

Insider Trading Reforms and Corporate Transparency: Evidence from the STOCK Act

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Abstract

We examine how insider trading restrictions on government officials affect corporate transparency. The 2012 STOCK Act prohibited executive branch officials from profiting from non-public information, potentially limiting firms' access to policy insights used in forecasting. Using a difference-in-differences design, we find that firms with significant government contracts reduced the frequency and precision of management forecasts following the Act. These firms also experienced declines in price informativeness and increases in implied cost of capital, suggesting weakened capital market information environments. Effects are strongest among politically engaged firms and those heavily reliant on government business, suggesting that the response reflects a loss of privileged information rather than heightened uncertainty. Text-based evidence from earnings calls reveals fewer procurement-related discussions and a rise in policy risk language. Our results suggest that insider trading reforms, while enhancing accountability, may inadvertently reduce firms' access to discretionary information, weakening disclosure quality and impairing market transparency.

Keywords: STOCK Act, disclosure, management forecast, private communication

JEL Classification Codes: D82, G34, G38, M41

1. Introduction

Corporate voluntary disclosure reduces information asymmetry and enhances market efficiency. Firms adjust their disclosure strategies in response to regulatory constraints and information availability (Verrecchia, 1990; Lambert, Leuz, and Verrecchia, 2007). While insider trading laws primarily aim to prevent individuals with privileged access from profiting from non-public information, they may also shape corporate transparency by restricting the flow of privileged insights, potentially limiting firms' ability to issue reliable forward-looking guidance. This paper examines whether the Stop Trading on Congressional Knowledge (STOCK) Act, enacted in 2012, disrupted corporate disclosure by altering firms' access to regulatory and policy-related information from executive branch officials. More specifically, we ask (1) whether the Act changed the amount of guidance that managers issued and its contents and (2) whether any resulting shift altered the firm-specific information that reaches investors.

Concerns over government officials leveraging insider knowledge for financial gain were a key catalyst for the STOCK Act. These concerns focused on the risk that policymakers could influence legislation while holding financial stakes in affected firms. A widely cited case is the 2008 Visa IPO, during which House Speaker Nancy Pelosi and her husband acquired shares at the offering price while major credit card legislation, potentially harmful to Visa, was pending in Congress. As Speaker, Pelosi had influence over whether the bill advanced, and her access to Visa executives raised questions about whether she could also provide the firm with insights into legislative prospects. Although no wrongdoing was found, the optics of powerful lawmakers trading in firms affected by their policy decisions underscored the risk of privileged, two-way information flows. The controversy, brought to national attention by a 2011 *60 Minutes* investigation, helped catalyze bipartisan support for the STOCK Act's passage. The issue remains salient. On April 28, 2025, lawmakers introduced the Preventing Elected Leaders from Owning Securities and Investments (PELOSI) Act, a stricter proposal that would ban members of Congress and their families from owning individual stocks, reviving long-standing concerns about the entanglement of political power and private financial interests.

The STOCK Act extended corporate insider trading restrictions to over 28,000 executive, legislative, and judicial branch officials, prohibiting them from profiting from non-public information and requiring disclosure of stock trades.¹ Even today, government officials hold financial stakes in firms they regulate,

¹The STOCK Act applies to officials across more than 50 federal agencies and 15 executive departments, including the President, Vice President, executive officers and employees, appointed administrative law judges, and Members of Congress and their staff. See Section 6 of the S.2038 STOCK Act at <https://www.congress.gov/bill/112th-congress/senate-bill/2038>.

creating both access to non-public information and incentives for informal exchanges.² Although the Act does not explicitly prohibit private communication between firms and officials, it increases scrutiny and liability risks, likely discouraging discretionary exchanges. This shift may be particularly consequential for firms with significant government contracts, which rely on interactions with executive branch officials to anticipate procurement decisions, regulatory enforcement, and budgetary allocations. Prior research suggests that policymakers strategically divested from firms before negative contract announcements, suggesting privileged access to government decisions (Cherry, Heitz, and Jens, 2018). If the STOCK Act curtailed these interactions, firms may have faced greater uncertainty in forecasting contract renewals, policy shifts, and compliance expectations.

Because the STOCK Act barred officials from monetizing non-public information while leaving procurement rules unchanged, it offers a nationwide quasi-experiment on how the loss of a privileged information channel affects corporate disclosure and price informativeness. We find that, where information loss is plausibly greatest, management forecasts become less frequent and less precise. Furthermore, price informativeness weakens, and the implied cost of capital rises. In contrast to prior studies that emphasize the private benefits of political connections and access, our evidence reveals the capital market costs when those access channels are disrupted.

To empirically examine these effects, we employ a difference-in-differences framework comparing firms with significant government contracts to those without, both before and after the Act's enactment. Our primary measure of voluntary disclosure is the frequency of all management forecasts, which capture firms' forward-looking guidance and transparency. We identify firms with major government contracts as those reporting at least 10% of revenue from government contracts in at least three of the four years preceding the STOCK Act. Given their frequent interactions with government officials, these firms are more vulnerable to disruptions in information access.

To address our first research question, we use a difference-in-differences design that compares pre- and post-Act guidance frequency between firms with and without major government customers. We find that post-Act, firms with substantial government contracts experienced an 8.62% decline in management forecast frequency, equivalent to 0.31 fewer forecasts per firm per year. The pre-trend power analysis from Roth (2022) provides no evidence to reject the parallel trends assumption. This reduction in disclosure coincides with a shift from precise point forecasts to broader range forecasts, with the likelihood of issuing

²A 2022 *Wall Street Journal* investigation found that over 20% of officials across 50 federal agencies owned or traded stocks in companies lobbying their agencies. See <https://www.wsj.com/articles/six-takeaways-from-wsj-investigation-into-the-stock-trades-of-government-officials-11665491360?mod=djem10point>.

range forecasts increasing by 8.6% post-STOCK Act, indicating heightened uncertainty.

We further explore heterogeneity in disclosure responses based on firms' exposure to government-related uncertainty and political engagement. Firms with high revenue dependence on government contracts typically face lower business risk due to stable demand (Cohen and Li, 2020) but may be particularly vulnerable to shifts in the information environment. If the STOCK Act made it more difficult for senior managers to obtain timely or reliable insights from government officials, these firms may have faced greater challenges in forecasting performance, leading to reduced disclosure (Anantharaman and Zhang, 2011; Guay, Samuels, and Taylor, 2016). Consistent with this mechanism, we find that firms with stronger government ties, such as those heavily reliant on government sales, major government customers, or political connections reduce guidance more than their counterparts. This pullback suggests that firms facing greater exposure to shifting government priorities, contract renewals, or regulatory changes were disproportionately affected by the decline in informal information channels.

In addition to examining changes in management forecast behavior, we analyze how corporate discussions of government contracting and political risk evolved following the STOCK Act. Using earnings call transcripts, we track how frequently firms reference key government-related topics, providing insight into whether companies adjusted their communication strategies in response to reduced access to informal government insights. We find that firms with significant exposure to government contracts decreased their discussions of procurement-related topics, consistent with a decline in discretionary information flow. At the same time, discussions of regulatory and policy risk increased, indicating that firms perceived heightened uncertainty regarding government-related business conditions. These findings complement our primary results, providing additional evidence that the STOCK Act reshaped firms' information environments and disclosure strategies.

We then turn to our second research question, testing whether the post-Act pullback in guidance coincided with a measurable thinning of the firm's information environment. Post-Act, firms reliant on government contracts exhibited a 12.6% decline in equity price informativeness, suggesting that diminished disclosures weakened market participants' ability to incorporate firm-specific information into stock prices. We further show a decline in price informativeness using the generalized price informativeness (GPIN) from Duarte, Hu, and Young (2020) and price delay measures from Hou and Moskowitz (2005). At the same time, analyst forecast errors increased while forecast dispersion declined, suggesting that analysts may have struggled to accurately assess firm performance despite reaching more similar estimates. As price informativeness declined, these firms also experienced an increase in their implied cost of capital

(ICC), highlighting the financial implications of reduced transparency. Together, these results suggest that firms' dependence on government-sourced insights for strategic planning and investor communication was significantly disrupted by the Act.

Finally, we examine the effects of the STOCK Act on firms owned by members of Congress. Like executive branch officials, Congress members could profit from non-public information, particularly when they held stocks in firms affected by government procurement decisions. [Cherry, Heitz, and Jens \(2018\)](#) find that Senators strategically divested from firms before negative contract announcements, suggesting access to early procurement insights. While their study focuses on politicians' trading behavior, it highlights that privileged access to regulatory and procurement information was valuable. Our study extends this idea by examining the firm-level consequences of restricting such access. If policymakers' ability to trade on private information was curtailed, firms that previously benefited from these information channels may have also experienced disruptions. Consistent with this, we find that firms where Congress members held stock prior to the STOCK Act reduced the frequency of management forecasts after the Act's passage. However, after controlling for firms with the government as a major customer, this effect disappears. This finding suggests that while both Congress and executive branch officials lost access to profitable trading opportunities, the flow of sensitive information from executive branch officials played a more pivotal role in shaping corporate disclosure practices. This is likely due to their direct involvement in procurement, regulatory enforcement, and policy execution.

To better understand the underlying mechanism, we examine whether forecast reductions stem from diminished informal communication with government officials or heightened managerial uncertainty. Our evidence suggests both channels are relevant, but the stronger effects among firms with greater government revenue dependence and political engagement point to reduced information flow as the dominant driver. If general uncertainty were the primary cause, we would expect more uniform disclosure reductions across firms. Instead, the decline is concentrated among firms most reliant on government-sourced insights, suggesting that the STOCK Act disrupted a key informational advantage. However, the evidence also indicates a managerial response to increased uncertainty, since firms reduce guidance frequency and shift toward range forecasts, which is consistent with reduced confidence in precise estimates. The accompanying decline in price informativeness and increase in implied cost of capital further supports the notion that investors also perceive a loss of firm-specific information. Thus, while both channels are at play, the bulk of the evidence suggests that reduced access to government-sourced information is the primary mechanism, with uncertainty-driven disclosure adjustments playing a secondary role.

Our results are robust to a battery of tests. First, we employ propensity score matching and entropy balancing to ensure that observable firm characteristics do not bias our estimates, yielding consistent results. Second, a placebo test, randomly assigning treatment status 10,000 times, demonstrates that our observed effect is unlikely due to chance. Third, we test alternative event dates and time windows, confirming that our findings are not an artifact of time measurement. Finally, we address sample composition concerns by excluding firms that never issued management forecasts. Collectively, these tests reinforce our conclusion that the STOCK Act significantly altered firms' voluntary disclosure practices by limiting private communication with executive branch officials.

Our study contributes to three strands of the literature. First, we contribute to the ongoing debate regarding the relationship between uncertainty and firms' voluntary disclosure practices. On one hand, the absence of precise information is often associated with reduced disclosure frequency ([Anantharaman and Zhang, 2011](#); [Guay, Samuels, and Taylor, 2016](#)). On the other hand, heightened uncertainty may increase investor demand for information, prompting management to provide more voluntary disclosures ([Verrecchia, 1990](#); [Balakrishnan et al., 2014](#); [Nagar, Schoenfeld, and Wellman, 2019](#)). [Balakrishnan et al. \(2014\)](#), in particular, demonstrate that firms respond strategically to exogenous reductions in public information by enhancing disclosures to mitigate information asymmetry and improve liquidity. By focusing specifically on the flow of government-sourced information through private communication with executive branch officers, our findings reveal a distinct outcome: restricted access to critical government insights disrupts managers' forecasting abilities without significantly elevating investor uncertainty enough to compel increased voluntary disclosures. Thus, we extend the insights from [Balakrishnan et al. \(2014\)](#) by highlighting the distinct role government-sourced information plays in shaping corporate disclosure decisions and illustrating how regulatory shocks like the STOCK Act can alter transparency by disrupting informal information channels beyond the scope of traditional disclosure regulations.

