Comments

Do Powerful Politicians Really Cause Corporate Downsizing?

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Cohen, Coval, and Malloy (2011) 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 US 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. Cohen et al. 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.

Cohen et al. then reported that new chairmanships caused a decline in the consolidated worldwide 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 percent 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 circu-

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. Data are provided as supplementary material online.

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late in various blogs and commentaries on the internet. The peer-reviewed academic evidence in their article remains relevant and influential.

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

I. Ascension Coding

Cohen et al.'s 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 6 years. (3) No second senatorial shock was applied to a state when one was already in place for this state. (4) Oregon in 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 (Kansas in 1979 and Iowa 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 coding and the actual historical Senate chairmanships.

Cohen et al. 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 would have reduced their specific Senate dummy coding by only 1 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]

[1] Even if endogenous election outcomes had been pervasive, we could not understand Cohen et al.'s specific rules. In Monte Carlo experiments on some possible underlying models, the bias worsened when we applied their rules 1, 2, and 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 Cohen et al. coding attributes corporate investment declines to a senator's earmarks when this senator could not possibly have given them.

Cohen et al. also always coded new firms as control observations, even when in treated states. For example, they coded the 464 firm-years from New York (not in the data set at the time of the first senatorial appointment) as control observations. These New York firm-years had average capex of 7.3 percent, leaving only the 2,545 New York firm-years with average capex of 5.3 percent 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 Cohen et al. hypothesis. We note that 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" (681).

TABLE 1
CCM versus Historical Chairman Coding with Explanations

State	Senator	Party	Cycle	Historical	CCM Coding	Departure	CCM Coding Notes
Louisiana	Long	Dem.	1974, 1980	1968–80	1971	Majority changed	Excluded (minor early Compustat issue)
Kansas	Dole	Rep.	1980, 1986	1981–84	No	Majority leader	Excluded because Dole was ranking member
Oregon (1)	Packwood	Rep.	1986, 1992	1985–86	1985–90	Majority changed	Coded continued 4 years beyond tenure
Texas	Bentsen	Dem.	1988, 1994	1987–92	1987–92	VP candidate	
New York	Moynihan	Dem.	1988, 1994	1993–94	1993–98	Majority changed	Coded continued 4 years beyond actual tenure
Oregon (2)	Packwood	Rep.	1986, 1992	1995	No	Resigned	Excluded due to scandal
Delaware	Roth	Rep.	1994, 2000	1996–2000	1996–2001	Election defeat, possibly endogenous	
Iowa (1)	Grassley	Rep.	1998, 2004	2001	No	Majority changed	Excluded because Grassley was ranking member
Montana (1)	Baucus	Dem.	2002, 2008	2002	2001–6	Majority changed	Midsession changes; 5 in 6 years not actual chair
Iowa (2)	Grassley	Rep.	2004, 2010	2003–6	No	Majority changed	Excluded because Grassley was ranking member
Montana (2)	Baucus	Dem.	2002, 2008	2007–8	No	Majority changed	Excluded because MT shock had just ended

Note.—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 contained only three states, NY, DE, and MT. Baucus (MT) was chair from January 3 to January 20, 2001, and from June 2001 to November 2002. Grassley was chair from January 20, 2001, to June 6, 2001. Thus, either starting year 2001 or 2002 is reasonable, and neither choice makes much difference.

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

II. Placebos, Texas, and Clustering Issues

Most of our data were obtained from and are thus identical to those used in Cohen et al. (2011). The reader can consult their descriptive statistical tables (tables 1–3).

In the middle column of our table 2, we replicate Cohen et al.'s key 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 Cohen et al. With their original 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 Cohen et al. (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 coefficients are similarly negative as those reported in their original article. The table thus suggests that their 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 their ascension and post-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 1 year, the standard errors are much higher, but the point estimates are similarly negative for capital expenditures, employee reductions, and sales declines. For the 4 years preceding Cohen et al.'s ascension coding, the coefficient estimates and standard errors are similar to those reported in their original article. The R&D results moderate, though their results are modestly stronger. Measured relative to our placebo null, the incremental R&D reduction on their coded ascension is insignificant.

