCM capital-expenditure effect is a Texas effect. With controls, the coefficient is not –0.94%, but –0.03%.

The Texas basis and the magnitude of the CCM capex coefficient estimates invites further consideration. In 1991, the 447 Texas firms with Compustat data together had assets of $525 billion. A coefficient of –0.01 would imply Texas corporate capital-expenditure reductions of about $5 billion. Yet, in 1991, Texas received under $0.1 billion in earmarks. CCM acknowledge that the estimated coefficient is too large, but argue that the earmarks could have been the tip of an iceberg. However, we know of no plausible alternative channel with any evidence that could be large enough. Levitt and Poterba (1999) find no association between chairmanship and Federal flows. Fowler and Hall (2014) find that more senior Congressmen did not bring more discretionary federal outlays to their districts, although earmarks were only 2% of total outlays. The magnitude seems further startling when one realizes that (1) the headquarter variable is only a crude proxy of the (often world-wide) operations of many firms, which would suggest the true homestate effect would have to be even larger than the measured one. (2) The kinds of projects

that were classified as earmarks seem hardly the types that would crowd out corporate investment: of the $100 million in 1991, $92.6 million were earmarked for extending the Red River waterway to Shreveport (attributed in the CAGW report to be pork on behalf of the *other* Texas Senator, Bennett Johnston) and $75,000 for Plant Stress Research. And (3), typical crowding-out theories are based more on the substitution effect (with higher taxes, investment becomes less profitable) than on the income effect (corporations slack off). The costs of earmarks are not charged to the state itself, so only the income effect remains. Theoretically, earmarks could even have been complements. Our working paper investigated in some detail an alternative Texas channel: crude oil prices had increased from $14 in 1977 to $39.50 by 1980, remained above $25 until 1986, and then fell back to $15, just as Bentsen's chairmanship began. It is plausible that the stark decline and low level of crude oil prices during the Bentsen years, following years of higher prices, could have partly contributed to the reduction in Texas' private capital expenditures.

Table 3 also notes another issue: the treatment effect 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)). Table 3 shows that, when properly measured, the T-statistics in Table 3 drop from about 3 to 4 to about 1.5 to 1.7.

References

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Table 1: CCM vs. Historical Chairman Coding With Explanations

State	Senator	P	Cycle	Historical	CCM Coding	Departure	CCM Coding Notes
Louisiana	Long	(D)	74,80	1968-1980	1971	Maj Changed	excluded (minor early Compustat issue)
Kansas	Dole	(R)	80,86	1981-1984	no	Maj Leader	excluded because Dole was Ranking Member
Oregon (1)	Packwood	(R)	86,92	1985-1986	1985-1990	Maj Changed	continued 4 years beyond tenure
Texas	Bentsen	(D)	88,94	1987-1992	1987-1992	VP Candidate	
New York	Moynihan	(D)	88,94	1993-1994	1993-1998	Maj Changed	continued 4 years beyond tenure
Oregon (2)	Packwood	(R)	86,92	1995	no	Resigned	excluded due to scandal
Delaware	Roth	(R)	94,00	1996-2000	1996-2001	Election defeat, possibly endogenous. 2001 still coded.	
Iowa (1)	Grassley	(R)	98,04	2001	no	Maj Changed	excluded because Grassley was Ranking Member
Montana (1)	Baucus	(D)	02,08	2002	2001-2006	Maj Changed	mid-session changes; five out of six years not actual Chair
Iowa (2)	Grassley	(R)	04,10	2003-2006	no	Maj Changed	excluded because Grassley was Ranking Member
Montana (2)	Baucus	(D)	02,08	2007-2008	no	Maj Changed	excluded because MT shock had just ended

Explanations: P is the party affiliation; Cycle is the 6-year election cycle; Departure gives the reason for the chairmanship switch (most frequently, a change in majority of the Senate). The second half of the sample is separated with an extra space. It contained only three states, NY, DE, and MT. Baucus (MT) was chair from Jan 3 to Jan 20, 2001, and from Jun 2001 to Nov 2002. Grassley was chair from Jan 20, 2001 to Jun 6, 2001. Thus, either starting year 2001 or 2002 is reasonable, and neither choice makes much difference.

Interpretation: The only change which is possibly endogenous was when Sen. Roth was removed when he was not reelected in 2000. This changed one year (i.e., 2001) in the CCM coding vis-a-vis the historical coding. The remaining chair changes were due to other-state or country-wide electoral changes.