Second, we redirect the political-connections literature from what firms gain to what capital markets lose when privileged access is curtailed. Prior work shows that connections yield regulatory forbearance, preferential contracting, and cheaper financing (e.g., [Akey, 2015](#); [Cooper, Gulen, and Ovtchinnikov, 2010](#); [Wellman, 2017](#); [Ovtchinnikov, Reza, and Wu, 2020](#); [Heitz, Wang, and Wang, 2023](#)), and that federal contracts themselves influence corporate policy choices ([Cohen and Li, 2020](#); [Cohen et al., 2022](#); [Samuels, 2021](#)). [Christensen et al. \(2023\)](#) extend the dialogue by linking connections to more frequent policy-related disclosures. Unlike prior work, which ties connections to greater disclosure, we ask whether information flow depends on officials' incentives to share it in the first place. The STOCK Act's blanket ban

on trading profits sharply weakens those incentives, giving us our difference-in-differences experiment. We show that firms most exposed to procurement-based executive contacts respond by issuing fewer and less precise forecasts, documenting that political access underpins not just economic rents but also the supply of decision-useful information. By isolating executive-branch channels from broader legislative ties, our study uncovers an overlooked transparency cost of tightening ethical constraints on government-firm interactions. We center on executive-branch officials, key conduits of regulatory and procurement insight, and show that the STOCK Act's disruption of these channels heightened uncertainty and prompted firms to scale back voluntary disclosure. This evidence demonstrates that political connections influence corporate transparency, not just financial rents, and emphasizes the broader ramifications of restricting procurement-based executive access relative to broader legislative ties.

Finally, our study advances the literature on financial regulation by uncovering the unintended consequences of insider trading laws on corporate information environments. While prior research has focused on the STOCK Act's impact on government officials, particularly members of Congress ([Cherry, Heitz, and Jens, 2018](#); [Belmont et al., 2022](#); [Huang and Xuan, 2023](#); [Wei and Zhou, 2023](#)), we shift the focus to its firm-level effects, demonstrating how restrictions on information flow reshape corporate disclosure practices. Our findings reveal that while the STOCK Act strengthens regulatory oversight and mitigates insider trading risks, it also curtails firms' access to valuable policy insights, leading to less precise and less frequent voluntary disclosures. This unintended consequence aligns with concerns raised by [Nagy and Painter \(2012\)](#), who cautioned that well-intended transparency regulations can inadvertently constrain the information firms use for strategic decision-making. More broadly, while prior research finds that the introduction and enforcement of insider trading laws reduces the cost of capital by improving investor protection ([Bhattacharya and Daouk, 2002](#)), we provide new evidence of a countervailing effect. When such reforms curtail informal, firm-relevant information flows, they may impair price discovery and raise financing costs. By documenting how the STOCK Act alters firms' ability to issue forward-looking guidance, we highlight a previously overlooked tradeoff between regulatory transparency and corporate information quality. These findings have important implications for policymakers, suggesting the need to weigh transparency reforms against their potential to disrupt firm-investor communication.

2. Background and Hypothesis Development

2.1. The STOCK Act Background

The Stop Trading on Congressional Knowledge (STOCK) Act was signed into law on April 4, 2012, in a swift legislative response to public outrage. The catalyst was a *60 Minutes* investigation that aired on November 13, 2011, exposing how high-ranking lawmakers attended confidential meetings during the 2008 financial crisis and subsequently traded on the sensitive information they received. The revelations prompted bipartisan concern and accelerated momentum toward legislative reform.

At the time the episode aired, the STOCK Act had only eight co-sponsors, reflecting its prior lack of legislative traction. Yet within two weeks, support surged, with 112 lawmakers signing on by the end of November. By the time the bill was formally voted on less than five months later on April 4, 2012, over 65% of House members were listed as co-sponsors.³ This rapid legislative shift is particularly striking given that three previous versions of the bill had languished in committee, emphasizing the abrupt and exogenous nature of the Act's passage.

The Act mandated timely disclosure of stock transactions for key government figures—including members of Congress, the President, judges, and executive branch officials—requiring reports within 30 to 45 days, replacing the prior annual disclosure requirement. While designed to curb insider trading, its effectiveness remains debated. [Cherry, Heitz, and Jens \(2018\)](#) found that prior to the law, members of Congress earned abnormal returns by timing sales based on non-public information. These returns diminished post-enactment, suggesting some success in reducing trading abuses, though oversight gaps remain, particularly within the executive branch.

Notably, the STOCK Act did not prohibit private communication between government officials and firms, a key issue in government procurement, where agencies frequently interact with corporate managers. Executive branch officials possess proprietary data on contractors and economic policies. While government procurement interactions comply with the Federal Acquisition Regulation (FAR), the STOCK Act may have altered the incentives of government officials by removing opportunities for personal financial gain from privileged information. This shift could have reduced the willingness of officials to engage

²Prior to the STOCK Act, insider trading restrictions largely applied to corporate executives and directors. The Sarbanes-Oxley Act of 2002 (SOX) expanded insider trading enforcement but was part of a broad governance and financial reporting overhaul, making it difficult to isolate insider trading-specific effects. In contrast, the STOCK Act targeted government officials and provides a cleaner setting to examine how restrictions on insider access affect firms' voluntary disclosure behavior.

³A detailed history of the STOCK Act and *60 Minutes* episode titled 'Insiders', which aired on November 13, 2011, can be found in [Cherry, Heitz, and Jens \(2018\)](#).

in discretionary information-sharing, even in cases where communication remained legally permissible under FAR.⁴ Despite these restrictions, [Goldman \(2019\)](#) and [Cohen et al. \(2022\)](#) suggest that private communications between officials and major suppliers persist, indicating that regulatory constraints alone do not eliminate these interactions. Given that government contracting accounts for nearly 20% of U.S. GDP ([Mills, Nutter, and Schwab, 2013](#)), firms reliant on government business have strong incentives to maintain close coordination with officials ([Samuels, 2021](#)), potentially facilitating the exchange of sensitive information.

The Act's disclosure requirements apply to approximately 28,000 executive branch officials. Initially, all disclosures were supposed to be posted online, but an amendment passed in 2013 (S.716) revoked online reporting approximately 70 executive branch officials, thereby, limiting transparency.⁵ However, while these officials were no longer required to publicly disclose their financial transactions online, they were still obligated to file financial disclosure reports, which could be accessed through Freedom of Information Act (FOIA) requests or agency-specific procedures.⁶ Despite these limitations, studies suggest that the law affected political information flows. [Cherry, Heitz, and Jens \(2018\)](#) found that U.S. Senators reduced opportunistic trades after the STOCK Act, while [Huang and Xuan \(2023\)](#) documented declines in abnormal returns tied to mergers, acquisitions, and earnings surprises among Congress members. Furthermore, [Yu \(2022\)](#) found that firms previously owned by politicians experienced significant losses in government procurement contracts and grants post-STOCK Act, reinforcing the idea that the legislation altered information-sharing dynamics.

Senator Kirsten Gillibrand introduced the STOCK Act 2.0 in 2023 to strengthen enforcement and address compliance failures. This initiative followed a *Business Insider* report revealing that 78 members of Congress had violated the original law's disclosure requirements.⁷ A key factor behind widespread non-compliance is the nominal fine per violation. Moreover, no member of Congress has ever faced prosecution under the STOCK Act since its enactment, raising concerns about its enforceability. While enforcement remains inconsistent, the 2025 introduction of the PELOSI Act signals continued regulatory efforts to address insider trading risks. Even in May 2025, President Donald Trump endorsed a congressional stock trading ban, reigniting debates about the ethical use of non-public information by government officials.

⁴The Federal Acquisition Regulation (FAR) is the primary set of rules governing procurement processes for U.S. federal agencies. It establishes standards for competitive bidding, contractor qualifications, and ethical conduct in government contracting. For the full regulation, see: <https://www.acquisition.gov/far>.

⁵See <https://www.congress.gov/bill/113th-congress/senate-bill/716> for details.

⁶For further details, see https://www.citizen.org/wp-content/uploads/2017_stock_act_report.pdf and <https://sgp.fas.org/crs/misc/R42495.pdf>.

⁷See <https://www.businessinsider.com/congress-stock-act-violations-senate-house-trading-2021-9>.

2.2. Hypothesis Development

Corporate voluntary disclosure reduces information asymmetry and enhances market efficiency (Verrecchia, 1990; Lambert, Leuz, and Verrecchia, 2007). Firms adjust disclosure based on private information availability, regulatory constraints, and investor expectations (Diamond and Verrecchia, 1991; Balakrishnan, Core, and Verdi, 2014; Nagar, Schoenfeld, and Wellman, 2019). Some research suggests firms increase disclosure under uncertainty (Verrecchia, 1983; Christensen et al., 2023), while others argue uncertainty discourages disclosure when managers lack confidence in their projections (Anantharaman and Zhang, 2011; Guay, Samuels, and Taylor, 2016).

The STOCK Act, by restricting executive branch officials from profiting from non-public information, likely disrupted regulatory and policy information flows to firms. Although the Act does not explicitly ban private communication, increased scrutiny over financial transactions may have discouraged officials from sharing discretionary insights. This shift may have limited firms' access to procurement expectations and regulatory guidance, critical factors for strategic planning, leading to our first null hypothesis:

H1 (null): *The STOCK Act has no significant effect on the frequency of management forecasts in firms where government contracts represent a large portion of business operations.*

We analyze two key channels through which these changing incentives may have influenced corporate disclosure: (1) demand uncertainty and (2) political engagement.

Demand Uncertainty Channel. The extent to which firms altered disclosure likely depended on their exposure to demand uncertainty. Government contracts generally provide revenue stability and reduce business volatility relative to private-sector clients (Cohen et al., 2022; Samuels, 2021), yet firms reliant on these contracts remain highly sensitive to procurement decisions and budget fluctuations. If the STOCK Act constrained access to informal regulatory insights, firms in volatile industries or those dependent on discretionary government contracts may have struggled with forecasting funding renewals, managing compliance risks, and adapting to competitive pressures. Cohen and Li (2020) show that firms with major government customers exhibit heightened investment sensitivity to procurement-related demand fluctuations, emphasizing the role of government-sourced information in decision-making. In such an environment, managers may have withheld guidance to avoid credibility risks stemming from uncertain forecasts (Skinner, 1997; Anantharaman and Zhang, 2011; Guay, Samuels, and Taylor, 2016).

H2 (null): *The STOCK Act has no significant effect on the frequency of voluntary disclosures for facing higher business uncertainty.*

Political Engagement Channel. Beyond procurement ties, firms leverage political connections to gain regulatory insights and influence policy (Gao and Huang, 2016; Jagolinzer et al., 2020). Unlike procurement-based firms, politically engaged firms—those investing in lobbying or making campaign contributions—primarily obtain information through legislative relationships (Christensen et al., 2017, 2023). These firms may have previously used voluntary disclosures to signal political risk and convey policy insights gained from government relationships. If the STOCK Act heightened scrutiny of firm-official interactions, politically engaged firms may have faced greater difficulty accessing discretionary regulatory insights, prompting disclosure adjustments. However, they may have also developed alternative channels to maintain information flow, mitigating the Act’s impact. Whether politically engaged firms altered disclosure post-STOCK Act remains an empirical question, leading to our final null hypothesis:

H3 (null): *The STOCK Act has no significant effect on the frequency of voluntary disclosures for politically engaged firms.*

3. Data and Empirical Design

3.1. Management Forecasts

We examine how the STOCK Act affected the frequency of voluntary management forecasts, drawing on the I/B/E/S Guidance database. Our key dependent variable, *GuidanceCount*, aggregates all forward-looking forecasts issued by management, including earnings, sales, cash flow, and capital expenditures, across both annual and quarterly horizons. $\text{Log}(1+\text{GuidanceCount})$ is defined as the natural logarithm of one plus the total number of forecasts issued in a firm-year (e.g., sales forecasts and capital expenditure (CAPEX) forecasts). Following standard practice, we winsorize $\text{Log}(1+\text{GuidanceCount})$ at the 1st and 99th percentiles.

