How Do Demand Shocks Affect CEO Compensation? Evidence from Government Contractors

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Abstract

This paper examines the impact of demand shocks on CEO compensation and corporate outcomes. Using census-driven, long-term changes in U.S. government spending, we demonstrate that government contractors experience increased procurement opportunities, reduced demand uncertainty, and significantly improved investment efficiency. To fully adapt to and benefit from these demand shocks, corporate boards adopt more convex executive pay contracts, leading to higher expected CEO compensation. This effect is strongest in sectors characterized by stable demand and flexible product pricing, where risk-taking generates greater expected benefits. In this context, risk-based incentives effectively enhance investment activity and corporate performance.

Keywords: CEO Compensation; Demand Shocks; Government Spending.

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1 Introduction

Positive demand shocks provide corporations with significant opportunities for expansion and growth (Ferraz et al., 2015; Pozzi and Schivardi, 2016; Atkin et al., 2017; Hvide and Meling, 2023). However, misaligned managerial incentives, such as the pursuit of a quiet life or excessive risk aversion, can lead to decisions that fail to maximize shareholder value (Gaver and Gaver, 1995; Bertrand and Mullainathan, 2003).

Executive compensation structure emerges as a potential tool to align the interests of managers with those of shareholders. However, from a theoretical point of view, the relationship between demand shocks and managerial remuneration remains ambiguous. Simple contracting models suggest that managerial pay should not be affected by performance shocks beyond managers' control (Holmström, 1979; Holmstrom, 1982). Nevertheless, managers must undertake risky investments, such as expanding operational capabilities and increasing research and development spending, to fully adapt to and capitalize on these shocks (Ngo and Stanfield, 2022; Clemens and Rogers, 2023; Hvide and Meling, 2023), which risk-averse managers can be reluctant to pursue, making it optimal for boards to adjust managerial compensation incentives accordingly (Smith and Stulz, 1985; Guay, 1999; Coles et al., 2006; Belkhir and Chazi, 2010). In addition, positive demand shocks raise expected revenues and reduce demand uncertainty. This enables firms to benefit from additional customer-specific investments by improving managerial responsiveness to investment opportunities and expected firm profitability (e.g., Patatoukas, 2012; Irvine et al., 2016; Hui et al., 2019; Cohen and Li, 2020).

In this paper, we seek to provide an in-depth investigation into how demand shocks affect CEO compensation structure and, in turn, corporate policies and performance. A better understanding of the actions taken to navigate these shocks and an evaluation of the effectiveness and value creation of firm decisions can generate significant micro- as well as macro-economic implications, such as improving the optimization of long-term economic growth opportunities (Antolin-Diaz and Surico, 2022; Bernstein et al., 2022; Ilzetzki, 2024). Additionally, a comprehensive understanding of the CEO pay determination is central to the corporate finance literature and is also of great interest to the general public (Edmans et al., 2017).

To advance our research objective, we focus on government contractors and changes in government spending in the U.S., which we view as laboratory experiments. Government contractors are highly dependent on the level of public spending (Ngo and Stanfield, 2022). When government spending changes, these contractors experience substantial changes in demand for their products and services compared to other firms.[1]

[1] The idea of using government contractors to investigate how corporations react to demand shocks is not new. Goyal et al. (2002) use the defense industry during the Reagan defense build-up of the early 1980s to examine how growth opportunities affect the level and structure of corporate debt policies. Ngo and Stanfield (2022) exploit changes in government spending at the national level to investigate how government contractors and their respective peers adjust their expenditures in research and development. Moreover, Bargeron et al. (2018), Clemens and Rogers (2023), and Ilzetzki (2024) consider

From an empirical point of view, assessing the impact of changes in government spending on corporate outcomes is not an easy task and requires a carefully designed identification strategy. Indeed, estimating this relationship poses significant challenges, primarily due to the difficulty in identifying changes in government expenditures that are orthogonal to current economic conditions (Chodorow-Reich, 2019). To address this fundamental endogeneity problem that government spending generally responds negatively to the level of economic activity, we follow an empirical strategy proposed by Suárez Serrato and Wingender (2016) based on the fact that a significant determinant of the distribution of federal expenditures across counties is based on county population estimates and their revisions (Blumerman and Vidal, 2009). These figures undergo sudden changes after each decennial census count, resulting in significant shifts in government budget allocations. While the decennial census relies on an accurate physical count, annual population estimates, known as "postcensal" estimates, use administrative data to estimate yearly changes. Consequently, disparities between actual census counts and concurrent postcensal estimates introduce unpredictable measurement errors, which accumulate yearly over the decade between each census count.

We refer to the disparity between census counts and postcensal estimates as *census shocks*. Using information on the universe of government procurement contracts across counties and years in the U.S., we validate this approach and show that a census shock is associated with substantial changes in government expenditures at the county level. More specifically, a one-standard-deviation increase in the shock measure corresponds to an approximate annual increase of $22 million in government procurement spending in the affected county. In addition, we show that this effect is long-lasting, and leads to a substantial decrease in government spending volatility for the exposed counties.

In our empirical analysis at the corporate level, we follow Even-Tov et al. (2022) and compare firms headquartered in different counties, which are, therefore, heterogeneously exposed to decennial Census shocks. Our focus on the headquarters of a company is based on the idea that its business activities are typically concentrated near its headquarters (Pirinsky and Wang, 2006; Henderson and Ono, 2008; Chaney et al., 2012), and proximity to government project sites increases the likelihood of government departments relying on them (Bajari et al., 2014). This approach allows us to compare companies that serve as government contractors with those that are not, but are located *within* the same county. The identifying assumption of this "difference-in-difference-in-difference" (DDD) estimator merely requires that there are no other events in the county generating a difference in the differential trends of government contractors to non-government contractors which affect their relative outcomes (Gruber, 1994; Gruber and Poterba, 1994).

Using this approach, we provide corporate-level evidence that census shocks influence the likelihood that government suppliers discuss government procurement opportunities in their 10-K filings. Further-

the significant increase in demand for certain types of products during the Civil War, World War I, and World War II to investigate corporate outcomes as a response to demand shocks.

more, considering that reduced demand uncertainty enables better alignment of resources with expected needs, minimizing waste or overcapacity, we find that government contractors see a substantial increase in their investment efficiency, as documented by a closer alignment of firm investment with optimal CEO compensation benchmarks, measured by the absolute value of residuals from industry-year regressions linking investment to investment opportunities.

The central focus of this paper is to examine how board directors adjust CEO compensation levels following these demand shocks. We predict that demand shocks positively affect shareholder preferences regarding corporate risk-taking, given the increased availability of investment opportunities with a positive net present value and the enhanced marginal benefits of risk-taking activities (Ngo and Stanfield, 2022; Clemens and Rogers, 2023; Hvide and Meling, 2023). This is expected to positively affect the convexity of executive pay contracts that boards of directors select.

To investigate this compensation question, we use *Vega* as an outcome variable, representing CEO wealth sensitivity to stock return volatility (Coles et al., 2006). We document that census shocks prompt the boards of directors of government contractors to use more convex executive pay contracts. In terms of magnitude, the effect is economically meaningful; one standard deviation change in our government spending shock is associated with an increase in *Vega* equal to $15,000 for the average firm in our sample, which represents 14% of its mean value.

While our research reveals that demand shocks enhance the convexity of CEOs' compensation packages by documenting an increase in the number and amount of newly awarded stock options, we do not find evidence that government spending shocks influence CEOs' decisions regarding option exercise. Moreover, we observe no statistically significant changes in other forms of compensation. Simultaneously, we document that executives of affected firms derive a relatively large proportion of their pay from long-term forms of incentive compensation. This result suggests that corporate boards want to incentivize managers to avoid making myopic investment decisions in the presence of rapidly rising investment opportunities. In addition, this long-term compensation component offsets the potential adverse effects of high-vega incentives, such as excessive risk-taking (Lewellen et al., 1987; Gopalan et al., 2014; Kubick et al., 2017).

Overall, we find that an exogenous increase in government spending positively impacts the overall CEO compensation of government suppliers. The positive effect we document aligns with previous literature suggesting that CEO total compensation is predicted to be higher for firms with growing investment opportunities (Smith and Watts, 1992; Clinch, 1991; Gaver and Gaver, 1995; Baber et al., 1996). Indeed, higher compensation levels are expected because the selection of investment projects is assumed to command a higher equilibrium wage than the supervision of existing assets and ongoing investment projects.

To provide a more comprehensive understanding of the relationship between demand shocks and

CEO compensation, we show that our main results are stronger for firms that operate in sectors where government demand is less volatile and more predictable. This aligns with the idea that risk-taking becomes more valuable under these conditions, as they enable managers to make irreversible investment decisions with greater confidence (Cohen and Li, 2020). Additionally, the effect is more pronounced in sectors with less sticky prices. Flexible pricing enables firms to adjust their output prices swiftly in response to idiosyncratic shocks, thereby enhancing their ability to seize new opportunities and adapt to market conditions efficiently (Gorodnichenko and Weber, 2016; D'Acunto et al., 2018), raising the benefits of risk-taking incentives and justifying board decisions to offer more significant risk-taking incentives to their CEOs.

We also explore alternative explanations for our main finding and conduct a cross-sectional analysis to deepen our understanding of these results. One such alternative is the rent extraction hypothesis, which posits that CEOs use their power and influence to extract excessive compensation from the firm without necessarily exerting additional effort or bearing greater risk (e.g., Bebchuk and Fried, 2003; Bertrand and Mullainathan, 2001; Garvey and Milbourn, 2006). In contrast to this hypothesis, our results are primarily driven by the subsample of firms with good corporate governance. This suggests that effective governance mechanisms enable shareholders to align executive interests towards reaping growth opportunity benefits (Morse et al., 2011; Coles et al., 2014). Contrary to most of the empirical literature documenting a positive relationship between competition and risk-taking incentives (e.g., Cuñat and Guadalupe, 2009; Dasgupta et al., 2018), we also show that our main effect diminishes with increased competition to win government contracts, in line with predictions that such competition reduces the expected revenues and opportunities associated with demand shocks (Schmidt, 1997; Raith, 2003). Moreover, we find greater impacts on firms where CEOs are likely to be more risk-averse, such as those with higher inside debt (Wei and Yermack, 2011) or lower overconfidence (Campbell et al., 2011; Malmendier and Tate, 2005; Langer, 1975; March and Shapira, 1987; Ho et al., 2016), further supporting our primary hypothesis that incentives are used to motivate risk-averse managers and mitigate agency issues.

Finally, we demonstrate that adopting risk-taking incentives in this environment is effective. Managers who face an increase in *Vega* are more likely to make investments to enhance their firms' production capacity and pursue risky policies that could significantly improve corporate performance during this critical growth opportunity period. More specifically, we find that these government contractors are less likely to distribute dividends and instead redirect resources toward growth-oriented strategies, such as increasing capital expenditures, investing in research and development, and pursuing acquisition activities. At the same time, we document a positive effect on corporate sales growth and total factor productivity.

Previous literature and contribution. Our paper contributes to the extensive theoretical and empirical literature on the determinants and structure of CEO compensation. An early study in this literature by Bertrand and Mullainathan (2001) demonstrates that oil price changes influence the compensation of CEOs in the oil industry, that exchange rates impact pay in import-affected industries, and that other general industry shocks also affect CEO compensation. Since their results are driven by the sample of firms with weak governance, the authors attribute their findings to managerial skimming. Garvey and Milbourn (2006) reach similar conclusions by showing that CEO pay is more likely to be tied to industry benchmarks when they perform well than when they perform poorly. Feriozzi (2011) produce further empirical evidence on the existence of an asymmetry in "pay for luck", which is the finding that CEOs are rewarded for good luck, but are not penalized to the same extent for bad luck. However, the hypothesis of the existence of this asymmetry has recently been challenged by Daniel et al. (2020). Using over 200 different specifications, they conclude that there is no evidence to support the existence of "pay-for-luck".

In our paper, we demonstrate that positive corporate demand shocks impact CEO pay using government spending and government contractors as a laboratory experiment. As an additional exercise, we provide evidence that demand contractions decrease the convexity of CEO compensation. Furthermore, the sample of firms with good governance drives our results. Therefore, our findings contrast sharply with the predictions of the managerial skimming and "pay-for-luck" hypotheses.

These results also offer a new perspective on the Relative Performance Evaluation (RPE) theory of executive compensation (Antle and Smith, 1986; Gibbons and Murphy, 1990; Janakiraman et al., 1992; Aggarwal and Samwick, 1999). This theory suggests that optimal compensation contracts should filter out external shocks, and base CEO pay only on firm-specific performance. However, our findings indicate that, in the case of demand shocks, it may be optimal for boards to adjust CEO compensation in response to these exogenous factors. This apparent deviation from RPE theory can be explained by the fact that demand shocks create new investment opportunities that require specific managerial actions and risk-taking for them to be fully exploited.

Another line of inquiry investigates the effect of a negative shock to corporate cash flow on executive compensation incentives, and they report mixed evidence. More specifically, Gormley et al. (2013) and Choi et al. (2024) show that exogenous changes in left-tail risk and expected increases in production costs due to compliance with environmental regulations, respectively, reduce the convexity of CEO compensation. On the other hand, Cen et al. (2024) find the opposite results and document that a decrease in asset value leads to more risk-taking incentives.

Our paper provides valuable insights into these divergent findings and is the first to examine how changes in the demand for corporate goods and services influence the structure of CEO compensation. Despite the importance of positive demand shocks for firm growth and survival, they remain largely unexplored in the finance literature. Additionally, our analysis examines the roles that various demand shock characteristics play in this context, with particular emphasis on demand uncertainty and sticky prices, offering a comprehensive understanding of this relationship.

2 Data and variables

To advance our research, we gather information from alternative sources. This section aims to provide comprehensive details regarding the construction of our sample and the primary variables employed in our empirical analysis, along with their respective data sources.

After creating our census shocks variable, we use ExecuComp as a primary dataset, which contains detailed executive compensation information. This database is merged with financial and accounting data provided by the Center for Research in Securities Prices (CRSP) and Compustat. Following previous literature (e.g., Fang and Huang, 2024), we exclude from this sample financial firms with Standard Industrial Classification (SIC) codes between 6000 and 6999, as well as firms with missing information on CEO compensation. Finally, we remove singleton observations in our fixed-effects specification (Correia, 2016).

CEO compensation. Our primary dependent variable of interest, *Vega*, represents the sensitivity of CEO total compensation to stock return volatility, serving as a measure of the convexity of compensation payoffs. Specifically, it is defined as the dollar change in the value of the CEO's total portfolio of option grants for a 0.01 increase in the annualized standard deviation of a firm's stock returns (Coles et al., 2006). This variable is commonly employed to measure the convexity of executive compensation, and a significant body of literature indicates that *Vega* is positively correlated with manager risk-taking (Croci and Petmezas, 2015; Francis et al., 2017; Mao and Zhang, 2018).

In addition to *Vega*, we also analyze a CEO's *Delta*, which represents the slope of the CEO wealth-to-firm performance relationship. We define this variable as the change in the dollar value of an executive's compensation for a one percentage point change in the firm's stock price. Furthermore, to analyze changes in CEO compensation structure, including salary, bonus, stock grants, option grants, and other components, we construct a variable, *Total pay*, representing the natural logarithm of one plus a CEO's total compensation. In addition, we separately examine the proportion of each component of a CEO's total compensation (% *Salary*, % *Bonus*, % *Stock*, % *Option*, and % *Others*).

