

Reply: Do Powerful Politicians Really Cause Corporate Downsizing?*

Lauren Cohen

Harvard Business School and NBER

Joshua Coval

Harvard Business School and NBER

Christopher Malloy

Harvard Business School and NBER

* We would like to thank Jason Snyder and Ivo Welch for helpful comments and engaging conversation throughout. We are grateful for funding from the National Science Foundation.

Reply: Do Powerful Politicians Really Cause Corporate Downsizing?*

ABSTRACT

While we commend the initiative of Snyder and Welch (2017), we lay out in this short reply why we remain highly confident in our results and our interpretation thereof. We welcome authors to continue to explore the data for themselves, and look forward to the new questions they can tackle with it.

We first want to say that we believe the close examination of the methodology and robustness of published studies is absolutely necessary in our profession. We commend Snyder and Welch for their effort in following up with the data and code we posted on our sites, and for investigating the questions explored in our paper, Cohen, Coval, and Malloy (JPE, 2011). We have had an engaging conversation with them for the past few years and we have enjoyed it.

This having been said, we still do not agree with the analysis, inferences, and conclusions of Snyder and Welch (2017). Indeed, their concerns have prompted us to carry out a large amount of additional analysis – including a number of tests that address critiques that have since been dropped from the current version of their paper – that has only strengthened our confidence in our results. The main tables of our original paper contain over 50 different sets of results that all point in the same direction: 10 different definitions of important senate committees in Tables III-VI in the original paper, 6 different designations of house committees, not to mention cross-sectional firm-level results, cross-sectional state-level results, and aggregate state-level results. Our confidence derives primarily from the fact that even if their main remaining critique were valid – and we will detail below why we think it is not – it only changes the outcome of *one* of these specifications.

In what follows, we will lay-out simply and concisely why we do not think their remaining concerns hold up well in the data. In fact, the results presented in our original paper in many instances grow stronger (economically and statistically) when we focus on the sub-periods suggested in their critique. We point all readers interested in more detailed analysis to the data and code on our websites. We do hope this exchange will spur more research on the topic, and our data and analysis will prove helpful to future researchers interested in exploring the rich area of the complementarities and substitution between public- and private-sector investment.

1. Central critique: Snyder and Welch argue that one of the main results of the paper is 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).

Our response: To begin with, it is important to emphasize that the only regression that becomes appreciably weaker when the Bentsen appointment is removed is the first regression

in each of the key tables -- the regression in which only the Senate Finance Committee is classified as powerful. At a high level, it should not be surprising that if one focuses on the single specification with the smallest number of influential observations out of all the regressions in the entire paper (there were only 6 ascension “shocks” to the chair of the Senate Finance Committee that we identified during our sample period) and finds reasons to remove one of the strongest observations, the results will weaken noticeably. Of course, one could easily do the opposite and find reasons (ex-post) to remove a weak observation and see the results get stronger.

In our view, a more sensible alternative is to deal with the broader economic argument – that the oil price drop and S&L crisis were important macroeconomic events that may have distorted our results – in the cleanest way possible: by removing the entire “problem” sample period and observations that are claimed to be driving our full-sample results.

If 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 focus only on the Senate Finance Chair shock that Snyder and Welch chose to analyze, we find that the results are large, robust, and significant across a range of specifications and outcome variables. These can be seen in the attached Table 1, Panels A and 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), it seems peculiar to conclude that the results are “driven” by this time period.

2. Secondary critiques – Snyder and Welch then go on to criticize the clustering, coding choices, and coefficient magnitudes.

Response: In Panels A and B of the attached Table I, 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. Nevertheless, since the results in the paper vary at a state-shock period level, we stand by the clustering choice we employed in the paper. We also stand by 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. We point also to the Pre-Shock variables that we employ in the original paper (for specifications other than just the Senate

Finance Chair shock, which is the sole focus of Snyder and Welch's critique, as noted above); these are important placebo tests that were included in our 2011 work, and again support our conclusions.

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 tractable 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.

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 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.

References

Cohen, Lauren, Joshua Coval, and Christopher Malloy. 2011. “Do Powerful Politicians Cause Corporate Downsizing?” *Journal of Political Economy* 119, no. 6 (December 2011): 1015–1060.

Snyder, Jason and Ivo Welch. 2017. “Do Powerful Politicians Really Cause Corporate Downsizing?” *Journal of Political Economy*, forthcoming.

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 and B 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%