The STOCK Act, signed into law by President Obama on April 4, 2012, received strong bipartisan support. Our study period spans from 2008 to 2015, covering Obama’s presidency, with four years before and after the Act’s enactment. To measure yearly voluntary disclosures for pre- and post-regulation periods, we define the firm-year as ending on April 4th of each year. For instance, all management forecasts between April 4, 2011, and April 4, 2012, measure a firm’s voluntary disclosure for 2011.

Our sample construction begins with firms in Compustat with continuous financial data from 2008 through 2015, yielding 52,464 firm-years for 6,558 unique firms. These firms are matched to I/B/E/S Guidance data, and firm-years with no forecast activity are coded as zero. This disclosure-based design

allows us to directly test whether firms altered their public communication strategies in response to the regulatory shock introduced by the STOCK Act.

3.2. Firms with Major Government Contracts

Publicly listed firms are required to disclose major customer sales under FAS 131, including those contributing more than 10% of total revenue. Utilizing data from the COMPUSTAT segment database, we identified firms that reported the U.S. government as a major customer for at least three consecutive years from 2008 to 2011, prior to the enactment of the STOCK Act. These firms maintain direct financial and operational relationships with executive branch agencies through structured bid evaluations, compliance audits, and contract renegotiations, necessitating sustained interactions with procurement officers, regulatory compliance teams, and federal agency auditors.

Unlike lobbying or PAC contributions, where firms seek to influence broad policy outcomes, government contracting involves formalized, recurring interactions with executive branch agencies. These relationships extend beyond regulatory compliance to real-time contract execution, performance audits, and agency negotiations that directly shape firms' operations. Firms with significant government contracts rely on timely procurement updates, agency policy shifts, and evolving compliance requirements, making them more dependent on ongoing access to government-sourced insights. The STOCK Act's restrictions on executive branch officials, therefore, represent a targeted shock to the flow of information these firms historically relied on, making them an ideal treatment group for this study.

A notable example of procurement-related misconduct is the 2003 Boeing-Air Force tanker scandal. Darleen Druyun, a senior Air Force procurement official, shared nonpublic bidding information with Boeing, giving the firm an unfair advantage in securing a multibillion-dollar refueling tanker contract. In exchange, she arranged a high-level job at Boeing and employment for family members. Investigators later uncovered that Druyun had inflated contract prices in Boeing's favor while disadvantaging competitors. She was sentenced to prison, and Boeing's Chief Financial Officer resigned as a result.⁸

This illustrative example highlights how firms with significant government contracts benefit from discretionary access to procurement-related information and how government officials may have incentives to share it. Prior to the STOCK Act, such relationships were largely unregulated under insider trading laws, which focused primarily on corporate executives rather than government officials.

We required each sample firm to have financial data for all eight years (2008-2015). These criteria

⁸U.S. Department of Justice. (2004). *Former Air Force Official Sentenced for Conspiracy to Violate Federal Conflict of Interest Laws*. Retrieved from https://www.justice.gov/archive/opa/pr/2004/October/04_crm_698.htm.

reduced the sample to 15,424 firm-years for 1,928 unique firms. We excluded observations with missing values for control variables in the main regressions. The final sample comprises 12,074 firm-years, with 15.4% classified as the treatment group.

Figure 1 shows the natural logarithm of the frequency of management forecasts for the treatment and control groups from 2008 to 2015. In the pre-STOCK Act period (2008-2011), the frequency of forecasts shows an upward trend for both groups. However, following the enactment of the STOCK Act in 2012, treatment firms, those with major government customers, demonstrated a significant decrease in management forecast frequency, while the change was much smaller for control firms. This comparison indicates that the STOCK Act had a more pronounced adverse impact on the frequency of management forecasts for firms with substantial government ties.

3.3. Empirical Design

To test our main hypothesis, we employ a difference-in-difference design using the following OLS model:

$$\text{Log}(1 + \text{GuidanceCount})_{f,t} = \alpha + \beta \text{Post}_t \times \text{MajorGovCustomer}_{f,t} + \chi_{f,t} + \phi_f + \phi_t + \varepsilon_{f,t}, \quad (1)$$

where $\text{GuidanceCount}_{f,t}$ represents the number of voluntary management forecasts issued by firm f in year t . We then examine the natural logarithm of one plus the total number of forecasts. The independent variables include MajorGovCustomer and Post , both dummy variables. We also conduct our analysis within a Poisson framework, following Cohn, Liu, and Wardlaw (2022).

The $\text{MajorGovCustomer}_{f,t}$ variable equals one if the government was a major customer for the company for at least three out of four years from 2008 to 2011. This definition captures firms with significant business relationships with the government, suggesting that these firms had greater opportunities for private communication with executive branch officers. The Post_t variable equals one for management forecasts issued after the passage of the STOCK Act, covering the fiscal years 2012 to 2015. The interaction term, $\text{Post}_t \times \text{MajorGovCustomer}_{f,t}$, captures the differential change in voluntary disclosure behavior for firms with major government customers following the Act's implementation. To control for firm-level heterogeneity, we include firm fixed effects (ϕ_f). Year fixed effects (ϕ_t) account for broader macroeconomic uncertainty, including fluctuations in government procurement spending and fiscal policy shifts, ensuring that our estimates isolate the differential impact of the STOCK Act rather than broader economic conditions.

We also incorporate a vector of firm-year control variables ($\chi_{f,t}$) to account for firm-specific factors that may affect voluntary disclosure. Specifically, we control for firm size (*Size*), profitability (*ROA*), book-to-market ratio (*BM*) and financial leverage (*Leverage*). The error term, $\varepsilon_{f,t}$, is clustered at the firm level to account for within-firm correlation over time.

The coefficient of interest, β , captures the effect of the STOCK Act on the disclosure behavior of firms with significant government contracts. A negative and significant β would indicate that firms reliant on government business reduced their voluntary disclosures following the Act.

One potential concern with our difference-in-difference design is that the financial crisis introduced heightened uncertainty, this period serves as a conservative benchmark. If firms were already issuing more forecasts due to the crisis, our estimates likely understate the true effect of the STOCK Act. To mitigate concerns that our results are driven by the economic recovery rather than the Act itself, we conduct placebo tests using firms that were not directly exposed to government procurement and confirm that their disclosure practices remain unaffected.

4. Empirical Results

4.1. Summary Statistics and Sample Composition

Our sample contains 12,074 firm-year observations from 1,664 firms included in the COMPUSTAT database that have continuous yearly performance from 2008-2015. We present our summary statistics for the full sample of firms in [Table 1](#). On average, 36.4 percent of firms issue some management forecasts, and the overall sample mean is 4.82 forecasts per year (logarithm of frequency of 0.868 per year). Approximately 15.4 percent of sample firms have been a major government supplier for at least three of the previous years before the STOCK Act (mean *MajorGovCustomer* = 0.154), indicating a reliance on government contracts.

We present the summary statistics for treatment and control firms pre- and post- STOCK Act in [Table 2](#). Panels A and B of [Table 2](#) indicate that major government customers (*MajGovCustomer* = 1) issue fewer forecasts, as compared to their counterparts, during both the pre- and post- period. During both the pre- and post- period, major government customers issue approximately 4 total forecasts, while their counterparts issue 4.755 in the pre-period and 5.157 in the post-period. During the pre-period, this difference is statistically significant at the 5 percent level, and it is statistically significant at the 1 percent level during the post period. On average, there is no difference between the size of firms designated and

not designated as government contractors pre- and post- STOCK Act, though government contractors have greater leverage and ROA.

4.2. Baseline Results: Management Forecasts and Forecast Precision

Our main difference-in-differences results are presented in Table 3, which examines the impact of private communication between executive branch officials and public firms on management forecast frequency. Columns 1 and 2 include industry and time fixed effects, with weakly significant negative coefficients on $Post \times MajorGovCustomer$. Column 4, which includes firm-year controls, shows a more pronounced effect, with a coefficient of -0.078 , significant at the 5 percent level. This corresponds to a 7.5% decline in forecast frequency, implying that affected firms issued approximately 0.08 fewer disclosures per year.⁹ We estimate a Poisson model in Column 5 to account for the count nature of *GuidanceCount* (Cohn, Liu, and Wardlaw, 2022), and the results are consistent in sign and significance.

To assess the timing of these effects, we estimate a dynamic event-study model where $Post \times MajorGovCustomer$ is replaced with interactions between *MajorGovCustomer* and annual indicators surrounding the STOCK Act's implementation. Figure 2 presents these coefficients and their 95% confidence intervals, using 2012 as the reference year. Prior to 2012, the interaction terms are negative but not statistically significant, supporting the parallel trends assumption. From 2012 onward, the coefficients remain negative, leading us to reject the null hypothesis that the STOCK Act had no effect on voluntary disclosures.

We validate parallel trends using the method of Roth (2022), which assesses whether pre-treatment trends would be statistically detectable given the observed sample variation. Figure 2 presents both univariate trends and the results of the pre-trends power analysis, with hypothesized pre-trends detectable 50% of the time. In the pre-treatment period, estimated coefficients are near zero with overlapping confidence intervals, providing no strong evidence of a violation of parallel trends. Post-treatment, the estimated coefficients remain stable, and the expectation after pre-testing aligns closely with the hypothesized trend. This supports the validity of our event-study approach.¹⁰

Beyond disclosure frequency, the precision of voluntary disclosures is important for market participants. Prior research suggests that when managers face heightened uncertainty, they may reduce the

⁹The estimated effect of the STOCK Act on management forecast frequency is obtained from Table 3, Column 4, where the coefficient on $Post \times MajorGovCustomer$ is -0.078 . Since the dependent variable is the natural logarithm of $(1 + \text{Frequency})$, the percentage decline is given by $e^{-0.078} - 1 = -7.5\%$. The mean pre-STOCK Act forecast frequency for government contractors is 0.752 (log scale from Table 2), translating to $e^{0.752} - 1 = 1.12$ actual forecasts per year. Multiplying 7.5% by 1.12 gives an approximate reduction of 0.08 forecasts per year.

¹⁰A discussion of this method and its implementation in Stata is available at <https://github.com/mcaceresb/stata-pretrends>.

specificity of their guidance, opting for range forecasts instead of point forecasts (Skinner, 1997; Anantharaman and Zhang, 2011; Guay, Samuels, and Taylor, 2016). In Table 4, we examine the precision of management forecasts. We define a variable *RangeForecast* that calculates the percentage of a company's forecasts that are reported as a range, as opposed to a point estimate. Please note that we only compute this variable for the subset of firms with management forecasts. Our results indicate that the coefficient on $Post \times MajorGovCustomer$ is statistically significant across all four columns. Specifically, Column 4 indicates that firms with major government contracts experienced an approximately 8.6% increase in the likelihood of issuing range forecasts post-STOCK Act, relative to the pre-STOCK Act mean.¹¹ This suggests that losing access to government-sourced information increased firms' uncertainty, leading them to provide less precise and potentially less informative guidance to investors.

This precision test also helps address concerns that our findings reflect mechanical effects of Reg FD. Reg FD requires firms to publicly disclose any material non-public information shared with select parties, including government officials, which may have previously encouraged more voluntary forecasts. If the STOCK Act reduced officials' incentives to seek such information, firms may have issued fewer forecasts as a result. However, Reg FD governs equal access, not disclosure format. It mandates public dissemination but does not require point forecasts over ranges. The shift we observe toward less precise guidance suggests a change in information quality, not just disclosure frequency.

Therefore, to disentangle the potential effects of the STOCK Act from those of Reg FD, we examine whether firms change their forecast behaviors post-STOCK Act. In particular, if firms lost access to privileged insights from government officials, they would be more likely to issue range forecasts, reflecting heightened uncertainty, rather than precise point forecasts. Table 4 supports this prediction: firms with major government customers became significantly more likely to issue range forecasts following the Act. This shift suggests that executive branch officials had previously served as a valuable source of private information. The decline in precision reflects reduced informational access, not merely a mechanical response to Reg FD.