Finally, we consider two alternative measures to capture CEO option exercise behavior: *Options exercised* and # *Options exercised*. *Options exercised* is defined as the value of the options exercised by a CEO, while # *Options exercised* represents the number of options exercised scaled by the firm's number of outstanding shares. These variables will enable us to evaluate the timing of option awards to CEOs more precisely (Gormley et al., 2013; Chen et al., 2022).

Census shocks. Information on executive compensation is available starting from 1992. So, the sample period in our main dataset covers four decades and four census years (1990, 2000, 2010, and 2020). For our analysis, we collect county-level data on the four census years and the post-censal population estimates from 1981 to 2022.

The derivation of post-censal population figures incorporates various demographic dynamics, including migration patterns, births, and deaths. The original data on census population counts and post-censual estimations for 1991-2022 are published by the Census Bureau, while the information before the 1990 census is no longer available. Therefore, we complement this data source with other documentation that reports the original statistics before 1990. Specifically, we obtain the post-censal population estimates between 1981 and 1988 from County Statistics File 4 (CO-STAT 4) in the Inter-University Consortium for Political and Social Research (ICPSR) database.[2]

Figure OA1 shows the annual average of the county-level population growth rates from 1981 to 2022. The population growth rate in a non-census year is computed using post-censal population estimates published by the Census Bureau. The population growth rates in the 1990, 2000, 2010, and 2020 censuses are computed using the current census population count and the post-censal population estimates for the prior year. The growth rate shows significant changes due to population revisions following the 1990, 2000, 2010, and 2020 censuses that we exploit in our empirical analysis.

Our final measure, *Census Shock*, is the log difference between the concurrent census count and the post-censal population estimates of the census year. Formally, we define county j's Census shock in census year y^C as:

$$Census\ shock_{j,y^C} = log(Pop^{Census}_{j,y^C}) - log(Pop^{Estimated}_{j,y^C}) \tag{1}$$

where Pop^{Census}_{j,y^C} is the physical census population count of county j in census year y^C, and $Pop^{Estimated}_{j,y^C}$ is the post-censal population estimation of county j in the same year.

We report the graphical distribution of the shocks across periods in Figure OA2. The picture illustrates the significant heterogeneity in these exogenous variations which we exploit each decade. Positive and negative shocks are widely dispersed across the country during each decade. Moreover, census shocks do not appear to be correlated across census years, further supporting the validity of our approach.

Company headquarters. In our firm-level analysis, we merge the county census shocks (times 100 for ease of interpretation) with firms' headquarters counties. Since firms' headquarters locations reported in the Compustat database are non-historical, we adjust them on a historical basis using the information

[2]The estimated population in 1989 and 1990 are calculated based on the 1988 data and the population dynamic information of 1988-89 and 1989-90, which includes birth, death, and flows of migration. The county-level birth and mortality figures are collected from Vital Statistics and reported by the Centers for Disease Control and Prevention (CDC). Inflows and outflows of population migrations are gathered from the County-to-County, State-to-State, and County Income Study Files, 1978–92 (ICPSR 2937). For details, see `https://www.icpsr.umich.edu/web/ICPSR/studies/9806`.

from firms' SEC filings following the 10-X Header Data from the Loughran-McDonald master dictionary.[3] We also exclude firms with non-US headquarters. As the shock is decennial, we assign the shock of a given census year to the remaining years of the decade. For example, the census shock of a county j in 2000 is assigned to firm i whose headquarters is located in county j for the years 2000 to 2009.

Government dependent firms. We identify government contractors using the Compustat Segment dataset, which details a firm's major customers and their types as well as the corresponding sales.[4] In this way, we classify firms as government-dependent if they report the federal government as one of their major customers, exceeding 10 percent of sales. We use a binary indicator *GovCustomer* as our main measure and further require that the corresponding revenue information is not missing.

Summary statistics. Our final sample consists of 35,731 firm-year observations, including 2,814 U.S. public firms, from 1992 to 2022. We present the summary statistics of all the variables used in our empirical analysis in Table 1. A comprehensive description of all variables and their sources included in our sample is provided in Table OA1.

The mean value of *Vega* is \$105.88, and the mean value of *Delta* is \$637.58. On average, CEOs in our sample earn \$3.07 ($e^{8.03}/1000$) million annually, which consists of 26% stock grants, 24% option grants, 28% salary, 10% bonus, and 13% in other compensation. The mean value of the census shocks is 0.63%. On average, federal agencies contribute 2.33% of sales revenues to our sample firms, 9% of which are identified as government contractors. These statistics align well with existing studies (Kim and Nguyen, 2020; Ngo and Stanfield, 2022).

3 Procurement activities and census shocks

The U.S. federal government is one of the world's largest purchasers of goods and services. In 2022, it allocated approximately \$694 billion in contracts with outside companies and organizations, representing an increase of about \$3.6 billion from the previous year.

Contracts to corporations are awarded through a bidding process. The projects funded by these contracts are often highly complex, and the contracts frequently span multiple years (Bajari et al., 2014). These processes involve extensive planning, with solicitation periods averaging 55 days, and where competitive bidding is commonly used. In addition, procurement activities exhibit a sectoral bias, with a strong focus on manufacturing, construction, scientific research, and various administrative services, which differs significantly from private sector spending patterns (Cox et al., 2024).

[3] More information can be found at `https://sraf.nd.edu/sec-edgar-data/10-x-header-data/`.

[4] The customer information utilizes the statutory requirement that mandates firms to report customers representing usually more than 10% of total firm sales (Liu et al., 2021).

Table 1: Descriptive statistics

	(1) Count	(2) Mean	(3) SD	(4) p25	(5) p50	(6) p75
Vega	35,731	106.48	193.65	4.50	33.85	111.86
Flow vega	35,731	37.26	68.86	0.00	8.87	41.37
Delta	35,731	640.34	1575.94	66.09	180.89	517.56
Total pay	35,731	8.03	1.07	7.26	8.08	8.82
% Stock	35,731	0.25	0.40	0.00	0.15	0.45
% Option	35,731	0.24	0.51	0.00	0.16	0.39
% Salary	35,731	0.28	0.22	0.12	0.21	0.37
% Bonus	35,731	0.10	0.15	0.00	0.00	0.17
% Other	35,731	0.13	0.60	0.02	0.11	0.27
# Options exercised	35,731	0.15	8.22	0.00	0.00	0.06
Options exercised	35,731	31.28	1249.78	0.00	0.00	6.95
Pay duration	15,497	2.33	1.05	1.24	2.51	2.90
Census shock	35,731	0.62	3.06	-1.10	0.64	2.41
DBA shock	35,731	1.01	0.38	0.58	1.07	1.22
Senator shock	31,647	0.01	0.10	0.00	0.00	0.00
GovSales	35,731	2.30	9.88	0.00	0.00	0.00
GovCustomer	35,731	0.09	0.29	0.00	0.00	0.00
HighGovInd	32,418	0.50	0.50	0.00	1.00	1.00
HighGovSales	35,731	0.07	0.26	0.00	0.00	0.00
Sales	35,731	7.24	1.62	6.15	7.20	8.31
Sales growth	35,731	0.21	8.39	-0.00	0.08	0.19
ROA	35,731	0.13	0.10	0.09	0.13	0.18
Leverage	35,731	0.22	0.17	0.07	0.22	0.35
ExCash	35,731	0.09	0.09	0.03	0.07	0.13
Stock Return	35,731	0.17	0.77	-0.13	0.10	0.35
Tobin q	35,725	0.54	0.52	0.15	0.44	0.83
MtB	35,731	0.86	0.76	0.33	0.80	1.31
R&D	35,731	0.03	0.04	0.00	0.00	0.05
EmpGrowth	35,462	0.08	0.70	-0.03	0.03	0.12
CAPEX	35,687	0.06	0.08	0.02	0.04	0.08
AQC	33,633	0.04	0.14	0.00	0.00	0.03
DPS	32,521	0.01	0.04	0.00	0.00	0.02
TFP	26,680	-0.17	0.42	-0.39	-0.20	0.02

Notes: This table shows descriptive statistics of the variables used in our analysis. Our final sample consists of 35,731 firm-year observations, including 2,814 U.S. public firms, spanning 1992 to 2022. SD displays the standard deviation, p25 the first and p75 the third quartile of the respective variable. See Table OA1 for a detailed description of our variables and their sources.

Census shocks and county economic characteristics. In our empirical analysis, we exploit the fact that federal budget allocation by counties hinges significantly on population levels, serving as the foundation for many formula-based grants (Suárez Serrato and Wingender, 2016). A crucial assumption behind the validity of our identification strategy is that census shocks are not correlated with other local economic conditions that could also affect corporate outcomes. To test whether this assumption holds, we collect information on employment, population, and income per capita at the county level.[5] We then regress these county characteristics on the census population shocks, controlling for year- and state-fixed effects. The results are reported in Table OA3, including all years, or focusing specifically on census years in an alternative specification. Importantly, we do not find any evidence of a correlation between local county characteristics and individual census shocks, supporting the hypothesis that our treatment is orthogonal to other regional economic factors.

Government procurement activities and census shocks. Our empirical analysis is also based on the assumption that the census shocks we analyze can predict unexpected variations in government spending. While this approach has been used in other papers (Suárez Serrato and Wingender, 2016; Kim and Nguyen, 2020; Even-Tov et al., 2022), we test whether this assumption is true in our setting.

To do so, we collect information on federal government contracts from the USASpending database.[6] We aggregate the contract value at the county level beginning in 1999 since the data is incomplete for earlier periods (Brogaard et al., 2021). Next, following Kim and Nguyen (2020) and Suárez Serrato and Wingender (2016), we estimate the following regression model:

$$\mathit{Log(Federal\ Spending)}_{j,y^{C}+t} = \beta \cdot \mathit{Census\ Shock}_{j,y^{C}} + \delta_j + \theta_{y^{C}+t} + \varepsilon_{j,y^{C}+t} \tag{2}$$

In Equation (2), $\mathit{Log(Federal\ Spending)}_{j,y^{C}+t}$ is the natural logarithm of the value of federal procurement contracts in county j in year t after Census years y^{C}, with $t \in [1, 10]$. $\mathit{Census\ Shock}_{j,y^{C}}$ is the log difference between the post-censal population estimate and the concurrent census population count of county j during Census years y^{C}, as reported in Equation (1). δ_j and $\theta_{y^{C}+t}$ denote county and year fixed effects, respectively. Standard errors are clustered at the county level.

The regression results are shown in Table OA4. In all three columns, the coefficients of the variable *Census shock* are positive and statistically significant, indicating that our treatment is positively associated with the government funding allocation. In column (1), we run an unconditional regression with county and year-fixed effects. The result is robust when time-varying local economic characteristics are accounted for by including state-by-year fixed effects and when controlling for the changes in other county-level economic conditions, as shown in columns (2) and (3). According to the estimate of our

[5] This information is obtained from the following website: `https://www.bea.gov/itable/`. We report the summary statistics of our county-level dataset in Table OA2.

[6] For details, see `https://www.usaspending.gov/`.

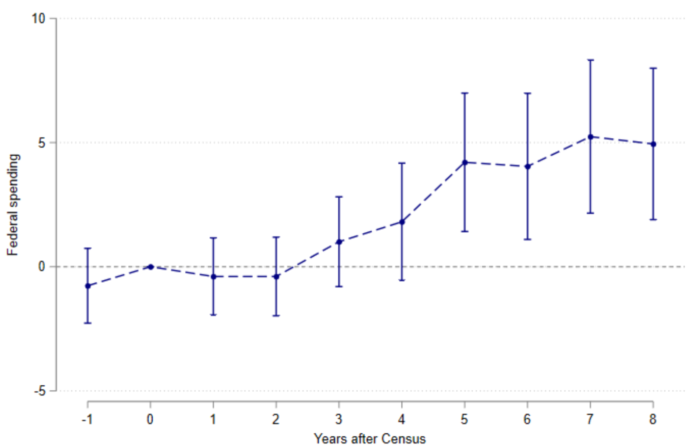
preferred specification, a one standard deviation increase in the shock is associated with 15% ($\approx$ 0.049 $\times$ 3.02) average contract value growth, which on average is equivalent to approximately $22 million per year in the county.

We also graphically present these yearly estimated coefficients in Figure 1. Specifically, we consider a stacked difference-in-differences specification (Cengiz et al., 2019), use the relevant census years y^C as the baseline year, and estimate the effects in other years related to a census year by employing a [y^{C-1}, y^{C+8}] event window. The sample is restricted to the period between 1999 and 2018, since the ten-year event window for the 2020 shock is incomplete.

In line with the results reported in Table OA4, the figure shows that expenditures by the Federal government undergo substantial changes in response to census shocks in the years succeeding the census. More specifically, we find a strong positive effect of the shock on the local allocation of federal spending in the third year after the new census year, which is persistent until the eighth year after the census shocks.

These dynamics are largely consistent with the existing literature, which indicates that the impacts of adjustments in federal government spending start two years after the census and persist for an additional four years (Suárez Serrato and Wingender, 2016; Kim and Nguyen, 2020). This deferred pattern is attributed to the fact that census data are generally disclosed one year following the execution of a census, and there is a temporal buffer for these figures to be thoroughly integrated into budget allocations of federal agencies.

Figure 1: Census shocks and federal government contract value

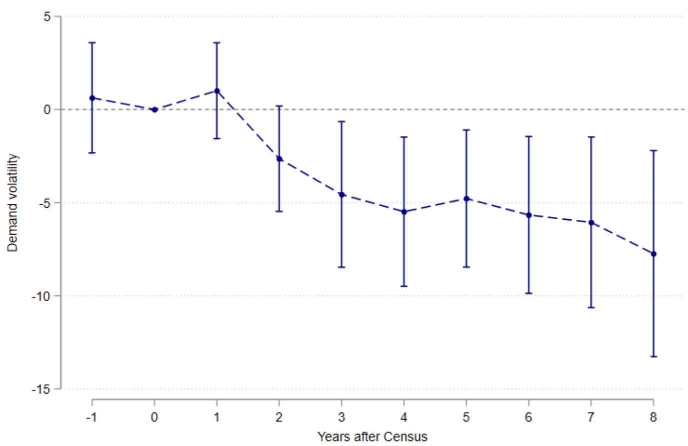


Notes: Figure 1 shows the effects of the census shocks on the logarithmic dollar amount of federal government spending at the county level in each year following a census year. *Federal Spending* is the logarithm of federal grants value in the county. *Census Shock* is the logarithmic difference between the postcensal population estimate based on the previous census and the current population count during census years. Standard errors are clustered at the county level. The confidence bands represent the 90% confidence level.

To better understand how census shocks affect the government procurement markets, we consider alternative outcome variables that proxy for government demand stability and competition for government contracts at the local county level. We present the dynamic graphical representation in Figure 2 using our primary variable, *Demand volatility*, which is the standard deviation of the monthly procurement amounts in a county. These dynamics suggest that the volatility of the procurement contract allocation is significantly reduced by the census shocks.

We further employ the concentration of federal procurement contractors, the average number of offers made to each contract weighted by contract size, and the proportion of competitive contracts, denoted as *contractor HHI*, *# Offers*, and *% Competitive*. The regression results for the average effects are reported in OA5, in which the odd and even columns show the unconditional estimates and estimates when control variables are added. We find that one standard deviation increase in the census shocks is associated with an 8.2% decline in *Demand volatility*, a 1.2% increase in *contractor HHI*, a 6.4% drop in *# Offers* per contract, and 1.1% decrease in *% Competitive*. Collectively, these results provide further evidence that census shocks are likely to benefit government contractors through increased and more stable demand for their products and services.