Table 2: Treatment Coefficient In Placebo Years Prior to CCM Ascenscion

Dependent Variable	Stat	Quasi-Placebo Ascent Starts Year –4	Year –1	Actual CCM Year 0	Table	No-Overlap Pre-Ascent –1	–4...–1
Cap Ex	Coef	–0.0168	–0.0137	–0.0122	T1 (1)	–0.0135	–0.0157
N=168,975	S.E. (sy)	0.0048	0.0040	0.0035		(0.0114)	(0.0057)
	pval	0.1%	0.1%	0.1%			0.6%
w/ controls	Coef	–0.0131	–0.0099	–0.0094	T1 (2)	–0.0067	–0.0111
N=139,564	S.E. (sy)	0.0041	0.0034	0.0030		(0.0103)	(0.0050)
	pval	0.2%	0.4%	0.2%			2.8%
R&D	Coef	–0.0014	–0.0027	–0.0043		–0.0000	–0.0019
N=87,865	S.E. (sy)	0.0014	0.0020	0.0020		(0.0018)	(0.0013)
	pval	31.1%	17.1%	0.3%			15.5%
w/ controls	Coef	–0.0028	–0.0040	–0.0045	T6 (A1)	–0.0006	–0.0030
N=74,842	S.E. (sy)	0.0015	0.0017	0.0017		(0.0031)	(0.0017)
	pval	5.2%	1.8%	0.8%			7.5%
Δ Employees	Coef	–0.0280	–0.0210	–0.0089	T6 (C1)	–0.0463	–0.0364
N=168,267	S.E. (sy)	0.0098	0.0112	0.0080		(0.0362)	(0.0116)
	pval	0.4%	6.2%	26.6%			0.2%
w/ controls	Coef	–0.0098	–0.0028	+0.0039		–0.0172	–0.0172
N=133,317	S.E. (sy)	0.0084	0.0090	0.0080		(0.0251)	(0.0095)
	pval	24.5%	75.2%	62.5%			6.8%
%Δ Sales	Coef	–0.0627	–0.0325	–0.0149	T6 (D1)	–0.0851	–0.0858
N=181,489	S.E. (sy)	0.0182	0.0175	0.0115		(0.0611)	(0.0212)
	pval	0.1%	6.4%	19.4%			0.1%
w/ controls	Coef	–0.0453	–0.0157	–0.0134		–0.0547	–0.0681
N=165,337	S.E. (sy)	0.0150	0.0134	0.0121		(0.0435)	(0.0169)
	pval	0.2%	24.3%	91.2%			0.1%

Explanations: The dependent variables are described in the first column. The independent variables include year and firm fixed effects, and "w/controls" further include lagged Q and leverage and contemporaneous asset-adjusted cash flow. The key coefficient reported in this table is on the independent variable that measures ascension to the Senate Finance Committee Chairmanship with timing as defined by CCM. The "actual CCM" column replicates the CCM regressions perfectly. In columns to the left, we code ascension treatment as if it had occurred x years earlier, but with the same duration as the actual chairmanship. In columns to the right, we code only the 1 year before the ascension or only the 4 years before the CCM ascension as "treatment" (0), and all CCM-coded treatment years as "control" (0). The standard errors are clustered by state-year, as in CCM, and pval's are double-sided.

Interpretation: The coefficients in "placebo" years are similar to those in actual years.

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

		Std. Error Clustering			
	Coef	State-Year	State	Adj. R^2	Firm-Years
(1) All Years	–0.0122†	(0.0035†)***	(0.0069)*	44.0%†	168,975†
(2) with add'l Cntrls	–0.0094†	(0.0030†)***	(0.0062)	50.1%†	139,564†
Without Texas					
(3) All Years	–0.0022	(0.0024)	(0.0011)*	43.2%	153,624
(4) with add'l Cntrls	–0.0003	(0.0021)	(0.0008)	49.2%	126,651

† indicates values that are identical to those reported in CCM .

*, ** , and *** indicate statistical significance at the 10%, 5%, and 1% confidence levels.

Explanations: For explanations, see Table 2. The first two regressions replicate the CCM regressions, the second two regressions omit Texas from the panel sample.

Interpretation: The CCM coefficient of –0.0094 in the "with-controls" specification becomes –0.0003 (i.e., de-facto zero) when Texas is excluded. The CCM effect in the 1968–2008 sample is primarily a Texas effect.