4.3. Cross-Sectional Analysis: Demand Uncertainty and Political Engagement

Although our baseline results show that the STOCK Act curtailed voluntary disclosure, part of the decline likely reflects the loss of privileged policy insights that federal contractors had relied on to

¹¹The estimated effect of the STOCK Act on forecast precision is obtained from Table 4, Column 4, where the coefficient on $Post \times MajorGovCustomer$ is 0.059. Since the dependent variable represents the proportion of forecasts issued as ranges, this corresponds to a 5.9 percentage point increase. The mean pre-STOCK Act proportion of range forecasts for government contractors is 0.689 (from Table 2), translating to a relative increase of $(0.059/0.689) \times 100 = 8.6\%$.

anticipate contract renewals and regulatory shifts. With those channels cut off, demand uncertainty rose and management forecasts fell, especially at firms most dependent on government business. This evidence suggests that political ties provide more than the economic rents documented by [Akey \(2015\)](#); they also supply discretionary information that shapes disclosure strategy. By severing those information flows, the STOCK Act heightened uncertainty and prompted managers to scale back guidance, emphasizing the informational value of political connections. We next test how this effect varies with (1) demand uncertainty and (2) a firm’s political engagement.

4.3.1. Demand Uncertainty Channel

To test whether demand uncertainty played a role in shaping disclosure responses, we examine three firm-level indicators: (1) the proportion of government sales relative to total sales (*GovSalesRatio*), (2) the number of major government customers (*GovCustomNum*), and (3) the volatility of government sales (*GovSalesVol*). Each of these measures is computed over the pre-STOCK Act period (2008–2011), with firms classified as high or low based on whether they fall above or below the median. We hypothesize that firms with greater dependence on government sales, more government customers, or higher revenue volatility are more affected by changes in information flow following the STOCK Act, leading to greater reductions in voluntary disclosures. To formally test this, we estimate the following regression model:

$$\begin{aligned} \text{Log}(1 + \text{GuidanceCount})_{f,t} = & \alpha + \beta_1 \text{Post}_t \times \text{HighDemandVar}_f + \beta_2 \text{Post}_t \times \text{LowDemandVar}_f \\ & + \chi_{f,t} + \phi_f + \phi_t + \varepsilon_{f,t} \end{aligned} \quad (2)$$

where the variables are defined as in [Equation \(1\)](#), and *HighDemandVar* and *LowDemandVar* represent the above- and below-median demand variables.

[Table 5](#) reports the results. Column 1 shows that firms with a high proportion of government sales (*HighGovSalesRatio*) exhibit a significant reduction in management forecast frequency post-STOCK Act, with the coefficient on *Post* $\times$ *HighGovSalesRatio* negative and significant at the 1% level. In contrast, the interaction term for firms with lower government sales (*LowGovSalesRatio*) is insignificant, indicating that firms less dependent on government revenue were not meaningfully affected. Columns 2 and 3 examine the number of government customers and the volatility of government sales, respectively. Across both measures, the results remain consistent: firms with more government customers (*HighGovCustomNum*) and firms with greater volatility in government sales (*HighGovSalesVol*) experience declines in management forecast frequency, while their low-exposure counterparts show no meaningful changes. We

estimate a poisson model in Columns 4-6 to account for the count nature of *GuidanceCount* (Cohn, Liu, and Wardlaw, 2022), and the results are consistent in sign and demonstrate statistical significance.

These findings suggest that firms with stronger ties to government business faced greater uncertainty post-Act, prompting a more cautious approach to voluntary disclosure. Rather than specifically prohibiting information exchanges, the Act altered officials' incentives to engage with firms, making key regulatory and policy insights less accessible. Among the three indicators, the proportion of government sales (*HighGovSalesRatio*) shows the strongest effect, potentially because revenue stability is a primary concern for investors. Faced with uncertainty around contract renewals and policy shifts, managers may have withheld guidance rather than risk issuing forecasts based on incomplete or unreliable information. This aligns with prior research showing that firms often reduce disclosure under uncertainty to avoid credibility risks (Anantharaman and Zhang, 2011; Guay, Samuels, and Taylor, 2016). Lower-quality disclosures can also trigger negative investor reactions (Chen et al., 2018).

4.3.2. Political Engagement Channel

Firms do more than rely on government contracts—they actively engage in political relationships to influence policy and navigate regulatory environments (Gao and Huang, 2016). Political engagement can be seen as a form of strategic compliance, where firms align with government priorities through contributions, policy advocacy, and disclosures about political events or positions. Some firms invest directly in maintaining access to decision-makers, while those in competitive industries may engage indirectly by leveraging government relationships for regulatory advantages.

For example, the case of Darleen Druyun, the Air Force procurement officer who admitted to favoring Boeing in contracting decisions while negotiating a job with the company, highlighting the risks posed by unchecked access to procurement information. While her case predated the STOCK Act, it vividly illustrates how personal incentives can compromise government integrity and distort the flow of sensitive information. The STOCK Act sought to reduce such vulnerabilities by increasing transparency and limiting the potential for officials to benefit privately from non-public information related to government decision-making.

The STOCK Act increased scrutiny on financial transactions and interactions between government officials and firms, potentially altering the incentives for politically connected firms to engage in private communication. If firms previously relied on executive branch ties for privileged insights, heightened oversight may have led them to scale back voluntary disclosures to mitigate regulatory and reputational

risks. At the same time, firms with strong government relationships may have signaled compliance by maintaining or even increasing disclosures related to political activities. These shifting trade-offs may have driven politically engaged firms, both established and aspiring, to reconsider how much, and what type of, information they release, producing measurable shifts in reporting behavior.

We test for direct political engagement by analyzing firms' political activity using two proxies: (1) the amount of political contributions made by the firm (*FedContribution*) and (2) the number of politicians connected to the firm through campaign donations or lobbying activities (*ConnectedPolitician*). We obtain these measures from OpenSecrets and classify firms into high and low engagement groups based on their ex-ante median values.

[Table 6](#) presents the results. Firms in the high-engagement group (Columns 1 and 3) exhibit a significant reduction in management forecast frequency post-STOCK Act, while there are no statistically significant changes for the low-engagement group. These findings suggest that politically connected firms curtailed voluntary disclosures when they could no longer benefit as directly from interactions with executive branch officials. The loss of privileged policy insights may have increased uncertainty, making firms more cautious in issuing forward-looking statements.

Taken together, these results support the idea that political engagement, both direct and indirect, played a significant role in shaping firms' voluntary disclosure decisions post-STOCK Act. Firms with established political connections responded to the changing regulatory environment by reassessing the trade-offs of voluntary disclosure, reinforcing the broader impact of the Act on corporate transparency.

4.4. Text-Based Evidence on the STOCK Act's Impact on Corporate Disclosure

To systematically capture discussions related to government contracting, along with regulatory uncertainty and political risk, we construct two text-based measures using firm-level conference call transcripts. These measures provide a direct window into how corporate executives communicate about government contracts and uncertainty following the STOCK Act, shedding light on how firms' narratives and priorities shifted when private communication with government officials became more constrained.

The variable *Gov_Contract_Terms* quantifies the extent to which firms discuss government contracts using a curated dictionary of procurement-related terms. The variable *Regulatory_Policy_Risk* quantifies the amount of content pertaining to regulatory and policy risk that firms discuss in their conference call transcripts. Both variables are scaled by the total number of words in the conference call and multiplied by 10,000. Details pertaining to the dictionaries for both variables is described in [Appendix OA3](#).

Table 7 presents the regression estimates for these measures, showing how firms adjusted their disclosure patterns post-STOCK Act. When examining government contract terms in Column 1, the coefficient on $Post \times MajorGovCustomer$ is negative and statistically significant, indicating a decline in government contracting discussions post-STOCK Act. This result suggests that firms may have become less willing or less able to discuss procurement publicly due to the loss of private communication channels with government officials. The reduced ability to gather informal regulatory insights likely increased uncertainty surrounding contract renewals, aligning with our broader findings on heightened disclosure frictions in affected firms.

The variable *Regulatory_Policy_Risk* measures discussions of regulatory and policy issues, derived from a dictionary of terms related to legislative changes and government oversight. The coefficient on $Post \times MajorGovCustomer$ is positive and statistically significant. This suggests that firms with government contracts faced heightened concerns about regulatory unpredictability following the STOCK Act.

Taken together, these text-based findings suggest that the STOCK Act altered how firms discuss government-related topics in their public disclosures. The reduction in *Gov_Contract_Terms* implies that firms became more cautious in explicitly referencing government procurement, potentially due to greater uncertainty or efforts to limit discussions of regulatory exposure. The increase in *Regulatory_Policy_Risk* indicates that firms perceived greater exposure to political and regulatory uncertainty, possibly reflecting a shift in how they assess and communicate risks related to government interactions. Collectively, these results suggest that restricting informal information flows influenced not just the frequency but also the nature of firms' disclosures, potentially shaping how investors and other stakeholders interpret government-related risks.

4.5. Stock Price Informativeness and Capital Market Consequences

Price Informativeness Market prices reflect a combination of public and private information. According to Roll (1984), a large portion of stock price movements are driven by firm-specific, nonpublic information. To examine the impact of the STOCK Act on price informativeness, we examine three measures: stock return synchronicity, generalized price informativeness (GPIN), and price delay.¹²

Our first measure, stock return synchronicity (R^2), captures the extent to which firm-specific infor-

¹²We incorporate the firm-year GPIN estimates from Duarte, Hu, and Young (2020), which reduces our sample size for this measure because their parameters are available only for NYSE-listed firms. In contrast, the sample sizes for stock return synchronicity and price delay are primarily reduced due to data availability and filtering in CRSP weekly returns. Specifically, the price delay measure requires at least ten weeks of valid firm returns and corresponding market returns per firm-year, while the synchronicity regressions depend on lagged and forward-looking market and industry returns. Thin trading, delistings, and misaligned or missing market or industry return data result in the exclusion of firm-years with insufficient or inconsistent data.

mation is incorporated into stock prices, with lower R^2 values indicating greater price informativeness (Morck, Yeung, and Yu, 2000; Durnev et al., 2003; Piotroski and Roulstone, 2004; Jin and Myers, 2006; Hutton, Marcus, and Tehranian, 2009). However, regulatory changes that affect information flow may alter this dynamic.¹³

Following this literature, we compute market synchronicity, RSQ , as the R^2 from the following index-model regression:

$$r_{f,t} = \alpha + \beta_1 r_{m,t-1} + \beta_2 r_{m,t} + \beta_3 r_{m,t+1} + \gamma_1 r_{i,t-1} + \gamma_2 r_{i,t} + \gamma_3 r_{i,t+1} + \epsilon_{f,t} \quad (3)$$

where $r_{f,t}$, $r_{m,t}$, and $r_{i,t}$ are monthly excess returns of firm f , the market m , and the industry i , respectively, during year t . To align with prior studies (Morck, Yeung, and Yu, 2000; Piotroski and Roulstone, 2004; Hutton, Marcus, and Tehranian, 2009; Crawford, Roulstone, and So, 2012), we apply a logistic transformation to obtain our empirical measure of firm-specific information arrival:

$$IDIOSYN_{f,t} = \ln \left(\frac{1 - RSQ_{f,t}}{RSQ_{f,t}} \right) \quad (4)$$

Higher values indicate greater firm-specific information impounded in stock prices. We present our results in Table 8, Column 1, showing that the coefficient on $Post \times MajorGovCustomer$ is negative and statistically significant at the 10% level. This suggests that firm-specific information was less incorporated into stock prices post-STOCK Act, leading to a weaker market reaction to firm-level news. This result suggests that the flow of firm-specific government-related information into stock prices was disrupted, reducing price informativeness. In Column 2, we add additional equity controls to account for the volatility ($Sigma$), skewness, and kurtosis of equity prices over the previous calendar year, as in Hutton, Marcus, and Tehranian (2009), and our results remain robust.

We corroborate these findings using an alternative measure of price informativeness from Duarte, Hu, and Young (2020), namely $GPIN$, an annual structural parameter estimate derived from order flow that proxies for private information arrival. Higher $GPIN$ values indicate more informative prices. Column 3 of Table 8 shows that firms with major government customers exhibit a statistically significant decline in $GPIN$ post-STOCK Act, with an estimated reduction of approximately 3.1% in private information arrival.¹⁴ We further add the equity controls in Column 4, and our results remain robust.