Figure 2: Census shocks and demand stability



Notes: Figure 2 shows the effects of the census shocks on the volatility of procurement allocation in each year following a census year. *Demand volatility* is the standard deviation of the monthly procurement amounts in the county. *Census Shock* is the logarithmic difference between the postcensal population estimate based on the previous census and the current population count during census years. Standard errors are clustered at the county level. The confidence bands represent the 90% confidence level.

4 Government spending and CEO compensation

CEO compensation incentives and census shocks. Once we validate our identification strategy, we aim to understand how corporate boards react to demand shocks, with a particular focus on CEO compensation incentives. Therefore, following Even-Tov et al. (2022), we estimate the following regression model:

$$
\begin{aligned}
Vega_{i,j,y^C+t} = \beta_1 GD_{i,j,y^C+t-1} \times Census\ Shock_{i,j,y^C} + \beta_2 GD_{i,j,y^C+t-1} \\
+\beta_3 Census\ shock_{i,j,y^C-1} + \beta_4 X_{i,j,y^C+t-1} + Fixed\ effects + \varepsilon_{i,j,y^C+t}.
\end{aligned}
\tag{3}
$$

In Equation (3), $Vega_{i,j,y^C+t}$ is the risk-inducing incentives provided in the CEOs' compensation of firm i headquarters in county j in t years after Census years y^C, and $t \in [1, 10]$. $Census\ Shock_{j,y^C}$ is the log difference between the postcensal population estimate and the concurrent census population count of county j during Census years y^C, and it varies by county decennially starting from the census years. GD_{i,j,y^C+t-1} is the government dependence measure of firm i headquarters in county j in $t-1$ years after Census years y^C. For the main results, we use a binary indicator *GovCustomer* that equals one if a firm discloses the U.S. government as a major customer, and zero otherwise. X_{i,j,y^C+t-1} is a matrix that includes the lagged time-varying control variables that potentially impact the convexity of compensation packages that boards award to CEOs, as documented by prior studies (Coles et al., 2006; Liu et al., 2021). More specifically, the matrix includes CEO pay-for-performance sensitivity (*Delta*), firm size (*Sales*), profitability (*ROA* and *Sales growth*), capital structure (*Leverage*), cash holding (*ExCash*), and market performance (*Stock return*). We include firm-fixed effects to control for any time-invariant firm-specific factors and industry-by-year fixed effects to capture industry trends over time. We further account for time-varying local economic conditions using state-by-year fixed effects. Finally, robust standard errors are clustered at the firm level (Petersen, 2008; Wooldridge, 2010).

Table 2 shows our main results. We are particularly interested in β_1 of Equation (3), which presents the effect of the exogenous variation in local government spending on CEO risk incentives of government-dependent firms. Regarding the regression specifications reported in columns (1) to (3), we start by showing estimates with only the standard firm and industry-year fixed effects, followed by a specification in which we account for state-year fixed effects and time-varying firm characteristics as additional controls. The coefficients of the interaction term are always positive and statistically significant, indicating that boards of government contractors promote CEO risk-taking incentives through an adjustment to compensation contracting, in reaction to the expansion of federal spending.

In terms of economic magnitude, when we consider our preferred specification reported in column (3) as a benchmark, one standard deviation (3.06) increase in *Census Shock* increases *Vega* by \$15,000 ($\approx 5.000 \times 3.06$) for government dependent CEOs. It suggests that CEOs of government contractors gain an additional \$15,000 in wealth when the annualized daily stock return volatility increases by 0.01. This change in CEO wealth is economically meaningful and corresponds to approximately 14% of the sample mean of *Vega* and 7% of its sample standard deviation.

In Equation (3), the continuous treatment variable *Census shock* takes both positive and negative values and captures the level of a firm being affected by the decennial shock. A positive (negative) value indicates that the population of an area is underestimated (overestimated) according to a decennial census, which therefore triggers an exogenous increase (decrease) in federal spending allocation (Suárez Serrato and Wingender, 2016). Hence, we categorize census shocks as positive and negative demand shocks and present the results in column (4). Specifically, we separate the shock into four groups based on quartiles of the original variable. *Census shock(>P75)* and *Census shock(≤P25)* take the value of one (zero otherwise) if values of the *Census shock* sit in the top and bottom groups which indicate considerable positive and negative shocks. The other two groups in the middle that have not experienced a significant estimation error are used as a reference group.

We observe symmetrical effects and that both types of demand shocks influence the convexity of CEOs' executive compensation. In terms of magnitude, the *Vega* of CEOs serving in government-dependent firms increased by $20,000 after a more extremely positive census shock, and decreased by $21,000 after a more extremely negative census shock. These findings contradict the existence of an asymmetry in the pay-for-luck and skimming hypotheses (Bertrand and Mullainathan, 2001; Garvey and Milbourn, 2006; Feriozzi, 2011).

Dynamic treatment effects. Following Cengiz et al. (2019), we perform an event study based on a stacked triple difference-in-differences (DDD) analysis to understand the dynamics of CEO compensation adjustments better. In addition, this approach identifies effects within each stack, avoiding erroneous comparisons of late to early implementing units that can bias classic Two-Way Fixed Effects (TWFE) estimates (Goodman-Bacon, 2021).

As well-documented by several existing studies and confirmed in our empirical evidence reported in Figure 1, the census shocks are a strong driver of the local allocation of federal funding two years after each decennial census (Suárez Serrato and Wingender, 2016; Kim and Nguyen, 2020). Similar to our prior analysis on federal spending, a $[y^{C-1}, y^{C+8}]$ event window for each census shock is used to estimate the effects on *Vega*. To ensure the completeness of the ten-year periods for each decennial shock, we restrict our sample to 1999 through 2018, which fully covers the ten-year intervals of the two census shocks in 2000 and 2010. Next, we estimate the dynamic treatment effects as reported in the following generalized model:

$$\begin{aligned} Vega_{i,j,y^{C+t}} = \sum_{\substack{t=-1 \\ t\neq 0}}^{t+8} \beta_t (GovCustomor_{i,j,y^{C+t-1}} \times Census\ Shock_{i,j,y^{C-1},y^{C+8}} \times \sum_{\substack{t=-1 \\ t\neq 0}}^{t+8} \tau_t) \\ + \lambda_{i,j,y^{C+t-1}} + Fixed\ effects + \varepsilon_{i,j,y^{C+t}} \end{aligned} \tag{4}$$

y^C refers to the census years 2000 and 2010. $Census\ Shock_{j,y^{C-1},y^{C+8}}$ indicates a population estimation error of county j where firm i is located for event years -1 to +8 relative to the census year

Table 2: Census population shocks and CEOs' incentive pay of government contractors

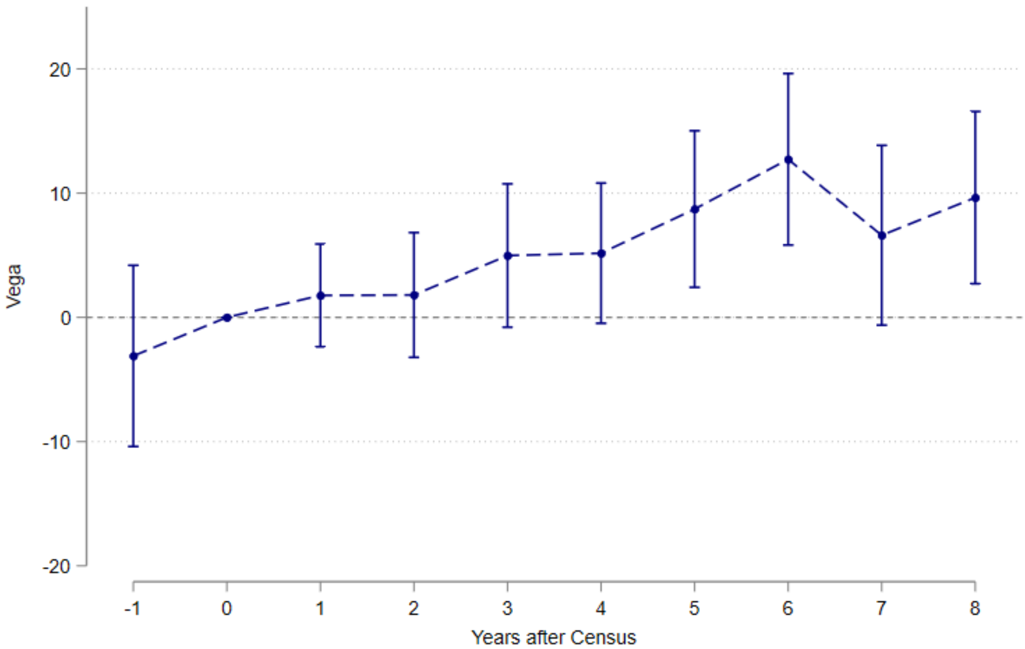
Dependent variable: Vega	(1)	(2)	(3)	(4)
GovCustomer × Census shock	2.892*	3.591**	5.000***	
	(1.713)	(1.805)	(1.729)	
GovCustomer × Census shock (>P75)				20.300***
				(7.712)
GovCustomer × Census shock (≤P25)				-21.044**
				(8.269)
GovCustomer	-1.679	-3.144	-7.981	-5.036
	(8.085)	(8.043)	(7.765)	(6.098)
Census shock	-2.137**	-0.422	-0.607	
	(0.892)	(1.006)	(0.962)	
Census shock (>P75)				1.317
				(3.222)
Census shock (≤P25)				11.314***
				(3.266)
Sales			40.637***	40.692***
			(3.816)	(1.721)
Leverage			-24.461*	-23.893***
			(14.216)	(7.921)
ROA			26.603	27.202*
			(22.173)	(14.783)
Delta			0.029***	0.029***
			(0.003)	(0.001)
Sales growth			-0.065*	-0.064
			(0.035)	(0.086)
ExCash			16.827	17.156
			(17.675)	(14.299)
Stock Return			-2.482*	-2.447**
			(1.313)	(1.035)
Firm FEs	Yes	Yes	Yes	Yes
Industry-Year FEs	Yes	Yes	Yes	Yes
State-Year FEs	No	Yes	Yes	Yes
Observations	35,731	35,731	35,731	35,731
Adj. R^2	0.543	0.544	0.579	0.580

Notes: This table shows regression results from Equation (3). We use *Vega* as our outcome variable. The sample period is fiscal year 1992 to 2022. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. *Census Shock* is the logarithmic difference between the postcensal population estimate based on the previous census and the current population count during census years in the county of firm headquarters. *Census shock(>P75)* and *Census shock(≤P25)* take the value of one (zero otherwise) if values of the *Census shock* sit in the top and bottom quartiles. *GovCustomer* is a binary variable that equals one (zero otherwise) if a firm reports the federal government as one of its major customers. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

y^C. τ_t is an indicator of each period relative to the census year. Census years, i.e., $t = 0$ (2000 and 2010), are omitted and used as a reference group for the dynamic estimation. $\lambda_{i,j,y^{C+t}-1}$ represent the remaining interacted control terms derived from the triple interaction. Following our standard baseline specification, we include firm, industry-by-year, and state-by-year fixed effects.

We plot β_t where $t \in [-1, 8]$ in Figure 3. Supporting the parallel trend assumption, we find that the coefficients from τ_{-1} to τ_2 are statistically insignificant and close to zero. We observe a considerable increase in the coefficients from τ_2 to τ_3 and τ_4. Importantly, the treatment effect becomes particularly strong from τ_5 to τ_8, consistent with the shock becoming effective three years after a census year. Overall, this evidence supports our baseline results that decision-makers of government contractors boost their CEOs' risk-taking incentive in response to a federal spending expansion.

Figure 3: Dynamic effect of census shocks on CEO Vega



Notes: Figure 3 displays the event study estimates of the triple difference-in-differences specification reported in Equation (4). We use *Vega* as an outcome variable. Standard errors are clustered at the firm level. The confidence bands are at the 90% confidence level.

What is driving the increase in Vega? We then next delve into what factors contribute to the increase in *Vega*. Indeed, *Vega* can increase due to direct changes in executive compensation packages approved by corporate boards or due to CEO adjustments in their corporate securities holdings, such as option exercises (Choi et al., 2024). To test these two alternative hypotheses, we consider outcome variables related to the structure of CEO compensation and then estimate Equation (3) again.

We present the results in Table 3. The positive and statistically significant coefficient in column (1) suggests that the census shock is positively associated with the proportion of option grants (*%Option*). Specifically, one standard deviation increase in the shock is associated with a 2 percentage point rise in option awards (8% of the sample mean) relative to the total compensation package received by CEOs of government contractors. At the same time, we observe no statistically significant changes in the share of other forms of compensation, according to the estimates reported in columns (2) to (5). These results reveal that expanded procurement opportunities enhance the convexity of CEOs' compensation packages, driven by additional option awards approved by boards of directors.

As reported in the last column, we also observe an increase in the CEOs' total pay amount. Specifically, one standard deviation increase in the shock intensity results in a 6% rise in the total pay of CEOs of government contractors. Considering the sample average, the increase in total pay is equivalent to a

$180,000 change in CEO wealth. This positive effect on CEO compensation that we document aligns with previous literature suggesting that CEO total compensation is predicted to be higher for firms with growth opportunities (Smith and Watts, 1992; Gaver and Gaver, 1995). Indeed, higher compensation levels are expected because the selection of investment projects is assumed to command a higher equilibrium wage than the supervision of existing assets in place or ongoing investment projects.

Table 3: CEO compensation structure

Dependent variables:	(1) % Option	(2) % Stock	(3) % Salary	(4) % Bonus	(5) % Other	(6) Total pay
GovCustomer × Census shock	0.006**	0.002	-0.002	0.001	-0.006	0.020***
	(0.003)	(0.003)	(0.002)	(0.001)	(0.004)	(0.007)
GovCustomer	0.012	-0.010	0.011	-0.003	-0.012	-0.066**
	(0.014)	(0.012)	(0.009)	(0.006)	(0.016)	(0.031)
Census shock	-0.002	0.002	-0.002*	0.000	0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Controls in Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,731	35,731	35,731	35,731	35,731	35,731
Adj. R^2	0.090	0.383	0.503	0.482	0.079	0.721

Notes: This table shows regression results for Equation (3). We use alternative outcome variables, as reported in the second row of the table. The dependent variables are the proportion of total annual CEO compensation that comes from option grants (*% Option*), stock grants (*% Stock*), and the proportion of total annual CEO compensation that comes from salary (*% Salary*), bonuses (*% Bonus*), and the logarithm of one plus the CEO's total compensation (in thousands) (*Total pay*). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

We also investigate CEO behavior in restructuring option holdings. We report the results in Table 4. Using the dollar value and frequency measures of CEO option exercises in columns (1) and (2), respectively, we do not find any indication that government spending shocks influence CEO conduct regarding option exercise against the hypothesis that the documented variation in *Vega* is triggered by executives' own portfolio decision.

Overall, these findings suggest that an exogenous increase in government spending has a positive and statistically significant impact on both the option-based and overall CEO compensation of government contractors and aligns with the notion that new investment opportunities prompt corporate boards to revise executive compensation to encourage more risk-taking.

5 Cross-sectional analyses

In this section, we run a cross-sectional analysis to understand when boards of directors incentivize managerial efforts through revisions in CEO compensation packages after the Census-driven demand shocks.