When we looked at the data state by state, we realized that 94 percent 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 Cohen et al.'s reported capital expenditure effect is a Texas effect. With controls, the coefficient is not -0.94 percent, but -0.03 percent.

TABLE 2

Treatment Coefficient in Placebo Years Prior to CCM Ascension

	Quasi-Placebo Ascent Starts			Actual CCM		No-Overlap Pre-Ascent
Statistic	Year −4	Year −1	Year 0	Table	−1	−4 . . . −1
	Dependent Variable: Capex (N = 168,975)					
Coefficient	−.0168	−.0137	−.0122	T1 (1)	−.0135	−.0157
Standard error (state-year)	.0048	.0040	.0035		(.0114)	(.0057)
p-value	.1%	.1%	.1%			.6%
	Capex with Controls (N = 139,564)					
Coefficient	−.0131	−.0099	−.0094	T1 (2)	−.0067	−.0111
Standard error (state-year)	.0041	.0034	.0030		(.0103)	(.0050)
p-value	.2%	.4%	.2%			2.8%
	Dependent Variable: R&D (N = 87,865)					
Coefficient	−.0014	−.0027	−.0043		−.0000	−.0019
Standard error (state-year)	.0014	.0020	.0020		(.0018)	(.0013)
p-value	31.1%	17.1%	.3%			15.5%
	R&D with Controls (N = 74,842)					
Coefficient	−.0028	−.0040	−.0045	T6 (A1)	−.0006	−.0030
Standard error (state-year)	.0015	.0017	.0017		(.0031)	(.0017)
p-value	5.2%	1.8%	.8%			7.5%
	Dependent Variable: Δ Employees (N = 168,267)					
Coefficient	−.0280	−.0210	−.0089	T6 (C1)	−.0463	−.0364
Standard error (state-year)	.0098	.0112	.0080		(.0362)	(.0116)
p-value	.4%	6.2%	26.6%			.2%
	Δ Employees with Controls (N = 133,317)					
Coefficient	−.0098	−.0028	+.0039		−.0172	−.0172
Standard error (state-year)	.0084	.0090	.0080		(.0251)	(.0095)
p-value	24.5%	75.2%	62.5%			6.8%
	Dependent Variable: %Δ Sales (N = 181,489)					
Coefficient	−.0627	−.0325	−.0149	T6 (D1)	−.0851	−.0858
Standard error (state-year)	.0182	.0175	.0115		(.0611)	(.0212)
p-value	.1%	6.4%	19.4%			.1%
	%Δ Sales with Controls (N = 165,337)					
Coefficient	−.0453	−.0157	−.0011		−.0547	−.0681
Standard error (state-year)	.0150	.0134	.0121		(.0435)	(.0169)
p-value	.2%	24.3%	91.2%			.1%

Note.—The independent variables include year and firm fixed effects, and "with 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 Coval et al. The "actual CCM" column replicates the Coval et al. 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 Coval et al.'s paper, and p values 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 HOME STATE, CCM CODING, 1968–2008

		Standard Error Clustering			
	Coefficient	State-Year	State	Adjusted R^2	Firm-Years
1. All years	−.0122[a]	(.0035)[a]***	(.0069)*	44.0%[a]	168,975[a]
2. With additional controls	−.0094	(.0030)[a]***	(.0062)	50.1%[a]	139,564[a]
Without Texas:					
3. All years	−.0022	(.0024)	(.0011)*	43.2%	153,624
4. With additional controls	−.0003	(.0021)	(.0008)	49.2%	126,651

NOTE.—For explanations, see the note to 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.

[a] Values are identical to those reported in Coval et al.'s original paper.

* Statistically significant at the 10 percent confidence level.

** Statistically significant at the 5 percent confidence level.

*** Statistically significant at the 1 percent confidence level.

The Texas basis and the magnitude of the CCM capex coefficient estimates invite 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. Cohen et al. 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 percent of total outlays. The magnitude seems further startling when one realizes the following: (1) The headquarter variable is only a crude proxy of the (often worldwide) operations of many firms, which would suggest that the true home state 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 was earmarked for extending the Red River waterway to Shreveport (attributed in the Citizens Against Government Waste report to be pork on behalf of the other Texas senator, Bennett Johnston) and $75,000 for Plant Stress Research. (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's 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; 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.

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