¹³Some debate exists over this interpretation. For example, Chan and Chan (2014) argue that lower R^2 can indicate less firm-specific information in prices, particularly for firms with no analyst coverage. However, almost all firms in our sample have analyst coverage, making this concern less relevant.

¹⁴The estimated effect of the STOCK Act on $GPIN$ is obtained from Table 8, Column 3, where the coefficient on $Post$

We further examine price delay, a complementary measure of price efficiency that captures how quickly stock prices incorporate market-wide information. Following [Hou and Moskowitz \(2005\)](#), we construct a firm-level delay measure based on the extent to which lagged market returns help explain contemporaneous firm returns. Higher values indicate slower incorporation of market information, reflecting lower price efficiency. In [Table 8](#), Column 5, we find that major government contractors experience a statistically significant increase in price delay following the STOCK Act. This result suggests that stock prices became slower to reflect public market signals, consistent with weakened information environments. The increase in price delay reinforces the view that restricting informal information channels, such as those potentially facilitated by interactions with government executive officials, reduced the speed and precision with which firm-specific information entered equity markets.

Taken together, these findings indicate that the STOCK Act reduced price informativeness by disrupting the flow of firm-specific regulatory and policy insights, making market prices less informative for investors.

Analyst Forecast Properties If the STOCK Act altered firms' information environments, its effects should manifest in analyst forecast accuracy. A reduction in firm-specific information could lead to greater forecast errors but lower dispersion, as analysts coalesce around noisier estimates.

We investigate the impact of the STOCK Act on analyst forecast properties, focusing on the number of analysts making forecasts (*ANALYSTS*), forecast errors (*FCSTERROR*) and forecast dispersion (*DISPERSION*) from the IBES Summary file for annual forecasts. [Table 9](#) presents the regression results. Column 1 shows that the interaction term $Post \times MajorGovCustomer$ is negative and significant, indicating that firms with major government customers are losing analysts at a faster rate than their counterparts after the STOCK Act. In Column 2, the interaction coefficient is positive and statistically significant for forecast errors (*FCSTERROR*), indicating that analysts' predictions became less accurate for firms with major government customers after the STOCK Act, reflecting heightened uncertainty in their information environments. Since some sample firms do not have an analyst following, this sample size is reduced. When examining *DISPERSION*, the sample size is further reduced because in order to compute this measure, two analysts must be present.

Column 3 indicates that forecast dispersion (*DISPERSION*) decreased post-STOCK Act for major government customers, with the interaction term negative and statistically significant. This suggests that,

$\times MajorGovCustomer$ is -0.013 . The pre-STOCK Act mean of *GPIN* for government contractors is 0.426 ([Table 2](#)). Applying the estimated decline, the new mean is approximately $0.426 - 0.013 = 0.413$, corresponding to a 3.1% reduction ($0.013/0.426 \times 100$).

despite the rise in forecast errors, analysts exhibited greater consensus in their estimates. One possible explanation for this finding is that the reduction in firm-specific information forced analysts to rely more heavily on the same set of public disclosures, leading to more forecast homogeneity.

These findings highlight the broader implications of the STOCK Act for capital market participants. While the Act was designed to curb insider trading, it also inadvertently narrowed the flow of firm-specific information, affecting not just firms but also the analysts and investors who depend on these forecasts. Increased forecast errors highlight the cost of reduced voluntary disclosure, while the decline in dispersion suggests that analysts are now working with a more uniform but less informative data environment.

Implied Cost of Capital Our findings indicate a decline in voluntary disclosure following the STOCK Act. According to disclosure theory (Easley and O'hara, 2004), greater transparency reduces information asymmetry and lowers a firm's cost of capital. If firms with major government contracts reduce disclosures post-STOCK Act, investors may perceive greater uncertainty, leading to an increase in the implied cost of capital (ICC) for firms that reduced their disclosures.

To assess the effect of the STOCK Act on firms' internal rates of return, as influenced by changes in voluntary disclosure, we utilize the following regression framework:

$$ICC_{i,t} = \alpha + \beta_1 \text{Post}_i \times \text{MajorGovCustomer}_t \times \text{Log}(1 + \text{GuidanceCount})_{i,t} + \beta_2 \text{Post}_i \times \text{MajorGovCustomer}_t + \beta_3 \text{Log}(1 + \text{GuidanceCount})_{i,t} + \chi_{it} + \phi_i + \phi_t + \varepsilon_{it}, \quad (5)$$

The dependent variable $ICC_{i,t}$ represents five widely used measures of ICC: GLS_ICC (Gebhardt, Lee, and Swaminathan, 2001), CAT_ICC (Claus and Thomas, 2001), PEG_ICC (Easton, 2004), OJM_ICC (Ohlson and Juettner-Nauroth, 2005), and AVG_ICC , an equally weighted average of the four.

Table 10 presents the results. The triple interaction term $\text{Post} \times \text{MajorGovCustomer} \times \log(1 + \text{GuidanceCount})$ is negative and statistically significant across most specifications. This pattern indicates that the rise in implied cost of capital for government contractors following the STOCK Act was concentrated among firms that curtailed their voluntary disclosures. In contrast, firms that maintained more frequent guidance were better able to preserve lower capital costs. These findings suggest that reduced managerial communication, potentially driven by the loss of privileged policy insight, contributed to a deterioration in firms' information environments and increased financing costs.

These results complement the analyst forecast and synchronicity findings. As analysts faced fewer dis-

closures and diminishing access to informal political channels, forecast accuracy declined and dispersion compressed, two consequences of a thinner information environment. The ICC effects provide further evidence that these shifts had real capital market consequences. Firms with major government contracts that curtailed disclosure after the STOCK Act not only experienced weaker information environments but also paid a measurable price in the form of increased financing costs.

5. Additional Analysis

5.1. Congressional Stock Holdings and Corporate Disclosure

Prior research finds that the STOCK Act curtailed insider trading by members of Congress (Cherry, Heitz, and Jens, 2018; Huang and Xuan, 2023; Wei and Zhou, 2023), raising the question of whether firms previously held by legislators adjusted their disclosure practices in response. Before the Act, congressional investors may have benefited from informal access to firm-specific information, either through private communications or privileged oversight. The Act's increased legal scrutiny may have reduced the frequency or value of such interactions, prompting firms to shift toward more public disclosure to compensate for the loss of these informational channels.

Beyond informal access, congressional ownership can also raise concerns about influence over government contracting. Legislators with equity stakes in firms negotiating federal contracts may have incentives to engage executive branch officials, potentially affecting award outcomes. This raises the question of whether firms with prior congressional ownership behave differently following the STOCK Act.

To test this, we construct a new variable, *CongressOwn*, equal to one if a firm was ever held by a member of Congress between 2008 and 2011, using data from OpenSecrets. We then estimate a difference-in-differences model, replacing the primary treatment indicator with *CongressOwn*, and introduce a triple interaction, $Post \times MajorGovCustomer \times CongressOwn$, to assess whether disclosure changes were concentrated among politically connected firms most reliant on federal contracts.

Table 11 tests whether equity holdings by sitting members of Congress moderated the STOCK Act's effect on voluntary disclosure. Column 1 shows that firms once held by legislators issued fewer management forecasts after 2012; the interaction coefficient ($Post \times CongressOwn$) is negative and statistically significant. The magnitude is smaller than in our main contractor tests (Table 5), consistent with congressional ties cushioning, though not eliminating, the loss of policy information.

Column 2 adds a triple interaction with the major government customer indicator. The coefficient

is economically small and statistically insignificant, indicating that congressional ownership does not differentially influence disclosure at firms most exposed to federal procurement. These results suggest that legislative connections provide only limited informational advantages relative to executive-branch channels, which remain the primary source of firm-specific policy insight. In Columns 3 and 4, we present consistent results using a poisson framework.

5.2. Government Contracts and Major Customers

In [Table A1](#), we refine our analysis by introducing *OnlyGovContract*, a dummy variable set to one for firms with government contracts but without the government as a major customer from 2008 to 2011, and zero otherwise.¹⁵ This distinction allows us to compare firms based on their varying reliance on government relationships through mutual acknowledgement. The results show that the coefficient on $Post \times MajorGovCustomer$ is -0.079 (significant at the 0.05 level), indicating that firms with major government customers reduce management forecasts post-STOCK Act. In contrast, the interaction term for *OnlyGovContract* is insignificant, suggesting that firms with government contracts but without major government customers are less affected.

This differential effect highlights two key insights: (1) firms with major government customers are more vulnerable to reduced information flow under the STOCK Act, and (2) firms with government contracts but limited dependence on government sales are less impacted, as their disclosure practices are not closely tied to government interactions. These findings underscore the distinct role of government reliance in shaping corporate disclosure.

6. Robustness

To ensure the robustness of our findings, we conducted a series of additional tests, as outlined in [Section 4.2](#). These tests consistently support our primary hypothesis, demonstrating that the reduced private communication between firms and executive branch officers due to the STOCK Act leads to a significant decrease in voluntary disclosure activities.

¹⁵Listed firms in the U.S. are required to disclose their major customers, including government customers, under Financial Accounting Standard No. 131 (FAS 131), while the U.S. government is required to publicly disclose procurement outcomes under the Federal Acquisition Regulation (FAR).

6.1. Propensity Score Matching and Entropy Balancing

We re-estimate the analysis in [Table 3](#) using two alternative matching strategies to improve covariate balance: propensity score matching (PSM) and entropy balancing. For PSM, we estimate treatment probabilities using a logistic regression with log market value, leverage, book-to-market ratio, and return on assets as covariates. Treated observations are then matched to the nearest control observation without replacement. In the entropy balancing specification, we reweight the control group to match the first moments (means) of the covariates in the treated group, ensuring exact balance on observed characteristics. This method avoids the loss of observations and improves covariate comparability without relying on functional form assumptions. Both approaches yield similar results to our main specification, reinforcing the robustness of our findings. Full results are discussed in Online [Appendix OA4](#) and presented in [Table OA.3](#).

6.2. Placebo Tests

To assess whether unobserved confounders drive our results, we first conduct a placebo test following [Brogaard, Gerasimova, and Rohrer \(2024\)](#). We randomly assign treatment status (*MajorGovCustomer* = 1) across firms while holding all other variables constant, repeating this process 10,000 times to generate a distribution of placebo coefficients. As shown in [Figure OA.1](#), the distribution is centered at zero, with 99.7% of simulated coefficients falling between -0.090 and +0.090, while our actual estimate (-0.078) lies in the extreme left tail ($p < 0.002$). This suggests that the observed reduction in management forecast frequency is unlikely to be driven by random chance or omitted variables, supporting a causal interpretation of the STOCK Act's effect.

Second, to assess the validity of our event date, we conduct a placebo test using 2006 as our event date, which was the year the STOCK Act was first introduced to Congress but ultimately failed to pass. We re-estimate our difference-in-differences specification using data spanning 2002-2012 and use 2006 as the event year and find no meaningful differences in forecast frequency between treatment and control groups. Further details are provided in Online [Appendix OA5](#).

6.3. Alternative Data Sample and Event Dates

To ensure our findings are not driven by time measurement choices, we employ two alternative definitions. First, we adopt a fiscal-year-based approach, excluding 2012 (the STOCK Act's passage year) and using a symmetric four-year window before and after. Second, we shift the event date from April

4, 2012, to April 30, 2012, a commonly used fiscal year-end. As shown in [Figure OA.2](#) and detailed in the Online Appendix, both approaches yield consistent results, confirming that the observed decline in management forecasts is not an artifact of time measurement.

We also test different event windows to account for uncertainty in the STOCK Act's impact duration. Using three-year and six-year periods, as well as an alternative event date—November 13, 2011, when 60 Minutes aired its exposé on Congressional insider trading—we continue to find significant declines in forecast frequency ([Table OA.5](#)). These results confirm that the STOCK Act's effects persist across various event definitions and timeframes.