Table 4: Options exercised

Dependent variable:	(1) Options exercised	(2) # Options exercised
GovCustomer × Census shock	-3.072	0.001
	(5.973)	(0.014)
GovCustomer	0.648	-0.033
	(9.134)	(0.062)
Census shock	1.261	-0.000
	(2.041)	(0.020)
Controls in Table 2	Yes	Yes
Firm FEs	Yes	Yes
Industry-Year FEs	Yes	Yes
State-Year FEs	Yes	Yes
Observations	35,731	35,731
Adj. R^2	0.024	0.014

Notes: This table shows regression results for Equation (3). We use alternative outcome variables, as reported in the second row of the table. *Options exercised* is the dollar (in thousands) value of options exercised by the CEO in the current year, and *#Options exercised* is the number of options exercised by the CEO in the current year scaled by outstanding shares. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

The role of demand characteristics. To provide a more comprehensive understanding of the relationship between demand shocks and CEO compensation, we exploit additional features of these types of shocks.

First, we construct alternative measures of government demand stability using information on the universe of government contracts available in *USAspending*. More specifically, Cox et al. (2024) argue that there is significant heterogeneity across sectors in government spending volatility, which we proxy using the monthly standard deviation. Additionally, there is considerable heterogeneity in the average duration of government contracts. We use these two characteristics measured at the sector level as proxies for demand stability, as less volatile spending and longer-term contracts are associated with more stable revenue streams. The average values of these characteristics measured at the sector level are reported in Panels (a) and (b) of Figure OA3.

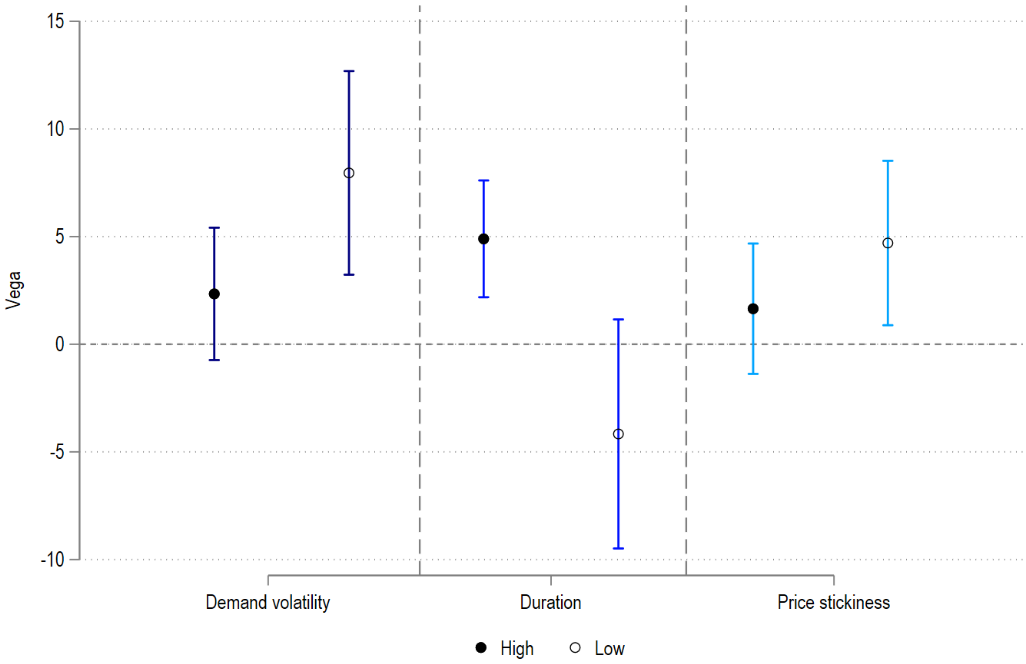
The first two panels of Figure 4 provide evidence that our main results are driven by the sample of firms where the government demand is less volatile, and thus, more stable and predictable. This aligns with the idea that risk-taking becomes more valuable under these conditions since these conditions allow managers to make irreversible, but less risky investments (Cohen and Li, 2020).

We next explore the role of procurement price stickiness and plot the values of this indicator in Panel (c) of Figure OA3.[7] In Figure 4, we show that the effect is stronger for the sample of firms that operate in sectors where prices are less sticky. Indeed, flexible prices allow firms to adjust to changes in demand and overall market conditions more quickly. This enables companies to better capitalize on new opportunities (Gorodnichenko and Weber, 2016; D'Acunto et al., 2018), potentially justifying greater

[7] We obtain this information from Pasten et al. (2020). They build this measure using data on the frequency of producer price changes at the two-digit NAICS code, derived from the micro-data that underlie the construction of the Producer Price Index (PPI) reported by the Bureau of Labor Statistics.

risk-taking incentives for CEOs.

Figure 4: The role of demand features



Notes: Figure 4 shows the coefficient of the interaction coefficient β_1 from Equation (3). We use *Vega* as our outcome variable. We further split the sample based on the level of government spending volatility, contract duration, and price stickiness at the sector level, as proxied by the variables we reported in x axis. The confidence bands are at the 90% confidence level. Robust standard errors are clustered at the firm level. See Table OA1 for a detailed description of the variables and their data sources.

The role of corporate governance. Given a CEO's superior insight into future opportunities compared to other shareholders (Heckerman, 1975), we consider a potential CEO rent extraction outcome in setting executive compensation (e.g., Bebchuk and Fried, 2003; Bertrand and Mullainathan, 2001; Garvey and Milbourn, 2006). This perspective suggests that executives may use their advanced knowledge to adjust their future pay relative to performance without exerting additional effort, potentially resulting in an inefficient compensation equilibrium.

To explore this phenomenon, we investigate whether the effect is heterogeneous depending on the quality of corporate governance or the existence of agency issues. If the executive rent-seeking conjecture holds, we would expect that our main result to be driven by the sample of firms with poorer governance since more entrenched CEOs have a higher ability to extract economic benefits from shareholders. Otherwise, we would observe a more pronounced effect from better-governed firms whose boards are more capable of aligning managerial risk-taking incentives with shareholder interests.

We consider alternative measures of CEO entrenchment and corporate governance. First, we use *Co-opted board*, which measures the share of directors joining the board after the CEO's appointment

(Coles et al., 2014). We also consider *Chairman-CEO*, a variable that identifies whether a CEO also serves as the board's chairperson. *CEO bargaining* compares the remuneration of a CEO with that of the top-ranking non-CEO executives, and it has been used to identify powerful CEOs (Bebchuk et al., 2011). Finally, we consider the variable *Institutional Ownership*, which denotes the proportion of a firm's equity owned by institutional investors.

Using these alternative outcome variables, we estimate the baseline specification reported in Equation (3) considering alternative sub-samples. More specifically, we split our sample using the median value of each variable as a cutoff, or whether or not *Chariman-CEO* assumes a value equal to one or zero. We next report the results in Figure 5.

Across the regression specifications, the estimates suggest that the significant positive effect of *Census shock* on *Vega* for government suppliers is driven by firms with a lower proportion of co-opted board members, a non-chairperson CEO or a CEO without strong bargaining power, and substantially lower levels of institutional ownership. These findings do not support the rent-extraction hypothesis (Bertrand and Mullainathan, 2001; Garvey and Milbourn, 2006; Feriozzi, 2011) and, instead, support the idea that boards of government-dependent firms award additional option-based compensation to their CEOs to align their interest with those of the shareholders and to improve risk-taking incentives in response to positive demand shocks.

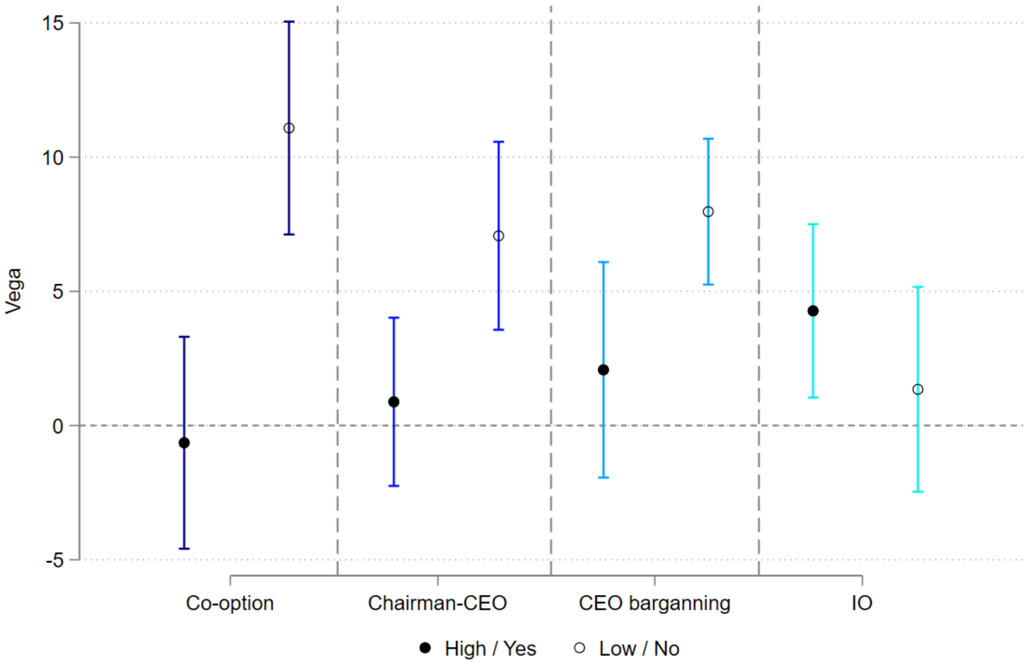
The role of competition. Competition is also considered a key determinant of CEO pay. Most empirical evidence reports that competition increases managerial compensation (Hubbard and Palia, 1995; Crawford et al., 1995; Cuñat and Guadalupe, 2009; Dasgupta et al., 2018). These results are in line with the hypothesis that boards of directors seek to strengthen CEO incentives in more competitive environments.

The theory leads to different conclusions when competition reduces the firm's expected revenues (Schmidt, 1997; Raith, 2003). Specifically, if greater competition leads to lower expected profits, the value of managerial effort declines. In addition, as reported in our theoretical framework, we then expect that boards of directors will design flatter incentive contracts because the set of investment opportunities with positive net present value will be reduced with increased competition.

We assess competition by considering the threat posed by competitors in securing government contracts at the local level. Specifically, we use an HHI index, calculated based on sales to the U.S. government, as a proxy. Moreover, we consider the share of government contracts at the sector level that was subject to competitive bidding.

We estimate Equation (3) separately on samples where government procurement competition is above the median and below the median. In line with our hypothesis, Figure 6 shows that the positive demand shock effect is less pronounced when there is higher competition for government contracts, potentially

Figure 5: The role of corporate governance



Notes: Figure 5 shows the coefficient of the interaction coefficient β_1 from Equation (3). We use *Vega* as our outcome variable. We further split the sample based on the quality of corporate governance, as proxied by the variables we reported in x axis. The confidence bands are at the 90% confidence level. Robust standard errors are clustered at the firm level. See Table OA1 for a detailed description of the variables and their data sources.

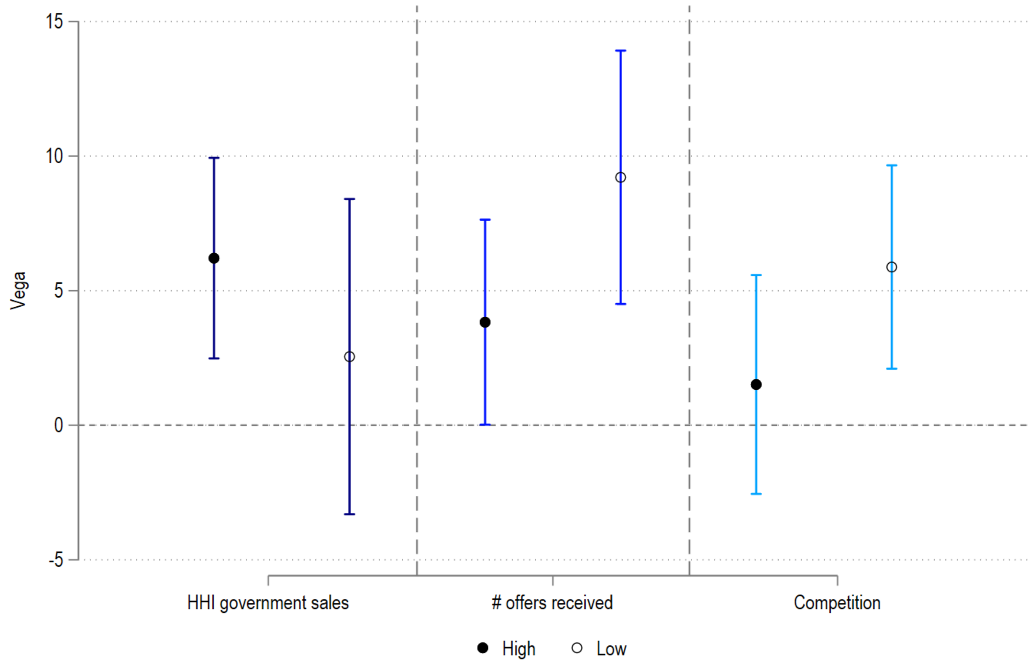
diminishing the expected revenues government contractors anticipate.

The role of CEO risk-aversion. Until now, we have not considered the role of CEO risk-aversion. This factor accounts for the fact that an increase in firm risk decreases managers' expected utility. Managers are typically risk-averse because they are poorly diversified with respect to corporate performance. Thus, they require higher compensation to manage additional risks. However, measuring the risk-aversion component requires access to information on a manager's wealth, which is unavailable in public or commercial databases.

To proxy for managers' risk aversion in our setting, we explore whether the effect of demand shocks on incentive pay contracts depends on CEOs' portfolios of inside debts. In fact, inside debt makes managers more risk-averse by linking their future wealth to corporate bankruptcy, thus discouraging excessive risk-taking (Wei and Yermack, 2011). More specifically, we estimate Equation (3) separately for samples of CEOs with above- and below-median debt-like claims (i.e., pensions and other deferred compensation) scaled by total compensation.

We show the resulting interaction term estimates in Figure 7. We find that firms promote CEO risk incentives when they hold more inside debts, indicating that shareholders are more likely to incentivize

Figure 6: The role of local government procurement competition



Notes: Figure 6 shows the coefficient of the interaction coefficient β_1 from Equation (3). We use *Vega* as our outcome variable. We further split the sample based on the level of government procurement competition at the local and sector levels, as proxied by the variables that we reported in x axis. The confidence bands are at the 90% confidence level. Robust standard errors are clustered at the firm level. See Table OA1 for a detailed description of the variables and their data sources.

CEOs who have more safety-seeking personal incentives.[8]

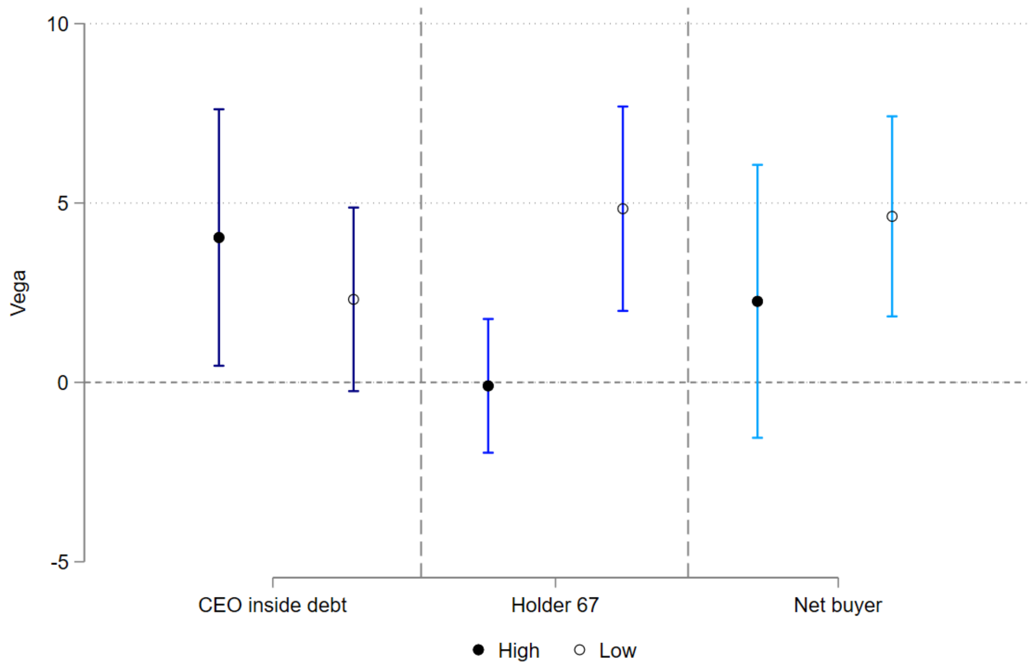
In a similar vein, we identify overconfident CEOs based on their propensity to voluntarily retain in-the-money stock options after the vesting period (Campbell et al., 2011), and as CEOs who continue on net to buy the firm stocks despite high exposure to firm risk (Malmendier and Tate, 2005). Specifically, *Holder67* is defined as an indicator that equals one if a CEO fails to exercise options despite a 67% or higher increase in stock price since the grant date, and zero otherwise. On the other hand, *Net buyer* equals one if a CEO is a net buyer of the company stocks during the first 5 years of her tenure, and zero otherwise.

As shown in Figure 7, the estimates demonstrate that the need for convex executive contracts is significantly reduced after positive demand shocks in firms with overconfident CEOs. The rationale behind this result is that overconfident individuals tend to believe they have complete control over their actions and underestimate the likelihood of failure (Langer, 1975; March and Shapira, 1987). Therefore, in our setting, the necessity to employ risk-taking compensation incentives to motivate managers diminishes, as

[8]In unreported tests, we find a similar result when we split the sample based on the CEO relative leverage, which refers to the ratio of the CEO's debt-to-equity ratio to the firm's debt-to-equity ratio (Wei and Yermack, 2011; Cassell et al., 2012).

smaller dollar amounts are predicted to be sufficient to induce managers to pursue growth opportunities.[9]

Figure 7: The role of CEO risk-aversion



Notes: Figure 7 shows the coefficient of the interaction coefficient β_1 from Equation (3). We use *Vega* as our outcome variable. We further split the sample based on the level of CEO overconfidence and CEOs' existing portfolio of firm inside debts, as proxied by the variables we reported in x axis. The confidence bands are at the 90% confidence level. Robust standard errors are clustered at the firm level. See Table OA1 for a detailed description of the variables and their data sources.

6 Corporate policies and risk-taking incentives

We investigate whether CEO incentives affect corporate performance in this setting. In line with this hypothesis, a large empirical literature suggests that compensation incentives affect CEO behavior and corporate policies (Guay, 1999; Coles et al., 2006; Croci and Petmezas, 2015; Francis et al., 2017; Mao and Zhang, 2018).

To test whether it is also true in our setting, we follow Chava et al. (2013) and Even-Tov et al. (2022). Specifically, we classify firms using an indicator variable that equals one if they experience an above-median change in *Vega* (i.e., ΔVega). Next, we fully interact this term with *Census Shock* and *GovCostumer*. Using this approach, we consider alternative outcome variables to evaluate corporate

[9]Humphery-Jenner et al. (2016) and Gervais et al. (2011) We also analyze the relationship between risk-taking incentives and CEO compensation, suggesting a different perspective from the one proposed in this paper. They argue that if an overconfident CEO assigns a high probability of success to a project, it becomes relatively inexpensive for the firm to offer a compensation package with high options and stock intensity. Therefore, in their theoretical framework, the purpose of a compensation contract with high equity intensity, at the margin, is to capitalize on the CEO's misvaluation rather than to provide incentives. This concept, referred to as the *exploitation hypothesis*, predicts a positive association between overconfidence and the incentive intensity of CEO compensation contracts.

policy and performance changes. We report the results in Table 5.[10]

Estimation results in column (1) prove that government contractors adopting risk-taking incentives are less likely to distribute dividends after census shocks (Lambert et al., 1989). The identified effect is economically meaningful and equal to approximately 20% relative to its mean value after one standard deviation change in the census shocks.

We also demonstrate that these firms are more likely to increase capital expenditures and engage in acquisition activities. Indeed, the triple interaction is positive and statistically significant, as reported in columns (2) and (3). One standard deviation increase in the census shock is respectively associated with a 0.36% and 1.07% increase in capital expenditure and investment in acquisitions related to the total assets (about 6% and 25% of their sample mean values).

We do not find in column (4) any statistically significant difference in research and development expenditures, although the estimation of the triple interaction term is positive. However, we do document that these risky policies positively affect overall corporate risk, as reported in column (5). Specifically, the annualized stock volatility of government contractors rises by around 1% as the level of shock increases by one standard deviation after firms significantly boost the CEO *Vega*.

Finally, we demonstrate that increasing *Vega* has a positive effect on sales growth and total factor productivity (TFP) for government contractors following census shocks. More specifically, as reported in the last two columns, we find that a one standard deviation change in the census shock variable increases corporate sales growth and TFP by 4% and 3%, which corresponds to 18% and 17% of their sample mean values, respectively.

Beyond highlighting the important role of CEO risk incentives in shaping corporate outcomes, these results are important because they help reconcile the mixed evidence on the relationship between corporate investment and government spending (Cohen et al., 2011; Snyder and Welch, 2017; Hebous and Zimmermann, 2021; Kim and Nguyen, 2020; Ngo and Stanfield, 2022) by highlighting the role of CEO risk-taking incentives. In this sense, our results suggest that incentivizing managers through changes in their compensation packages can lead to a greater fiscal multiplier, thereby enhancing the effectiveness of fiscal policies to stimulate economic growth. In addition, these findings are significant because, while many papers support our conclusions (Low, 2009; Gormley et al., 2013), other studies conclude that CEO compensation incentivizes managers to take excessive, value-destroying risks (John and John, 1993; Berger et al., 1997; Dong et al., 2010; Kuang and Qin, 2013).

[10]As discussed by Even-Tov et al. (2022), unlike the cross-sectional results, we refrain from dividing the sample based on the *Vega* changes being above or below the median and then separately analyzing each of the two sub-sample. This is because government contracts, as previously documented, tend to systematically increase *Vega* in response to census shocks. Consequently, any sub-sample categorized by an increase (or decrease) in *Vega* would disproportionately contain a larger (or smaller) portion of firms highly susceptible to positive census shocks and more reliant on government contracts. This disparity undermines the statistical robustness of a sub-sample analysis.

Table 5: Corporate policies and performance

Dependent variables ($t+1$):	DPS (1)	CAPEX (2)	AQC (3)	R&D (4)	Volatility (5)	ΔSales (6)	TFP (7)
GovCustomer × Census shock × ΔVega > P50	-0.077**	0.120***	0.354*	0.014	0.289*	1.262*	0.993**
	(0.034)	(0.045)	(0.199)	(0.015)	(0.168)	(0.701)	(0.406)
GovCustomer × Census shock	0.008	0.001	-0.060	0.004	-0.302	-0.860	-0.125
	(0.035)	(0.050)	(0.139)	(0.007)	(0.193)	(0.934)	(0.486)
GovCustomer × ΔVega > P50	0.000	0.001	-0.007	0.000**	0.001	-0.074*	-0.011
	(0.001)	(0.001)	(0.007)	(0.000)	(0.006)	(0.042)	(0.020)
Census shock × ΔVega > P50	0.027	-0.029*	-0.120*	-0.001	-0.289***	-0.411	0.074
	(0.018)	(0.015)	(0.064)	(0.002)	(0.060)	(0.286)	(0.152)
GovCustomer	-0.000	-0.001	-0.000	-0.000	0.004	0.034	-0.008
	(0.002)	(0.002)	(0.008)	(0.000)	(0.009)	(0.042)	(0.030)
Census shock	-0.001	-0.010	0.121*	0.002	0.194**	-0.306	0.070
	(0.025)	(0.020)	(0.062)	(0.006)	(0.091)	(0.336)	(0.264)
ΔVega > P50	-0.001	0.000	0.005**	-0.000	-0.013***	0.063**	0.005
	(0.001)	(0.000)	(0.002)	(0.000)	(0.002)	(0.028)	(0.004)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,229	25,811	23,963	27,802	25,748	23,672	17,989
Adj. R^2	0.167	0.718	0.061	0.987	0.667	0.069	0.610

Notes: This table shows the effects of the Census shock on the future outcomes of government contractors when there is a high increase in CEO incentive compensation. *Census shock* is the county-level population estimating errors in census years, it is not scaled by 100 as the previous tests for the purpose of presenting the estimates. ΔVega > P50 takes the value of one if the change in *Vega* is higher than the sample median, zero otherwise. We use *DPS*, *CAPEX*, *AQC*, *R&D*, *Volatility*, *Sales growth*, and *TFP* denoting the dividend per share, capital investment, acquisition investment, R&D investment, stock return volatility, growth in sales revenue, and total factor productivity in $t+1$ as our outcome variables. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p ¡ 0.10, **p ¡ 0.05, ***p ¡ 0.01. See Table OA1 for a detailed description of the variables and their data sources.

7 Additional results and robustness analysis

In this section, we present additional results and robustness analysis. To conserve space, all tables from this section are reported in the Online Appendix.

7.1 Additional results

Do demand shocks affect Delta? It is theoretically possible to induce managers to make investments that increase firm value by managing the slope of the wealth-performance relationship. In our empirical analysis, we take account of this relationship by including as a control variable *Delta*.

To further test whether corporate boards incentivize managers by managing the slope of the wealth-performance relationship, we use *Delta* as an outcome variable and estimate again Equation (3). Interestingly, as reported in Table OA6, we do not find any evidence that census shocks affect the slope of the wealth-performance relationship.

A possible explanation for the lack of a statistically significant relationship is that, as argued by Gormley and Matsa (2016), stock awards incentives mainly operate by influencing CEO efforts, but do not directly affect his risk-taking behavior. This type of compensation could even exacerbate risk-related agency issues by directly linking managers' wealth to corporate losses. Since growth opportunities affect shareholders' preferences towards corporate risk-taking (Myers, 1977; Myers and Majluf, 1984; Shin and Stulz, 2000; Billett et al., 2007), and pursuing risky projects is particularly important for firms to adapt to and benefit from demand shocks (Ngo and Stanfield, 2022; Clemens and Rogers, 2023; Hvide and Meling, 2023), modifying the risk-taking incentives helps to prevent managers from taking on less risk than is desired by diversified shareholders or even undertaking value-destroying actions that reduce a firm's risk, something that it is not possible to achieve by managing the slope of the wealth-performance relationship. Moreover, as positive demand shocks increase convexity, this may partially substitute for linear incentives (Armstrong et al., 2013).

The effect of census shocks on expected revenues and opportunities. An important assumption of our identification strategy is that changes in government spending positively affect the expected revenues and growth opportunities of government contractors.

To provide evidence on this point, in line with the previous literature, we show that our census population shocks affect government spending and increase the dollar amount of government contracts issued in exposed counties. In this section, we provide further evidence that the exogenous increase in federal spending affects the growth opportunities of government contractors. To do so, we conduct a simple word count from the 10-K filings and test whether top managers are more likely to discuss government investment opportunities after census shocks.[11]

[11]The dictionary includes the following keywords and phrases: government contract, federal contract, public sector

We use the natural logarithm of one plus this variable as an outcome variable in Equation (3) and provide evidence in column (1) of Table OA7 that census shocks predict a rise in top management discussions about procurement opportunities of government contractors, further validating our identification strategy. More specifically, a one standard deviation positive change in *Census shock* triggers an 8.27% rise in the outcome variable.

Our results remain consistent when we use simple word counts instead of the logarithmic transformation of word counts and estimate the model using a Poisson regression. In addition, our results are consistent when we adjust word counts using the length of the text. As shown in columns (2) and (3), a one standard deviation increase in the level of the *Census shock* results in the use of 4 additional keywords, or a 1% increase in keyword counts relative to the total number of words used in the 10-K reports of government contractors.

Investment efficiency. Increased government spending, coupled with reduced volatility in such spending, significantly lowers demand uncertainty for government contractors. This stable demand by government customers is likely to lead to more predictable cash flows, enabling better alignment of resources with expected needs, minimizing waste or overcapacity, facilitating efficient asset utilization, and lowering operating costs. These factors are expected to enhance investment efficiency, increasing the benefits from risk-taking (e.g., Cohen and Li, 2020).

We test this hypothesis in our setting. Following prior research (Wurgler, 2000; Shroff et al., 2014; Bekaert et al., 2007), we measure investment efficiency by evaluating the responsiveness of investment to investment opportunities. Firm investment opportunity is measured by revenue growth (e.g., Chen et al., 2011; Biddle et al., 2009; Chay et al., 2023) and Tobin's q (e.g., Cohen and Li, 2020; Badertscher et al., 2013). More specifically, we estimate a linear regression model per industry-year as follows:

$$Investment_{i,t} = \beta Salesgrowth_{i,t-1} + \epsilon_{i,t} \tag{5}$$

where *Investment* is the sum of research and development expenditure, capital expenditure, and acquisition expenditure minus cash receipts from the sale of property, plant, and equipment scaled by lagged total assets. *Sales growth* and *Tobin's q* are used as alternative independent variables. Industry groups are classified by 4-digit NAICS from which a minimum of 10 observations is required for the estimation. As a result, we proxy investment efficiency as the absolute value of the residual for each observation, so that the higher the variable, the closer the firm investment to the optimal baseline (Chay et al., 2023).

contract, procurement contract, contract award, contracting opportunity, government procurement, public procurement, federal agency, government agency, public agency, Department of Defense, NASA, Department of Energy, Department of Health and Human Services, Department of Transportation, General Services Administration, GSA, procurement opportunity, contracting opportunity, government opportunity, solicitation, request for proposal, RFP, invitation to bid, ITB, request for quotation, RFQ, tender, grant, funding, public contract, federal funding, state contract, municipal contract, local government contract.

These proxies are used as outcome variables in estimating Equation 1, and the regression results are shown in Table OA8. The coefficients of the interaction term in columns (1) and (2) are significantly positive, suggesting that demand shocks promote more efficient firm investment. It remains robust for the sensitivity of firm investment regarding both revenue growth and Tobin's q. To interpret the economic significance, one standard deviation increase in *Census shock* triggers a 15% to 16% increase in investment efficiency for government contractors, depending on specifications.

Long-term forms of incentive compensation. Lewellen et al. (1987) argue that in firms with extensive investment opportunities, it is important that managers do not make myopic investment decisions. Thus, they predict that executives of growth firms will derive a relatively large proportion of their pay from long-term incentive compensation. Furthermore, extending the contract's duration after an increase in the convexity of managerial compensation can be an optimal strategy to mitigate agency issues. Indeed, this is consistent with the idea that boards may use longer vesting periods to counterbalance the potential negative effects of high vega incentives, such as excessive risk-taking (Kubick et al., 2017).

To investigate this point further, we consider the duration of executive compensation as an alternative outcome variable. More specifically, we follow Gopalan et al. (2014) and propose a measure of executive pay duration that reflects the vesting periods of different pay components (in number of years), thereby quantifying the extent to which compensation is short-term. In line with our hypothesis, Gopalan et al. (2014) show that this measure is greater in firms with more growth opportunities.