To address sample composition concerns, we conduct two additional tests. First, we exclude firms that had never issued management forecasts, ensuring that the observed reductions are not concentrated among firms with historically low disclosure. Second, we restrict the sample to unregulated industries by removing financial, utility, and pharmaceutical firms, which are subject to continuous oversight by the SEC, Department of Energy, and FDA. The results, presented in [Table OA.6](#) and discussed in the Online [Appendix OA7](#), remain statistically significant. This suggests that the STOCK Act primarily affected firms that relied on private communication with government officials rather than those already under strict regulatory supervision.

6.4. Potential Influence of Outliers

In our primary tests, we examine the frequency of overall management forecasts before and after the STOCK Act. One potential concern is that our findings are driven by outliers. However, our underlying data indicates that over our total sample period of eight years, 693 firms issued forecasts, with 386 firms increasing their forecast frequency and 300 firms decreasing it. These findings suggest that the changes in management forecasts are driven by a broad range of firms rather than a few extreme cases, indicating that outliers do not distort the overall effect. This supports the conclusion that the STOCK Act impacts most firms issuing forecasts.

7. Conclusion

This study examines the unintended consequences of the STOCK Act on corporate voluntary disclosure. By restricting government officials from profiting from non-public information, the Act likely reduced their incentives to share regulatory insights, procurement details, and policy developments with firms. We find that firms with substantial government contracts experienced a 7.5% decline in manage-

ment forecast frequency following the Act's enactment, translating to 0.08 fewer forecasts per firm per year. This reduction in disclosure is accompanied by an increase in stock return synchronicity and cost of capital, suggesting weakened transparency, reduced investor confidence, and higher financing costs. The decline in management forecasts is most pronounced among firms with high demand uncertainty from government contracts and those with greater political engagement, indicating that firms most reliant on private information flows from executive branch officials were disproportionately affected. These findings highlight the broader capital market implications of restricting informal government-firm communication.

Our findings shift the focus of political connections research from the benefits of access to the consequences of withdrawal. We show that when firms lose access to privileged information flows, following the STOCK Act, their disclosure practices and capital market outcomes deteriorate. Whereas prior studies (e.g., [Akey, 2015](#); [Heitz, Wang, and Wang, 2023](#)) emphasize the financial and regulatory advantages of political connections, we demonstrate that severing these connections impairs the firm's ability to acquire discretionary policy information, with downstream effects on voluntary disclosure and market perception. The STOCK Act serves as a rare natural experiment that indirectly disrupted informal channels of discretionary government-firm information sharing. Firms that had come to rely on these opaque flows in shaping their disclosures appeared less able to adjust, highlighting the subtle but important role such ties can play in supporting corporate transparency.

From a policy perspective, our findings suggest an important trade-off. Insider trading reforms promote public accountability but may unintentionally degrade market transparency when they sever informal, informationally rich ties between firms and policymakers. These channels, while opaque, can play a critical role in shaping firms' disclosure practices and helping investors form expectations. Regulators weighing additional restrictions on official conduct should consider not only the benefits of limiting private gain but also the potential costs to capital-market efficiency when discretionary information flows are curtailed.

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Tables and Figures

Figure 1. The Frequency of Management Forecasts

This figure plots the natural logarithm of the frequency of management forecasts for firms with and without major government customers from 2008 to 2015. The sample consists of 12,074 firm-years. The red solid line represents the treatment firms (15.4% of the sample) that reported major government customers for at least three years in the four years (2008-2011) before the STOCK Act was enacted. The blue dashed line represents the control firms.

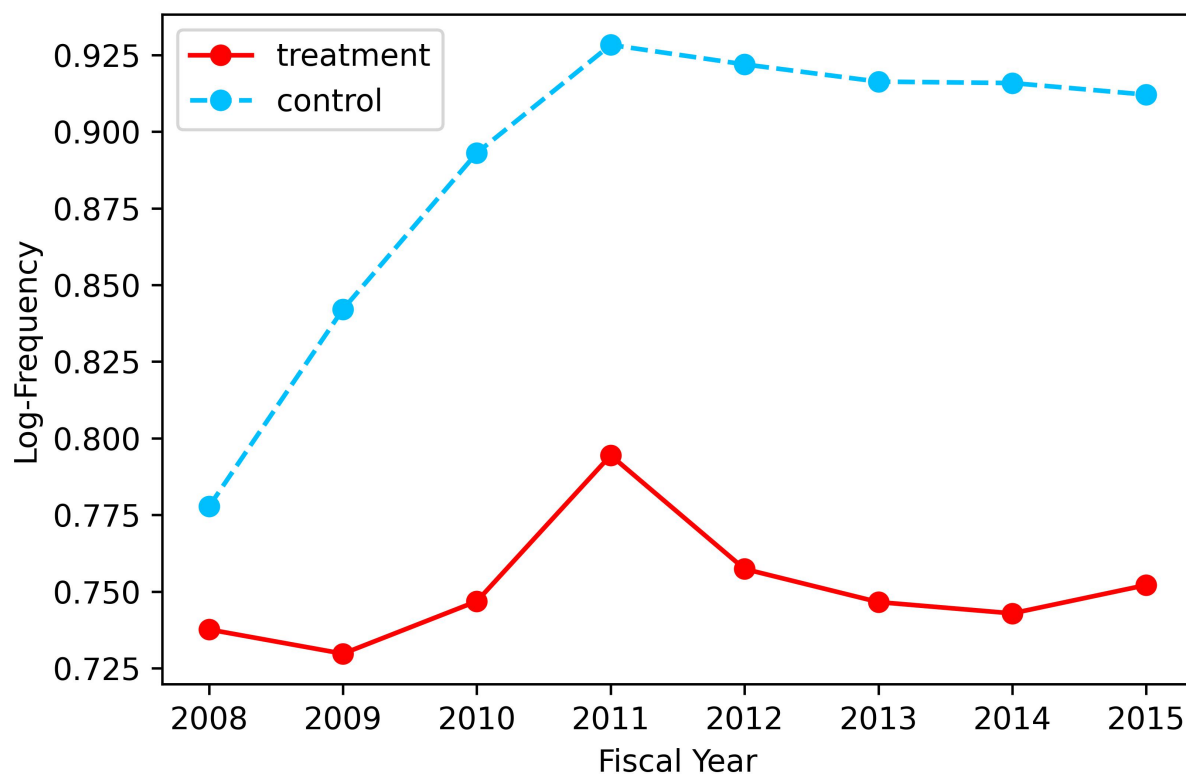


Figure 2. Roth (2022) Pre-Trend Power Analysis

This figure implements the Roth (2022) method for assessing post-period differences between treatment and control banks relative to expected outcomes from a continuation of pre-period trends. For each outcome variable, the estimated trend is based on a 50% likelihood of detecting a significant coefficient in the pre-period, and confidence intervals are set at the 95% level. The coefficients are based on a modified version of Equation (1) where the interaction term $Post \times MajorGovCustomer$ is replaced with interaction terms between $MajorGovCustomer$ and yearly indicator variables.

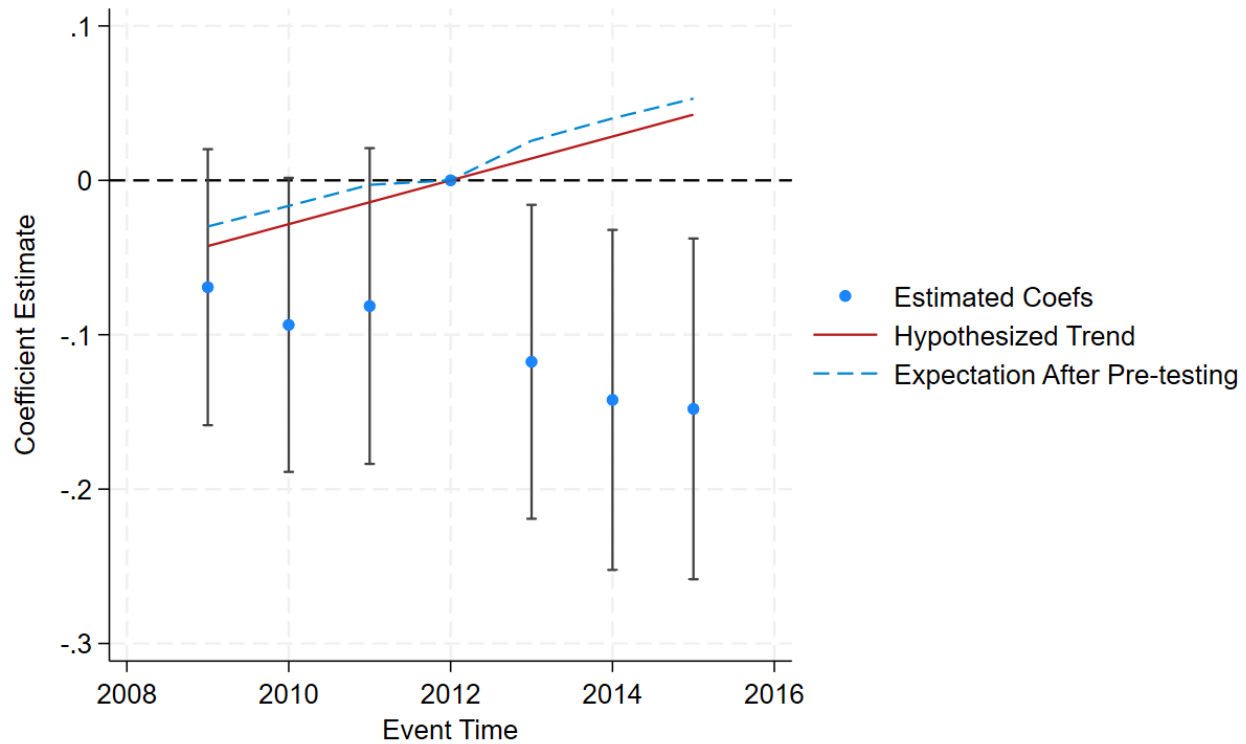


Table 1. Summary Statistics

This table presents descriptive statistics for the sample of publicly traded firms from 2008 to 2015. All continuous variables are winsorized at 1% and 99% levels. Variable definitions are provided in [Table OA.1](#).

Variable	N	Mean	Std	P25	Median	P75
Disclosure Measures:						
GuidanceCount	12,074	4.817	8.551	0	0	7
Log(1+GuidanceCount)	12,074	0.868	1.245	0	0	2.079
RangeForecast	2,535	0.705	0.338	0.545	0.833	1
Price Informativness and Analysts:						
IDIOSYN	11,609	-1.421	1.521	-2.153	-1.337	-0.561
GPIN	6,466	0.444	0.102	0.383	0.474	0.539
DELAY	11,667	0.336	0.284	0.103	0.239	0.507
ANALYSTS	12,074	4.825	7.225	0	1	7
FCSTERROR	6,570	0.118	0.676	0.001	0.003	0.013
DISPERSION	6,219	0.058	0.342	0.001	0.002	0.005
Textual Analysis:						
Gov_Contract_Terms	12,074	0.469	2.008	0	0	0
Regulatory_Policy_Risk	12,074	0.057	0.303	0	0	0
Implied Cost of Capital:						
GLS_ICC	8,401	0.096	0.068	0.063	0.083	0.110
OJM_ICC	7,048	0.082	0.132	0.032	0.050	0.077
CAT_ICC	8,378	-0.075	0.197	-0.192	-0.087	0.021
PEG_ICC	8,452	0.130	0.151	0.016	0.087	0.178
AVG_ICC	8,458	0.059	0.113	-0.009	0.033	0.091
Controls:						
Size	12,074	6.522	2.154	5.012	6.521	7.977
LEV	12,074	0.176	0.190	0.001	0.149	0.264
BM	12,074	0.608	0.594	0.267	0.485	0.788
ROA	12,074	-0.042	0.242	-0.039	0.027	0.066
Sigma	11,794	0.129	0.078	0.075	0.111	0.161
Skewness	11,794	0.190	0.713	-0.285	0.149	0.612
Kurtosis	11,794	-0.089	1.141	-0.867	-0.392	0.340