We use this variable as an outcome variable and re-estimate Equation (3). The results reported in Table OA9 provide evidence that census shocks positively affect the length of the pay duration. More specifically, in terms of magnitude, we find that one standard deviation rise in the shock variable increases the duration of executive compensation by one year.

7.2 Robustness analysis of our main specification

GLM and alternative measures of CEO risk incentives. In this paragraph, we employ an alternative estimator to account for potential biases from zero values occurring in our primary variable of interest, *Vega*. Furthermore, we also adopt alternative measures for this variable based on the current variation in option-based compensation.

Since roughly 18% of the observations of *Vega* assume a value equal to zero, we use a fixed-effects Poisson model to alter its distribution. Cohn et al. (2022) document that a generalized linear model (GLM) can accommodate outcomes with a value of zero and requires no assumptions about higher-order model error moments for consistent estimation. Furthermore, we also show that our results hold when we use the logarithm transformation of (1+ *Vega*) as a second approach. This transformation shrinks the distribution of this variable toward the left, ensuring that extreme values of the outcome variable do not drive our findings.

We also consider that *Vega* measures executive wealth change from the total option portfolio regarding incremental stock return volatility. Therefore, it may overlook the risk incentives captured by changes in the current remuneration package. To exclude this possible concern from our analysis, we consider as an outcome variable *Flow Vega*, that measures the vega of CEOs' current year option-based compensation regardless of the past accumulated ownership (Liu et al., 2021; Gormley et al., 2013). We then incorporate *# Option grants*, representing the number of options granted in the current year, scaled by the total shares outstanding.

We report detailed estimation results from Equation (3) for this additional robustness analysis in Table OA10. The positive and statistically significant estimates suggest that our results are robust to alternative measures of CEO risk-taking incentives. Specifically, one standard deviation increase in the government spending shock results in a 13% to 15% increase in *Vega*, representing an approximately $3,700 (10% of the sample mean) additional *Flow Vega*, or a 19% increase in options granted relative to the total stock outstanding in a given year.

Alternative variables to measure exposure to government spending. To further alleviate the concern that our main finding is sensitive to the proxy for a firm's dependence on government spending, three additional variables are explored.

We follow Ngo and Stanfield (2022) and use the share of sales to public agencies with respect to total revenue, *GovSales*. We also use a binary variable, *HighGovSales*, that equals one (zero otherwise) if *GovSales* exceeds the sample median. Furthermore, following Even-Tov et al. (2022), we construct an industry-level government exposure indicator, *HighGovInd* which takes a value of one when an industry generates above-median total sales revenue from government sales, based on the industry-by-industry Input-Output table recorded by the Bureau of Economic Analysis (BEA) in 2012.

We report the results in Table OA11. The coefficients of the interaction terms are positive and statistically significant for all the alternative exposure indicators. For a government contractor with an average *GovSales* (25.78%), a standard deviation increase in the *Census shock* results in an approximately $9,000 increase in CEO *Vega*. When interacting the treatment with *HighGovSales*, we observed a more substantial effect on *Vega* in terms of the magnitude of the effect related to a standard deviation rise in the *Census shock* (approximately $16,000). Similarly, when using *HighGovInd*, we still find a positive and statistically significant coefficient. However, the effect is smaller in terms of magnitude (approximately $8,000), probably because measurement error issues push the coefficient toward zero.

Alternative way of measuring the census shocks. Firms often operate in geographically dispersed areas. To account for this, we capture corporations' exposure to changes in government spending by aggregating census shocks at the state level, while considering the proportion of each firm's operations in different states. Specifically, using information from Garcia and Norli (2012), we identify the geographic distribution of each corporation's operations, as reported in the 10-K statements from 1994 to 2008. For each firm, we calculate the average value of census shocks across all states, weighted by the firm's level of exposure in each state. This measure is referred to as the *Census shock (weighted state)*.

We estimate Equation (3) using this new measure and report the results in Table OA12. Consistent with our main finding, an exogenous increase in government spending significantly raises the pay convexity of CEOs of government contractors. A one standard deviation increase in the *Census shock (weighted state)* leads to an approximate $20,000 increase in CEO wealth per 0.01 increase in stock return volatility. This effect is stronger than the one reported in our baseline specification, suggesting that measurement errors related to corporate exposure to government spending shocks could cause us to underestimate the true impact of this demand change.

Propensity score matching. Government contractors are likely to differ from non-government contractors in several dimensions. To ensure the robustness of our results, we use propensity score matching to pair government contractors with non-government contractors based on observable firm characteristics (Rubin and Thomas, 1996; Dehejia and Wahba, 2002).

To estimate the propensity score, we use a logistic regression model, where the dependent variable is an indicator variable equal to one if the firm is a government contractor and zero otherwise. The explanatory variables include all variables specified in the baseline model. Subsequently, we match each observation with a control observation using one-to-one nearest neighbor matching with replacement (Stuart and Rubin, 2008).

The results are presented in Table OA13. In Panel A, we demonstrate that the matching has been conducted appropriately, as there are no differences in observable firm characteristics. In Panel B, we confirm the consistency of our results within this sample. Importantly, the coefficient's magnitude falls within one standard deviation of the coefficient reported in our baseline specification.

Additional controls on county characteristics. Even after showing that census shocks are not correlated with county economic characteristics, we further employ time-series county-level characteristics as control variables in Equation (3). The county characteristics are collected from the U.S. Bureau of Economic Analysis (BEA). Specifically, *Population* is the logarithm of the county population, *Household income* is the logarithm of county per capita income, and *% Unemployment* is the ratio of non-employed labor to the total county population.

The results are shown in column (1) of Table OA14. We provide evidence that our main finding remains robust after considering local economic conditions. In addition, in column (2), we report the estimates of Equation (3) when we replace state-year fixed effects with county-year fixed effects, and we continue to find consistent results.

Additional controls on CEO characteristics. To further address the concern that the positive effect of the federal spending shock on *Vega* is driven by the heterogeneity of CEOs, we provide evidence that our baseline specifications hold when more CEO characteristics that potentially affect executive pay are considered. Following the argument that manager fixed effects account for most of the variation in remuneration (Graham et al., 2012), we estimate our model by adding CEO fixed effects. In an alternative exercise, we employ several CEO-level control variables commonly adopted by the executive compensation literature, such as CEO stock ownership, tenure, age, and gender. The results are shown in Table OA15. The evidence suggests that our baseline results remain robust when accounting for CEO heterogeneity.

Placebo test. One concern could be that the census shocks correlate with other factors that potentially lead to a change in managerial compensation. While we do not find any evidence that census shocks are correlated with county characteristics, as further evidence Figure OA4 shows the results of a placebo test on our baseline specification reported in column (3) of Table 2. For this test, we assign to the variable *Census shock* a random value 500 times, and we evaluate whether the coefficients of the interaction term are significantly different from the baseline estimates.

Panels (a) and (b) report the results for the random coefficients and t statistics separately, while the vertical dash lines are the estimates derived from the actual data. The evidence confirms that obtaining our observed estimated results from a random assignment of the treatment variable is statistically unlikely, rejecting the hypothesis that our finding is affected by unobservable treatment characteristics.

Clustering the standard errors along alternative dimensions. Our main specification for these panel regressions follows the standard approach of clustering standard errors at the firm level (Petersen, 2008; Wooldridge, 2010), which takes into account within-firm dependence in the residuals. Our baseline model is re-analyzed in this section by considering alternative clustering methods to account for other possible correlation structures in the error terms.

First, we employ standard errors clustered at the county level since this is where the treatment is assigned (Abadie et al., 2023; Bertrand et al., 2004). In addition, we cluster the standard errors at the county-industry level. Finally, following Thompson (2011), we use two-way clustering at both the firm-year and county-year levels to address both cross-sectional and time-series correlations in the error term. In Table OA16, we show that the coefficients are statistically significant when we cluster the standard errors using the alternative approaches discussed above.

8 Conclusions

A comprehensive understanding of CEO pay setting is a central question in the corporate finance literature and is also of great interest to the general public (Edmans et al., 2017). In this paper, we provide a first investigation into how demand shocks affect the structure of CEO compensation. To do so, we use government contractors and the allocation of government spending based on exogenous population shocks as a unique laboratory experiment.

Exploiting population count revisions in census years as exogenous shocks to the cross-sectional allocation of federal funds and using a triple difference-in-differences estimator, we demonstrate that demand shocks prompt corporate boards of government contractors to re-calibrate CEO compensation to enhance risk-taking incentives. This adjustment is driven by an increase in the use of options and does not impact other forms of compensation, resulting in a total compensation increase for CEOs. Our findings are heterogeneous depending on the volatility and predictability of the demand shocks, as well as depending on the price flexibility that the firms face. Moreover, the effect we identified is more pronounced in firms with good governance. We also find that, it is less pronounced as competition for expected revenues rises and when a CEO is more likely to exhibit risk-preference behavior. Finally, we demonstrate that using compensation incentives in this setting effectively increases corporate investments and improves corporate performance.

Our results are significant because they shed light on the complex relationship between demand shocks, CEO compensation, and corporate performance. At the same time, they illustrate from a novel perspective how government spending can affect corporate outcomes. Indeed, our findings demonstrate that government intervention in the product market and fiscal policies can influence CEO compensation and, in turn, managers' strategic behavior.

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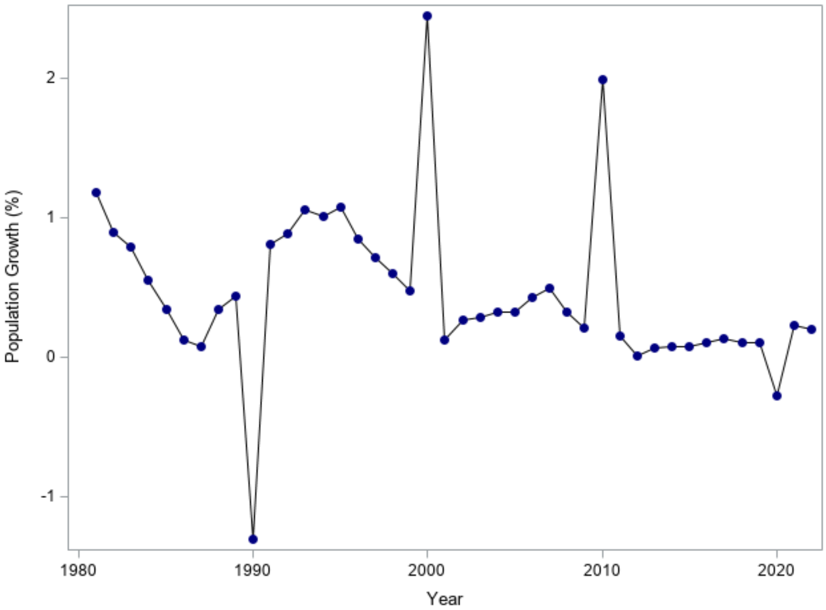
Online Appendix

This Appendix is for Online Publication (OA) and provides further details on the data and the paper's results "How Do Demand Shocks Affect CEO Compensation? Evidence from Government Contractors". More specifically, Online Appendix 1 (OA1) and Online Appendix 2 (OA2) report additional figures and tables, respectively.

OA1 Figures

Population growth rate in the counties. Figure OA1 shows the county-level population growth rate annual averages. It reports the average population growth rate from 1981 to 2022. The population growth rate in a non-census year is computed using post-censal population estimates published by the Census Bureau. Post-censal estimates are computed using the previous census count and population changes reflected in administrative data. The population growth rate in the 1990, 2000, 2010, and 2020 census is computed using the current census population count and the post-censal population estimates of the previous year.

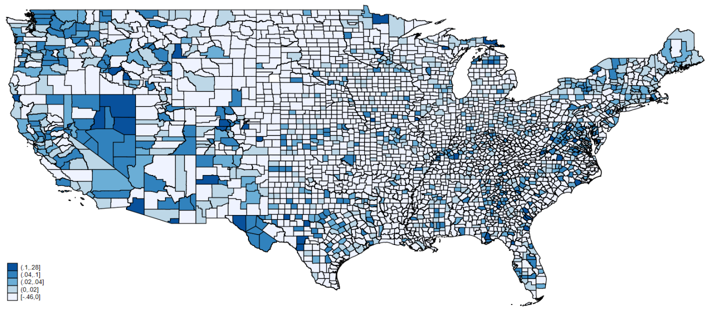
Figure OA1: Population growth rate in the counties



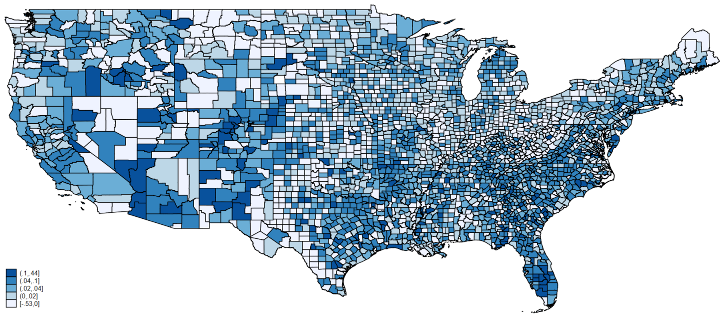
Notes: Figure OA1 shows the annual averages of county-level population growth rate. It reports the average population growth rate from 1981 to 2022. The population growth rate in a non-census year is computed using post-censal population estimates published by the Census Bureau. Post-censal estimates are computed using the previous census count and population changes reflected in administrative data. The population growth rate in the 1990, 2000, 2010, and 2020 census is computed using the current census population count and the post-censal population estimates of the previous year.

Geographical distribution of Census-Shock. Figure OA2 shows the geographical distribution of the Census-Shock for the Census years 1990, 2000, 2010, and 2020. Census-Shock is the logarithmic disparity between the population estimate after the census and the population count recorded during the Census itself.

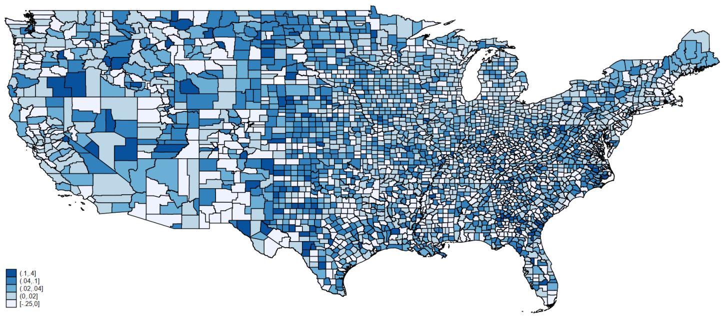
Figure OA2: Geographical distribution of Census-Shock



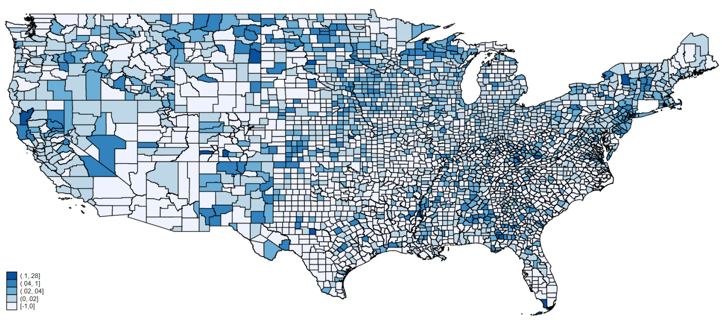
(a) Census-Shock 1990



(b) Census-Shock 2000



(c) Census-Shock 2010



(d) Census-Shock 2020

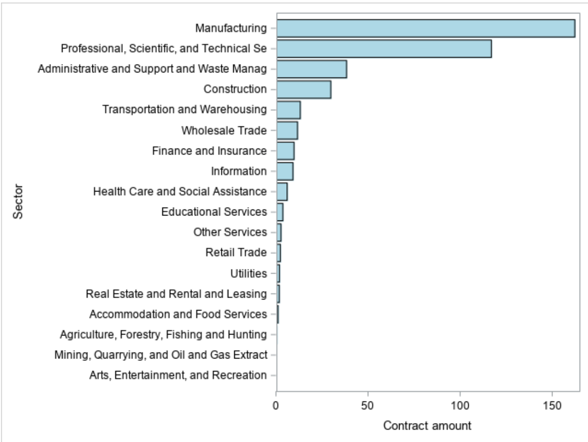
Notes: Figure OA2 show the geographical distribution of the Census-Shock for the Census years 1990, 2000, 2010, and 2020. Census-Shock is the logarithmic disparity between the population estimate after the census and the population count recorded during the Census itself.