Table 2. Treatment and Control Descriptive Statistics

This table presents descriptive statistics separately for treated firms (Major Government Customers) and control banks. Panel A shows summary statistics for the pre-STOCK Act period from 2008 to 2011, and Panel B shows summary statistics for the post-disclosure period from 2012 to 2015. Variable definitions are provided in [Table OA.1](#). Significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
	MajorGovCustomer=1			MajorGovCustomer=0			Mean		
	Mean	Med	N	Mean	Med	N	Difference	t-stat	
Panel A: Pre-STOCK Act Period									
GuidanceCount	4.044	0	933	4.755	0	4996	-0.711	(-2.50)	**
Log(1+GuidanceCount)	0.752	0	933	0.862	0	4996	-0.109	(-2.57)	**
RangeForecast	0.689	0.800	209	0.710	0.833	1265	-0.022	(-0.86)	
IDIOSYN	-1.603	-1.527	880	-1.639	-1.505	4754	0.036	(0.66)	
GPIN	0.426	0.474	477	0.424	0.474	2590	0.003	(0.52)	
DELAY	0.249	0.158	873	0.268	0.167	4809	-0.019	(-2.06)	**
ANALYSTS	4.815	2.000	933	4.389	1.000	4996	0.426	(1.90)	*
FCSTERROR	0.182	0.006	558	0.170	0.006	2581	0.011	(0.28)	
DISPERSION	0.118	0.003	531	0.086	0.003	2433	0.032	(1.35)	
Gov_Contract_Terms	1.815	0.0	933	0.286	0.0	4996	1.529	(12.22)	***
Regulatory_Policy_Risk	0.084	0.0	933	0.051	0.0	4996	0.034	(3.19)	***
GLS_ICC	0.093	0.090	748	0.090	0.083	3401	0.003	(1.56)	
OJM_ICC	0.092	0.058	632	0.084	0.049	2723	0.009	(1.49)	
CAT_ICC	-0.053	-0.054	745	-0.082	-0.085	3387	0.028	(3.98)	***
PEG_ICC	0.102	0.059	752	0.116	0.076	3438	-0.014	(-2.64)	***
AVG_ICC	5.610	4.076	819	5.152	3.198	3994	0.458	(1.19)	
Size	6.243	6.311	933	6.340	6.309	4996	-0.098	(-1.33)	
LEV	0.186	0.172	933	0.156	0.120	4996	0.030	(4.61)	***
BM	0.684	0.600	933	0.666	0.527	4996	0.018	(0.84)	
ROA	-0.024	0.037	933	-0.044	0.028	4996	0.020	(2.43)	**
Sigma	0.138	0.122	915	0.147	0.130	4842	-0.009	(-3.33)	***
Skewness	0.142	0.073	915	0.187	0.146	4842	-0.044	(-1.65)	*
Kurtosis	-0.033	-0.307	915	-0.092	-0.384	4842	0.060	(1.42)	
Panel B: Post-STOCK Act Period									
GuidanceCount	4.015	0	921	5.157	0	5224	-1.142	(-3.97)	***
Log(1+GuidanceCount)	0.750	0	921	0.917	0	5224	-0.167	(-3.92)	***
RangeForecast	0.732	0.800	139	0.697	0.830	922	0.036	(1.16)	
IDIOSYN	-1.286	-1.189	884	-1.208	-1.167	5091	-0.078	(-1.76)	*
GPIN	0.454	0.493	532	0.464	0.497	2867	-0.010	(-2.02)	**
DELAY	0.403	0.326	871	0.403	0.327	5114	0.001	(0.07)	
ANALYSTS	5.284	3.000	921	5.162	1.000	5224	0.122	(0.49)	
FCSTERROR	0.147	0.005	584	0.117	0.005	2847	0.030	(0.93)	
DISPERSION	0.055	0.003	550	0.083	0.002	2705	-0.027	(-1.29)	
Gov_Contract_Terms	1.450	0.0	921	0.231	0.0	5224	1.218	(11.62)	***
Regulatory_Policy_Risk	0.152	0.0	921	0.043	0.0	5224	0.109	(6.19)	***
GLS_ICC	0.109	0.087	697	0.101	0.079	3555	0.009	(2.76)	***
OJM_ICC	0.087	0.055	630	0.078	0.047	3063	0.008	(1.50)	
CAT_ICC	-0.029	-0.055	696	-0.083	-0.105	3550	0.054	(6.57)	***
PEG_ICC	0.147	0.098	699	0.146	0.101	3563	0.001	(0.19)	
AVG_ICC	7.666	4.619	823	5.943	2.971	4251	1.723	(3.72)	***
Size	6.627	6.860	921	6.726	6.772	5224	-0.099	(-1.31)	
LEV	0.213	0.182	921	0.187	0.162	5224	0.026	(3.84)	***
BM	0.587	0.514	921	0.543	0.424	5224	0.044	(2.43)	**
ROA	-0.019	0.026	921	-0.047	0.025	5224	0.028	(3.99)	***
Sigma	0.102	0.088	904	0.114	0.095	5133	-0.012	(-5.18)	***
Skewness	0.209	0.155	904	0.198	0.160	5133	0.011	(0.43)	
Kurtosis	-0.091	-0.388	904	-0.096	-0.418	5133	0.005	(0.12)	

Table 3. Private Communication and Management Guidance

This table presents the OLS regression results examining the effect of private communication between politicians and public firms on the frequency of overall management forecasts. The dependent variable, $\text{Log}(1 + \text{GuidanceCount})$, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Columns 1-3) and Poisson (Columns 4-6) frameworks. The key independent variable is the interaction term $\text{Post} \times \text{MajorGovCustomer}$. Post is a dummy variable equal to one for the period after the enactment of the STOCK Act. MajorGovCustomer is a dummy variable equal to one if the government was a major company customer for at least three out of four years from 2008 to 2011. Columns 2 and 4 include firm-year controls such as Size , LEV , BM , and ROA . Columns 1 and (2) include industry fixed effects and time fixed effects. Columns 3 and 4 include firm fixed effects and time fixed effects. The t-statistics, shown in parentheses, are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

	(1)	(2)	(3)	(4)	(5)
	$\text{Log}(1 + \text{GuidanceCount})$			Poisson GuidanceCount	
$\text{Post} * \text{MajorGovCustomer}$	-0.074** (-2.11)	-0.073** (-2.13)	-0.084*** (-2.67)	-0.078** (-2.53)	-0.111** (-2.07)
MajorGovCustomer	-0.052 (-0.57)	-0.002 (-0.03)			
Size		0.159*** (10.42)		0.078*** (5.67)	0.175*** (5.87)
LEV		0.036 (0.26)		0.117** (2.11)	0.253** (2.37)
BM		-0.036 (-1.07)		0.042*** (2.81)	0.161*** (3.85)
ROA		0.309*** (3.87)		-0.037 (-1.07)	0.037 (0.31)
Ind FE	Yes	Yes	No	No	No
Firm FE	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	12,074	12,074	12,074	12,074	12,074
R-squared	0.11	0.18	0.90	0.91	

Table 4. Forecast Precision of Management Forecasts

This table presents the OLS regression results analyzing the precision of management forecasts (point or range forecast). The dependent variable, *RangeForecast*, is defined each year as the percentage of forecasts a company issues that are range forecasts in a year, as opposed to point estimates, reported in decimal form. The independent variable of interest is the interaction term $Post \times MajorGovCustomer$. *Post* is a dummy variable that equals one for the period after the enactment of the STOCK Act. *MajorGovCustomer* equals one if the government was a major customer of the company for at least three out of four years from 2008 to 2011. Columns 2 and 4 include firm-year controls such as *Size*, *LEV*, *BM* and *ROA*. Columns 1 and 2 include industry fixed effects and time fixed effects. Columns 3 and 4 include firm fixed effects and time fixed effects. The t-statistics, shown in parentheses, are clustered at the firm level and adjusted for heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

	(1)	(2)	(3)	(4)
	<i>RangeForecast</i>			
<i>Post</i> × <i>MajorGovCustomer</i>	0.070** (1.97)	0.071** (2.00)	0.061* (1.77)	0.059* (1.71)
<i>MajorGovCustomer</i>	-0.057 (-1.13)	-0.058 (-1.15)		
<i>Size</i>		-0.005 (-0.62)		-0.032 (-1.51)
<i>LEV</i>		-0.075 (-1.04)		0.012 (0.18)
<i>BM</i>		-0.046 (-1.48)		-0.011 (-0.40)
<i>ROA</i>		0.008 (0.13)		-0.032 (-0.46)
Ind FE	Yes	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,535	2,535	2,535	2,535
R-squared	0.19	0.19	0.75	0.75

Table 5. The Importance of Government Sales

This table presents the OLS regression results examining the cross-sectional tests on demand uncertainty. The dependent variable, $\text{Log}(1 + \text{GuidanceCount})$, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Columns 1-3) and Poisson (Columns 4-6) frameworks. *HighGovSalesRatio* ($\text{HighGovCustomNum} / \text{HighGovSalesVol}$) is a dummy variable that equals one when firms have a ratio of government sales to overall sales (the total number of major government customers / the annual volatility of government sales) at or above the high quartile for firms with major government customers from 2008 to 2011; otherwise, it equals zero. *LowGovSalesRatio* ($\text{LowGovCustomNum} / \text{LowGovSalesVol}$) is a dummy variable that equals one when firms have a ratio of government sales to overall sales (the total number of major government customers / the annual volatility of government sales) below the low quartile for firms with major government customers during the same period; otherwise, it equals zero. All regressions include fixed effects and firm-year controls, including *Size*, *LEV*, and *ROA*. The t-statistics, shown in parentheses, are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Log(1 + GuidanceCount)</i>			Poisson <i>GuidanceCount</i>		
<i>Post*HighGovSalesRatio</i>	-0.126*** (-2.90)			-0.187** (-2.52)		
<i>Post*LowGovSalesRatio</i>	-0.028 (-0.68)			-0.035 (-0.50)		
<i>Post*HighGovCustomNum</i>		-0.085** (-2.18)			-0.137** (-2.22)	
<i>Post*LowGovCustomNum</i>		-0.073* (-1.66)			-0.084 (-0.99)	
<i>Post*HighGovSalesVol</i>			-0.112*** (-2.86)			-0.148** (-2.24)
<i>Post*LowGovSalesVol</i>			-0.044 (-0.97)			-0.066 (-0.81)
<i>Size</i>	0.078*** (5.65)	0.078*** (5.67)	0.078*** (5.67)	0.172*** (5.73)	0.175*** (5.88)	0.175*** (5.88)
<i>LEV</i>	0.116** (2.11)	0.117** (2.11)	0.117** (2.12)	0.244** (2.32)	0.250** (2.37)	0.249** (2.34)
<i>BM</i>	0.042*** (2.82)	0.042*** (2.82)	0.042*** (2.83)	0.159*** (3.77)	0.161*** (3.86)	0.160*** (3.84)
<i>ROA</i>	-0.036 (-1.06)	-0.037 (-1.08)	-0.037 (-1.09)	0.037 (0.32)	0.036 (0.30)	0.037 (0.32)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,074	12,074	12,074	12,074	12,074	12,074
R-squared	0.91	0.91	0.91			

Table 6. Direct Political Activity Engagements

This table presents the OLS regression results of the cross-sectional tests on the coordination incentives measured by conference call disclosed political risks and political contribution records. The dependent variable, *Log (1 + GuidanceCount)*, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Panel A) and Poisson (Panel B) frameworks. Columns 1 and 2 present results for firms with federal contributions (*FedContribution*) that are higher and lower than the sample median. Columns 3 and 4 presents results for firms with a number of connected politicians (*ConnectedPolitician*) that are higher and lower than the sample median. The dependent variable, *Frequency*, is defined as the natural logarithm of one plus the total number of management forecasts issued annually by the firm. The key independent variable is the interaction term *Post × MajorGovCustomer*. *Post* is a dummy variable set to one for the period following the enactment of the STOCK Act. *MajorGovCustomer* is set to one if the government was a major customer of the company for at least three out of the four years from 2008 to 2011. All regressions include fixed effects and firm-year controls, including *Size*, *LEV*, *BM*, and *ROA*. The t-statistics, provided in parentheses, are clustered at the firm level and adjusted for heteroscedasticity. Significance levels are denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