Sector-level government procurement volatility, price stickiness, and contract duration. Figure OA3 illustrates significant differences in government procurement volatility and price stickiness across sectors. High procurement volatility is observed in sectors like "Professional, Scientific, and Technical Services" and "Construction," likely due to project-based contracts and fluctuating government demand. In contrast, sectors such as retail trade, agriculture, mining, and utilities show lower volatility, suggesting more consistent procurement needs.

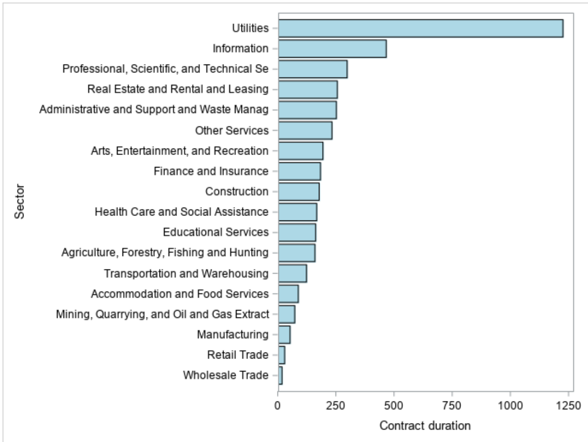
Price stickiness is most pronounced in sectors like agriculture, utilities, and mining, where prices adjust slowly, possibly due to long-term contracts or regulatory factors. Conversely, sectors like education and healthcare exhibit lower price stickiness, indicating more flexible pricing in response to changes in government demand.

The utilities sector has the longest average duration, reflecting the need for long-term commitments in infrastructure and service provision. Sectors like "Information" and "Professional, Scientific, and Technical Services" also show longer contracts, likely due to ongoing, complex projects. In contrast, sectors such as "Wholesale Trade," "Retail Trade," and "Manufacturing" have much shorter contract durations, suggesting a focus on more transactional or short-term procurement. This distribution indicates differing procurement strategies, with some sectors favoring sustained agreements while others opt for flexibility and rapid adjustments.

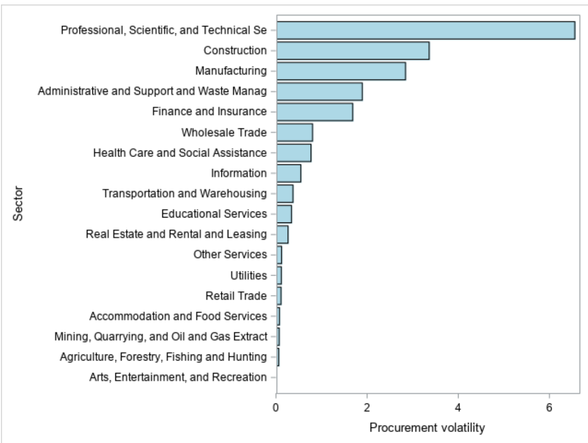
Figure OA3: Sector-level government procurement volatility and price stickiness



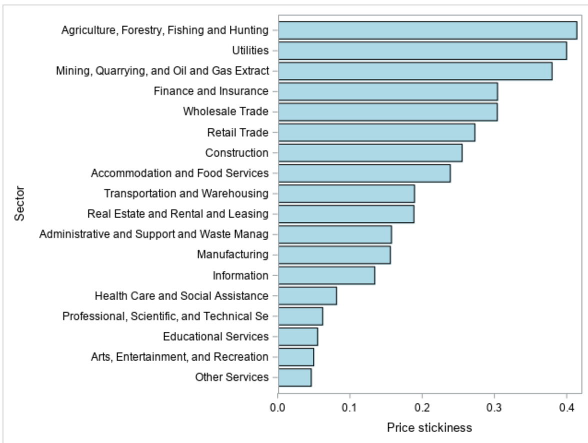
(a) Procurement amount ($ Billions)



(b) Contract duration (# Days)



(c) Procurement volatility

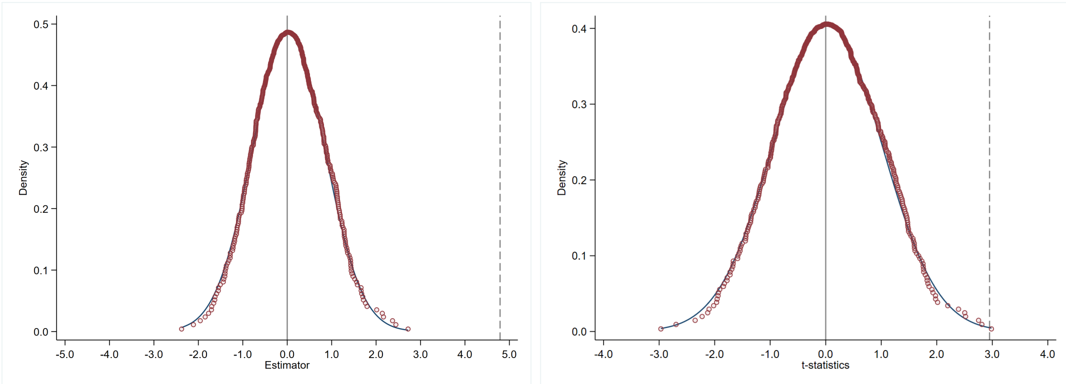


(d) Price stickiness

Notes: Figure OA3 shows the summary of government procurement features for each 2-digit NAICS sector, aggregating from the contract level dataset from *USAspending*. Panel (a) and panel (b) plot the yearly mean value of government procurement in billions of dollars and the average contract duration in days. Panel (c) and panel (d) present government procurement's yearly standard deviation and price stickiness.

Placebo test. Figure OA4 shows the results of the placebo test on our baseline specification reported in column (2) of Table 2. We assign a random value to the variable *Census shock* 500 times and test if the coefficients of the interaction term are significantly different from the baseline estimates.

Figure OA4: Placebo test



(a) Density of random estimators

(b) Density of t-statistics

Notes: Figure OA4 shows the results of the placebo test derived from 500 times random selections of the *Census shock* according to the baseline regression. Panel (a) shows the density of coefficient estimates of the 500 simulations. Panel (b) shows the frequency of t-statistics of the estimates. The vertical dash lines denote the coefficient and t-statistic estimated with actual data.

OA2 Tables

Variable definition. Table OA1 contains detailed information on the variables we use in the empirical analysis, their description, and their sources.

Table OA1: Variable description

Variable name	Description	Source
	Panel A: Firm-level dataset	
Vega	Dollar (in thousands) change in a CEO's option portfolio associated with 0.01 increase in the annualized standard deviation of the firm's stock daily returns (Coles et al., 2006).	ExecuComp
Flow Vega	Dollar (in thousands) change in a CEO's current option grants associated with 0.01 increase in the annualized standard deviation of the firm's stock daily returns.	ExecuComp
# Option grants	Number of option grants scaled by the firm's total number of shares outstanding.	ExecuComp
Total pay	The logarithm of one plus the CEO's total compensation (in thousands), consisting of salary, bonuses, restricted stocks granted, options granted, long-term incentive awards, and other types of compensation.	ExecuComp
% Stock	The proportion of total annual CEO compensation from stock grants.	ExecuComp
% Option	The proportion of total annual CEO compensation from option grants.	ExecuComp
% Salary	The proportion of total annual CEO compensation from salary.	ExecuComp
% Bonus	The proportion of total annual CEO compensation from bonus.	ExecuComp
% Other	The proportion of total annual CEO compensation from others.	ExecuComp
# Options exercised	The number of options exercised by the CEO in the current year multiplied by one thousand divided by shares outstanding.	ExecuComp
Options exercised	The value (in thousands) of options exercised by a CEO in the current year (divided by 100).	ExecuComp
Delta	Sensitivity of wealth from CEO's stock and option portfolio to firm performance.	ExecuComp
Census shock	The log difference between the post-censal population estimate and the concurrent census population count (times 100) for the county of headquarter during census years. Post-censal population estimates are computed using the previous census count and population changes reflected in administrative data.	census.gov; ICPSR
DBA	Yearly total discretionary budget authority (in trillion dollars)	whitehouse.gov
Senator shock	An indicator equals one, zero otherwise, if the firm's headquarters is located in the state whose senator is the incumbent chairperson of the Senate Finance Committee (Cohen et al., 2011)	Charles Stewart's website

Table OA1: Continued

Variable name	Description	Source
GovCustomer	An indicator equals one (and zero otherwise) if the firm discloses the federal government as a major customer.	Compustat-Segment
GovSales	The ratio of firm sales sold to the federal government as a fraction of total sales (for firms with the federal government as a major customer), and zero otherwise	Compustat-Segment
HighGovSales	An indicator equals one if GovSales is above the median (for firms with the federal government as a major customer), and zero otherwise	Compustat-Segment
HighGovInd	An indicator that equals one if the fraction of sales to the government of the industry reported by the 2012 BEA IO USE table is above the sample median, and zero otherwise.	bea.gov
Sales	Total sales during the fiscal year.	Compustat
Sales growth	The growth rate of total sales	Compustat
ROA	Operating income before depreciation divided by the book value of assets	Compustat
Leverage	Total current and long-term debt divided by the book value of assets	Compustat
ExCash	(Net Cash Flow from Operating Activities – Depreciation Amortization + R&D Expense) / Lagged Book Value of Assets.	Compustat
Stock Return	Stock return of last 12 months of fiscal period	CRSP
R&D	Research and development (R&D) intensity, equals R&D Expense / Lagged Book Value of Assets. Observations with missing information take the value of the industry average.	Compustat
Tobin q	(Total Assets - Book Value of Equity + Market Value of Equity) / Lagged Book Value of Assets.	Compustat
EMP	The growth rate of the number of employees	Compustat
CAPEX	(Capital Expenditures) / Lagged Book Value of Assets.	Compustat
Chairman-CEO	An indicator equals one, zero otherwise, if the CEO is also the chairperson of the board.	ExecuComp
Co-opted board	Co-opted directors and co-opted independent directors as a fraction of the total board (Coles et al., 2014).	RiskMetrics
CEO bargaining	The ratio of total CEO compensation to the highest compensation earned by a non-CEO executive in the firm (Bebchuk et al., 2011).	ExecuComp
Institutional ownership	The observation of the last quarter in a given year of the total value of all shares owned by institutions for each security for each quarter divided by Total Shares Outstanding at quarter end.	Thomson-Reuters 13F

Table OA1: Continued

Variable name	Description	Source
TFP	Firm level total factor productivity (İmrohoroğlu and Tüzel, 2014).	(İmrohoroğlu and Tüzel, 2014)
DPS	The dividend per share divided by the price per share.	Compustat
ACQ	The acquisition investment divided by the total asset.	Compustat
Procurement Volatility	The standard deviation of the amount of procurement contracts awarded to a given 2-digit NAICS code sector.	USAspending
Duration	The average number of years of procurement contracts awarded to a given 2-digit NAICS code sector.	USAspending
Price stickiness	The frequency of average procurement price for the 58 aggregated NAICS sectors.	USAspending
# Keywords	The number of keywords related to government procurement being discussed in firm 10-K statements.	SEC
% Keywords	The number of keywords related to government procurement scaled by the total number of words being discussed in firm 10-K statements.	SEC
	Panel B: County-level dataset	
Census shock	The log difference between the post-censal population estimate and the concurrent census population count for the county during census years. Post-censal population estimates are computed using the previous census count and population changes reflected in administrative data.	census.gov; ICPSR
Federal spending	The total procurement contract value being granted to recipients or performing in the county.	`www.USAspending.gov`
Population	The natural logarithm of county-level population.	`www.bea.gov`
Household income	The natural logarithm of county-level income per capita.	`www.bea.gov`
Unemployment	The county-level unemployment rate.	`www.bea.gov`
Demand volatility	The standard deviation of monthly government procurement amount in the county	`www.USAspending.gov`
Contractor HHI	The Herfindahl–Hirschman index (HHI) of contract values for government contractors in the county	`www.USAspending.gov`
% Competitive contracts	The proportion of competed contracts in the county	`www.USAspending.gov`
# Bidders	The average number of offers per competed contract weighted by contract value in the county	`www.USAspending.gov`

Notes: This table shows a detailed description of the variables and their data sources.

County-level characteristics. We report the summary statistics of our dataset at the county level in Table OA2. A detailed description of our variables and their sources is available in Table OA1.

Table OA2: Descriptive statistics: county-level

	(1) Count	(2) Mean	(3) SD	(4) p25	(5) p50	(6) p75
Census shock	91,638	0.01	0.05	-0.02	0.01	0.03
Population	91,638	10.25	1.45	9.31	10.14	11.08
Income per capita	91,638	10.28	0.39	9.99	10.28	10.55
Unemployment	91,638	0.49	0.16	0.41	0.50	0.59
ΔPopulation	88,584	0.00	0.02	-0.00	0.00	0.01
ΔIncome per capita	88,584	0.04	0.06	0.02	0.04	0.06
ΔUnemployment	88,583	-0.00	0.44	-0.02	-0.00	0.01
Federal spending	73,314	187.83	1630.68	0.04	1.12	13.97
Demand volatility	42,431	3.87	12.77	0.04	0.23	1.54
Contractor HHI	42,431	0.66	0.31	0.39	0.68	1.00
% Competitive contracts	42,431	0.79	0.23	0.66	0.86	1.00
# Bidders	42,431	5.97	17.94	2.00	3.00	5.00

Notes: This table shows descriptive statistics of the variables used in our county-level analyses. See Table OA1 for a detailed description of our variables and their sources.

Census shocks and other county characteristics. We collect information on local employment rates, local population, and income per capita. We next regress them on the census shocks. We report the results in Table OA3. Importantly, we do not find any evidence that local county characteristics can predict the census shocks.

Table OA3: County characteristics and census shocks

Sample:	Full county-years		Census year only
Dependent variable: Census shock (y^C)	(1)	(2)	(3)
Population	-0.001		
	(0.001)		
Household income	-0.003		
	(0.006)		
Unemployment	-0.001		
	(0.007)		
ΔPopulation		0.031	-0.045
		(0.038)	(0.076)
ΔIncome per capita		0.008	0.009
		(0.005)	(0.019)
ΔUnemployment		0.000	-0.002
		(0.001)	(0.002)
State FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Observations	91,638	88,583	9,165
Adj. R^2	0.133	0.127	0.146

Notes: This table shows the relationship between the census shocks and county economic characteristics after controlling for state and year-fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the county level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Census shocks and government spending at the county level. The aggregated regression results from Equation (2) are shown in Table OA4.

Table OA4: The effect of Census shock on federal spending

Dependent variable: *log*(Federal Spending)	(1)	(2)	(3)
Census shock	3.020***	2.293***	2.421***
	(0.667)	(0.665)	(0.682)
ΔPopulation			-0.260
			(1.394)
ΔIncome per capita			-0.001
			(0.296)
ΔUnemployment			-0.002
			(0.024)
County FEs	Yes	Yes	Yes
Year FEs	Yes	No	No
State-Year FEs	No	Yes	Yes
Observations	73,341	73,341	73,289
Adj. R^2	0.729	0.739	0.738

Notes: This table shows regression results for Equation (2). We use the logarithm of *Federal Spending* as our outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the county level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Census shocks and stability of local procurement. The aggregated regression results from Equation (2) are shown in Table OA5, using proxies of government procurement stability as outcome variables. [Is the heading COntractor IIIII a typo? I think that it should be HII]

Table OA5: The effect of Census shock on procurement stability

Dependent variable: [Should this be Competitors? or Competitive Bids?]	Demand volatility		Contractor HHI		# Bidders		% Competitive	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Census shock	-4.303***	-4.267***	0.239**	0.251***	-8.101***	-8.358***	-0.168***	-0.193***
	(0.877)	(0.888)	(0.093)	(0.096)	(3.110)	(3.205)	(0.059)	(0.061)
ΔPopulation		6.118***		-0.105		10.253		-0.067
		(2.233)		(0.211)		(8.713)		(0.107)
ΔIncome per capita		-1.041***		-0.026		-3.152*		0.014
		(0.274)		(0.047)		(1.703)		(0.027)
ΔUnemployment		0.118*		0.004		0.101		0.002
		(0.069)		(0.003)		(0.201)		(0.002)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	73,341	73,289	43,318	42,431	43,565	42,356	43,633	42,814
Adj. R^2	0.619	0.612	0.683	0.683	0.174	0.176	0.390	0.389

Notes: This table shows regression results for Equation (2). We use measures of demand stability as outcome variables. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the county level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

What happens to Delta? We use *Delta*, which measures the CEO's pay sensitivity to stock performance, as an outcome variable and estimate Equation (3). We report the results in Table OA6.

Table OA6: Census population shocks and CEO Delta of government contractors

Dependent variable: Delta	(1)	(2)
GovCustomer × Census shock	-18.940	-17.576
	(12.472)	(12.663)
GovCustomer	10.304	19.811
	(62.028)	(65.474)
Census shock	2.606	2.249
	(7.306)	(7.399)
Sales		160.727***
		(50.814)
Leverage		-335.662***
		(124.345)
ROA		944.602***
		(258.973)
Sales growth		0.293
		(0.375)
ExCash		356.143**
		(179.253)
Stock Return		168.597***
		(34.470)
Firm FEs	Yes	Yes
Industry-Year FEs	Yes	Yes
State-Year FEs	Yes	Yes
Observations	35,731	35,731
Adj. R^2	0.560	0.573

Notes: This table shows regression results from Equation (3). We use *Delta* as our outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

A textual analysis of the 10-K. We consider a textual analysis of the 10-K to test whether top managers are more likely to discuss government investment opportunities after census shocks. We count the number of total words related to government contracting and use the natural logarithm of this variable plus one as the outcome in Equation (3). Table OA7 shows that census shocks predict top management discussion about government investment opportunities for government contractors, further validating our identification strategy. Our results remain consistent when we use the simple word counts instead of the logarithm transformation of word counts and estimate the model using Poisson regression, as well as when we consider the word count adjusted by the length of the text.

Table OA7: A textual analysis of the 10-K

Dependent variable:	# Keywords (1)	# Keywords (2)	% Keywords (3)
GovCustomer × Census shock	0.027***	1.333***	0.003*
	(0.007)	(0.368)	(0.002)
GovCustomer	0.049	0.677	0.006
	(0.042)	(1.605)	(0.012)
Census shock	-0.001	-0.077	-0.001
	(0.003)	(0.138)	(0.001)
Controls	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Industry-Year FEs	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes
Observations	32,482	32,482	32,482
(Pseudo) Adj. R^2	0.452	0.367	0.314

Notes: This table shows regression results from Equation (3). We use alternative outcome variables as reported in the first raw. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Investment efficiency. We consider alternative measures of investment efficiency as outcome variables in estimating Equation (3). The regression results are shown in Table OA8. The coefficients of the interaction term in columns (1) and (2) are significantly positive, suggesting that demand shocks promote more efficient firm investment. It remains robust for the sensitivity of firm investment regarding both revenue growth and Tobin's q. To interpret the economic significance, one standard deviation increase in *Census shock* triggers a 15% to 16% increase in investment efficiency for government contractors, depending on specifications.

Table OA8: Investment efficiency

Dependent variable:	Investment efficiency	
	sales growth	*Tobin's q*
	(1)	(2)
GovCustomer × Census shock	0.035**	0.062***
	(0.016)	(0.020)
GovCustomer	-0.107	-0.244*
	(0.096)	(0.138)
Census shock	-0.007	-0.018
	(0.007)	(0.012)
Controls	Yes	Yes
Firm FEs	Yes	Yes
Industry-Year FEs	Yes	Yes
State-Year FEs	Yes	Yes
Observations	35,119	35,068
Adj. R^2	0.678	0.702

Notes: This table shows regression results from Equation (3). We use alternative outcome variables as reported in the first raw. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Long-term forms of incentive compensation. We consider as an additional outcome variable the duration of executive compensation. To do so, we follow Gopalan et al. (2014) and propose a measure of executive pay duration that reflects the vesting periods of different pay components (in number of years), thereby quantifying the extent to which compensation is short-term. We use this variable as an outcome variable and report the results in Table OA9.

Table OA9: Long-term forms of incentive compensation

Dependent variable:	CEO pay duration (1)
GovCustomer × Census shock	0.030**
	(0.148)
GovCustomer	-0.026
	(0.099)
Census shock	-0.006
	(0.005)
Controls in Table 2	Yes
Firm FEs	Yes
Industry-Year FEs	Yes
State-Year FEs	Yes
Observations	15,374
Adj. R^2	0.464

Notes: This table shows regression results for Equation (3). We use *CEO pay duration* as our outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Table OA1 for a detailed description of the variables and their data sources.

Alternative measures of CEO risk incentives. In table OA10, we use a fixed-effects Poisson model to alter the distribution of Vega, and log of 1 plus Vega as the second approach to estimate Equation (3). We also estimate the *Flow Vega* that captures vega of CEOs' current year compensation regardless of the past accumulated ownership (Liu et al., 2021), and the *# Option grants* which refers to the current number of option grants scaled by the total number of shares outstanding. We report the results in Table OA10

Table OA10: Alternative measures of CEO risk incentives

	Poisson Vega (1)	ln(1 + Vega) (2)	Flow Vega (3)	# Option grants (4)
GovCustomer × Census shock	0.043***	0.048**	1.215*	0.062*
	(0.015)	(0.020)	(0.689)	(0.037)
GovCustomer	-0.029	-0.114	2.360	0.095
	(0.077)	(0.081)	(2.915)	(0.221)
Census shock	-0.006	0.000	0.119	-0.020
	(0.007)	(0.009)	(0.282)	(0.019)
Controls in Table 2	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Industry-Year FEs	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
Observations	33,035	35,731	35,731	35,731
(Pseudo) Adj. R^2	0.768	0.642	0.452	0.198

Notes: This table shows the effect of the census shock from Equation (3) when we use a GLM estimator and other measures of CEO risk incentive compensation as an outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Alternative exposure variable. We propose an alternative measure of exposure to treatment in Equation (3). We use a continuous variable measured as the share of sales to public agencies on total revenue, *GovSales*, and a binary variable, *HighGovSales*, equals to one (zero otherwise) if *GovSales* exceeds the sample median. We report the results in the first two columns in Table OA11. In addition, we follow Even-Tov et al. (2022) and construct an industry-level government exposure, *HighGovInd*. Specifically, this variable takes the value of one when an industry generates an above-median sales revenue from the government, according to the industry-by-industry Input-Output table recorded by the Bureau of Economic Analysis (BEA) in 2012. We report the results in the second column in Table OA11.

Table OA11: Alternative Definition of exposure

Dependent variable: Vega	(1)	(2)	(3)
GovSales × Census shock	0.108**		
	(0.044)		
HighGovSales × Census shock		5.089***	
		(1.963)	
HighGovInd × Census shock			3.093*
			(1.668)
GovSales	-0.822**		
	(0.369)		
HighGovSales		-12.810	
		(9.078)	
Census shock	-1.433	-0.539	-1.804
	(0.918)	(0.954)	(1.375)
Controls in Table 2	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Industry-Year FEs	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes
Observations	35,731	35,731	32,402
Adj. R^2	0.578	0.579	0.566

Notes: This table shows the effect of the census shock from Equation (3) when we use alternative measures to identify firms' exposure to government spending. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Alternative way of measuring exposure to census shocks. Firms may operate in geographically dispersed areas. Therefore, to better capture exposure to census shocks, we aggregate them at the state level and match them to firms based on the proportion of their operations in different states, following Garcia and Norli (2012). We report the results in Table OA12.

Table OA12: Alternative mapping firms to regions

Dependent variable: Vega	(1)	(2)
GovCustomer × Census shock (weighted state)	14.688**	15.118***
	(6.097)	(5.718)
GovCustomer	-25.674**	-30.469***
	(12.264)	(11.306)
Census shock (weighted state)	-1.316	2.636
	(5.398)	(4.904)
Controls in Table 2	No	Yes
Firm FEs	Yes	Yes
Industry-Year FEs	Yes	Yes
State-Year FEs	Yes	Yes
Observations	21,288	21,288
Adj. R^2	0.636	0.667

Notes: This table shows the results from Equation (3) when we use an alternative way to map firms to regions. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Propensity score matching. We use a propensity score matching to pair government contractors with non-government contractors based on observable firm characteristics. To estimate the propensity score, we use a logistic regression model, where the dependent variable is a dummy variable equal to one if the entity is a government contractor and zero otherwise. The independent variables include all variables specified in the baseline model. Subsequently, we match each observation with a control observation with the same industry and year using one-to-one nearest neighbor matching with replacement. The results are presented in Table OA13. In Panel A, we demonstrate that the matching has been conducted appropriately, as there are no significant differences in observable firm characteristics. In Panel B, we confirm the consistency of our results within this sample when we estimate Equation (3).

Table OA13: Propensity score matching

Panel A: Mean differences

	GovCustomer= 1	*GovCustomer*= 0	Difference	
Variables	Mean ($N = 2,693$)	Mean ($N = 2,693$)	Estimate	p-value
Sales	7.241	7.196	0.045	0.284
Leverage	0.235	0.234	0.000	0.905
ROA	0.120	0.119	0.001	0.545
Delta	385.639	382.160	3.479	0.882
Sales growth	0.160	0.167	-0.007	0.801
ExCash	0.072	0.072	0.000	0.791
Stock Return	0.171	0.172	-0.001	0.956

Panel B: Regressions

Dependent variable: Vega	(1)	(2)
GovCustomer × Census shock	6.476*	8.687**
	(3.374)	(4.219)
GovCustomer	-4.171	-1.903
	(14.553)	(12.760)
Census shock	-2.246	-2.945
	(4.691)	(4.614)
Controls in Table 2	No	Yes
Firm FEs	Yes	Yes
Industry-Year FEs	Yes	Yes
State-Year FEs	Yes	Yes
Observations	5,386	5,386
Adj. R^2	0.641	0.668

Notes: This table shows our propensity score matching results. We used it to pair government and non-government contractors based on observable firm characteristics. In Panel A, we demonstrate that the matching has been conducted appropriately, as there are no differences in observable firm characteristics. In Panel B, we present estimation results of Equation (3) using the matched sample. We use *Vega* as our outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Additional controls on county characteristics. To consider the time-varying county-level heterogeneity, we employ county-year fixed effects and time-series county-level characteristics as control variables in Equation (3). The results are shown in Table OA14.

Table OA14: Additional controls on county characteristics

Dependent variable: Vega	(2)	(1)
GovCustomer × Census shock	4.303**	4.299**
	(1.707)	(1.936)
GovCustomer	-9.346	-10.630
	(7.646)	(9.302)
Census shock	-2.027**	
	(0.959)	
Population	6.173	
	(4.850)	
Income per capita	15.151	
	(26.520)	
Unemployment	38.816	
	(30.875)	
Controls in Table 2	Yes	Yes
Firm FEs	Yes	Yes
Industry-Year FEs	Yes	Yes
State-Year FEs	Yes	No
County-Year FEs	No	Yes
Observations	35,009	31,531
Adj. R^2	0.583	0.596

Notes: This table presents the results from Equation (3) when we add time-varying county-level control variables. Columns (1) and (2) report the estimates of regressions that use our continuous and binary measures of corporate government dependence. *Population* is the logarithm of county population, *Household income* is the logarithm of county per capita income, and *% Unemployment* is the ratio of non-employed labors. The county characteristics are collected from the U.S. Bureau of Economic Analysis (BEA). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Additional controls on CEO characteristics. To further address the concern that the positive effect of the federal spending shock on CEO *Vega* is driven by the heterogeneity of CEOs, we provide evidence that our baseline specifications hold when more CEO characteristics that potentially vary executive pay are considered. Following the argument that manager fixed effects explain a majority of the variation in remuneration (Graham et al., 2012), we estimate our model by adding CEO fixed effects. Furthermore, we employ several CEO-level control variables commonly adopted by the executive compensation literature. They include CEO stock ownership, tenure, age, and gender.

Table OA15: Additional controls on CEO characteristics

Dependent variable: Vega	(1)	(2)
GovCustomer × Census shock	4.242**	5.216***
	(1.685)	(2.030)
GovCustomer	-2.390	-10.961
	(6.587)	(10.209)
Census shock	-1.269	-1.033
	(0.999)	(1.168)
CEO ownership		-0.300***
		(0.062)
CEO tenure		1.118**
		(0.437)
CEO Age		0.109
		(0.375)
Female		-16.388
		(16.347)
Controls in Table 2	Yes	Yes
Firm FEs	Yes	Yes
Industry-Year FEs	Yes	Yes
State-Year FEs	Yes	Yes
CEO FEs	Yes	No
Observations	34,901	23,563
Adj. R^2	0.713	0.617

Notes: This table shows the results from Equation (3) when we consider CEO characteristics. Columns (1) include CEO fixed effects while (2) add CEO features as additional control variables. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Alternative clustering. We show that our results hold when we cluster standard errors at the county level and county by industry level and when standard errors are two-way clustered at county and year and firm and year level. Table OA16 reports these estimation results.

Table OA16: Alternative clustering

SE clustered at: Dependent variable: Vega	County (1)	County-industry (2)	Firm & year (3)	County & year (4)
GovCustomer × Census shock	5.000***	5.000***	5.000***	5.000***
	(1.629)	(1.757)	(1.757)	(1.669)
GovCustomer	-7.981	-7.981	-7.981	-7.981
	(7.044)	(8.007)	(7.749)	(6.954)
Census shock	-0.607	-0.607	-0.607	-0.607
	(1.074)	(0.973)	(0.922)	(1.025)
Controls in Table 2	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Industry-Year FEs	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes
County-Year FEs	No	No	No	No
Observations	35,731	35,731	35,731	35,731
Adj. R^2	0.580	0.580	0.579	0.579

Notes: This table shows regression results from Equation (3). We use *Vega* as our outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at alternative levels, as reported in the first raw. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.