	(1)	(2)	(3)	(4)
	<i>Log (1 + GuidanceCount)</i>			
	<i>FedContribution</i>		<i>ConnectedPolitician</i>	
	High	Low	High	Low
<i>Post × MajorGovCustomer</i>	-0.118*** (-2.90)	0.000 (0.00)	-0.070* (-1.78)	-0.049 (-1.04)
Firm-Year Controls	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	6,254	5,820	6,637	5,437
R-squared	0.92	0.86	0.91	0.84
Panel B: Poisson Framework				
	<i>Poisson</i>			
	<i>GuidanceCount</i>			
	<i>FedContribution</i>		<i>ConnectedPolitician</i>	
	High	Low	High	Low
<i>Post*MajorGovCustomer</i>	-0.153*** (-2.70)	0.130 (1.04)	-0.095* (-1.87)	-0.180 (-0.96)
Firm-Year Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	6,254	5,820	6,157	5,917

Table 7. Conference Call Textual Analysis

This table reports the OLS regression results investigating the effect of the interaction between the post-STOCK Act period and major government customers on the content of earnings call transcripts. The dependent variables are *Gov_Contract_Terms* and *Regulatory_Policy_Risk*, which capture the count of words associated with government contract terms and regulatory and policy risk scaled by the total number of words (multiplied by 10,000). The regression includes firm and year fixed effects and controls for firm characteristics such as *Size*, *LEV*, *BM*, and *ROA*. Standard errors are clustered at the firm level, and significance is indicated at the 10%, 5%, and 1% levels. Further details pertaining to all variable definitions are defined in [Table OA.1](#).

	(1) <i>Gov_Contract_Terms</i>	(2) <i>Regulatory_Policy_Risk</i>
<i>Post*MajorGovCustomer</i>	-0.309** (-1.97)	0.070*** (3.03)
<i>Size</i>	0.071 (1.13)	0.010* (1.87)
<i>LEV</i>	0.132 (0.96)	-0.015 (-0.57)
<i>BM</i>	0.094 (1.28)	0.006 (0.74)
<i>ROA</i>	-0.322** (-2.00)	-0.010 (-0.83)
Firm FE	Yes	Yes
Year FE	Yes	Yes
Observations	12,074	12,074
R-squared	0.70	0.37

Table 8. Price Informativeness

This table reports the OLS regression results investigating the effect of the interaction between the post-STOCK Act period and major government customers on the informativeness of equity prices. The dependent variables in Columns 1-4 are *IDIOSYN* and *GPIN*, which capture the alignment of a firm's stock returns with broader market trends, the influence of market returns on the firm's performance, and the intensity of private information arrival based on noise trading, respectively. Columns 5 and 6 examine price delay, *DELAY*, which captures how quickly stock prices incorporate market-wide information. The regressions includes firm and year fixed effects and controls for firm characteristics such as *Size*, *LEV*, *BM*, and *ROA*. In Columns 2, 4, and 6, we include three control variables to account for a stock's skewness (*Skewness*), volatility (*Sigma*), and kurtosis (*Kurtosis*) over a calendar year. Standard errors are clustered at the firm level, and significance is indicated at the 10%, 5%, and 1% levels. All variable definitions are defined in [Table OA.1](#).

	(1) <i>IDIOSYN</i>	(2) <i>IDIOSYN</i>	(3) <i>GPIN</i>	(4) <i>GPIN</i>	(5) <i>DELAY</i>	(6) <i>DELAY</i>
<i>Post × MajorGovCustomer</i>	-0.155** (-2.11)	-0.171** (-2.34)	-0.013** (-2.12)	-0.013** (-2.04)	0.025** (2.13)	0.026** (2.18)
<i>Size</i>	-0.109*** (-2.79)	-0.128*** (-3.31)	0.000 (0.02)	-0.001 (-0.75)	-0.056*** (-10.20)	-0.051*** (-9.25)
<i>LEV</i>	-0.133 (-1.00)	-0.110 (-0.82)	-0.014 (-1.36)	-0.012 (-1.11)	-0.036 (-1.45)	-0.054** (-2.15)
<i>BM</i>	-0.102** (-2.28)	-0.094** (-2.13)	-0.000 (-0.25)	-0.002 (-0.85)	-0.013* (-1.69)	-0.008 (-1.09)
<i>ROA</i>	0.212 (1.20)	0.177 (1.01)	0.003 (0.66)	0.002 (0.48)	-0.057*** (-2.99)	-0.053*** (-2.81)
<i>Sigma</i>		-0.934*** (-3.03)		-0.029* (-1.66)		0.315*** (6.37)
<i>Skewness</i>		0.162*** (6.10)		0.001 (0.39)		0.005 (1.24)
<i>Kurtosis</i>		-0.079*** (-4.30)		0.001 (0.49)		0.011*** (4.83)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,609	11,609	6,466	6,226	11,667	11,667
R-squared	0.25	0.25	0.51	0.51	0.53	0.53

Table 9. Analyst Forecast Properties

This table presents the regression results analyzing the impact of private communication between politicians and public firms on the analysts within an OLS (Columns 1-3) and Poisson (Column) frameworks. *ANALYSTS* is the number of analysts covering a firm. *FCSTERROR* is the earnings per share forecast error, and *DISPERSION* is the standard deviation of analyst earnings per share forecasts. The primary independent variable of interest is the interaction term *Post* $\times$ *MajorGovCustomer*. *Post* is a dummy variable set to one for the period following the enactment of the STOCK Act. *MajorGovCustomer* is a dummy variable that equals one if the government was a major customer of the firm for at least three out of four years from 2008 to 2011. All regressions control for firm fixed effects, year fixed effects, and firm-year characteristics, including *Size*, *LEV*, *BM*, and *ROA*. The t-statistics, provided in parentheses, are clustered at the firm level and adjusted for heteroscedasticity. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variable definitions are defined in [Table OA.1](#).

	(1)	(2)	(3)	(4)
	<i>ANALYSTS</i>	<i>FCSTERROR</i>	<i>DISPERSION</i>	Poisson <i>ANALYSTS</i>
<i>Post</i> $\times$ <i>MajorGovCustomer</i>	-0.527*** (-2.92)	0.069** (1.98)	-0.039* (-1.73)	-0.104*** (-3.01)
<i>Size</i>	0.749*** (8.92)	-0.045 (-1.54)	-0.026** (-2.32)	0.178*** (6.84)
<i>LEV</i>	0.678 (1.61)	0.187 (1.56)	0.011 (0.40)	0.077 (0.73)
<i>BM</i>	0.408*** (4.68)	-0.007 (-0.13)	0.005 (0.21)	0.082** (2.22)
<i>ROA</i>	-0.726*** (-3.80)	-0.100 (-1.07)	-0.077* (-1.81)	-0.176** (-2.28)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,074	6,570	6,219	12,074
R-squared	0.90	0.76	0.81	

Table 10. Impact of the STOCK Act on Implied Cost of Capital

This table presents the OLS regression results examining the effect of the STOCK Act on firms' internal rates of return as reflected in voluntary disclosure changes. The dependent variables are five implied cost of capital (ICC) measures: *GLS_ICC* (Column 1) following Gebhardt, Lee, and Swaminathan (2001), *OJM_ICC* (Column 2) following Ohlson and Juettner-Nauroth (2005), *CAT_ICC* (Column 3) following Claus and Thomas (2001), and *PEG_ICC* (Column 4) following Easton (2004). Column 5 uses *AVG_ICC*, an equally-weighted average of these four measures. The main independent variable is $Post \times MajorGovCustomer \times Log(1 + GuidanceCount)$, where *Post* indicates the period after the STOCK Act, *MajorGovCustomer* is set to one if the government was a major customer from 2008 to 2011, and $Log(1 + GuidanceCount)$ is the log of management forecasts issued. Regressions include controls for *Size*, *LEV*, *BM*, and *ROA*, with fixed effects and firm-clustered t-statistics in parentheses. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Table OA.1.

	(1) <i>GLS_ICC</i>	(2) <i>OJM_ICC</i>	(3) <i>CAT_ICC</i>	(4) <i>PEG_ICC</i>	(5) <i>AVG_ICC</i>
$Post \times MajorGovCustomer \times Log(1 + GuidanceCount)$	-0.009*** (-3.29)	0.002 (0.48)	-0.013** (-2.43)	-0.017*** (-4.17)	-0.011*** (-3.81)
$Post \times MajorGovCustomer$	0.009 (1.30)	-0.005 (-0.46)	0.011 (0.89)	0.023** (2.56)	0.012* (1.79)
$MajorGovCustomer \times Log(1 + GuidanceCount)$	0.008** (2.30)	-0.009 (-1.35)	0.008 (0.87)	0.021*** (2.96)	0.009* (1.74)
$Log(1 + GuidanceCount)$	-0.003* (-1.75)	0.003 (0.78)	-0.004 (-0.92)	-0.010*** (-3.29)	-0.006** (-2.17)
<i>Size</i>	-0.024*** (-10.69)	-0.034*** (-5.02)	-0.078*** (-12.59)	-0.072*** (-16.22)	-0.053*** (-16.08)
<i>LEV</i>	-0.008 (-0.99)	0.013 (0.68)	-0.035 (-1.57)	-0.019 (-1.35)	-0.016 (-1.36)
<i>BM</i>	0.003 (1.17)	-0.006 (-0.62)	0.067*** (7.96)	-0.013** (-2.00)	0.016*** (3.47)
<i>ROA</i>	0.004 (0.54)	0.003 (0.13)	-0.087*** (-4.75)	-0.073*** (-6.80)	-0.054*** (-5.64)
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	8,401	7,048	8,378	8,452	8,458
R-squared	0.64	0.49	0.74	0.76	0.74

Table 11. The STOCK Act, Congressmen, and Corporate Communications

This table presents the OLS regression results of the cross-sectional tests on the coordination benefits measured by market competition on sales characteristics. The dependent variable, $\text{Log}(1 + \text{GuidanceCount})$, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Columns 1-2) and Poisson (Columns 3-4) frameworks. *CongressOwn* is a binary variable set to one if congressmen disclosed the firm's name for equity transactions at least once during the four years from 2008 to 2011. All regressions include fixed effects and firm-year controls, including *Size*, *LEV*, *BM*, and *ROA*. The t-statistics, shown in parentheses, are clustered at the firm level and adjusted for heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

	(1)	(2)	(3)	(4)
	$\text{Log}(1 + \text{GuidanceCount})$		Poisson GuidanceCount	
<i>Post</i> * <i>CongressOwn</i>	-0.043** (-2.00)	-0.041* (-1.78)	-0.089*** (-2.64)	-0.091** (-2.56)
<i>Post</i> * <i>MajorGovCustomer</i> * <i>CongressOwn</i>		-0.020 (-0.33)		0.000 (0.00)
<i>Post</i> * <i>MajorGovCustomer</i>		-0.075** (-2.02)		-0.114* (-1.75)
<i>Size</i>	0.080*** (5.75)	0.079*** (5.70)	0.175*** (5.82)	0.173*** (5.82)
<i>LEV</i>	0.123** (2.23)	0.120** (2.16)	0.270*** (2.58)	0.270** (2.55)
<i>BM</i>	0.044*** (2.92)	0.043*** (2.90)	0.161*** (3.81)	0.163*** (3.90)
<i>ROA</i>	-0.037 (-1.08)	-0.037 (-1.07)	0.041 (0.35)	0.041 (0.35)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,074	12,074	12,074	12,074
R-squared	0.91	0.91		

Appendix

Table A1. Government Contracts

This table presents the regression results from cross-sectional tests on demand uncertainty, assessed through the characteristics of major government customers and contractors. The dependent variable, *Log (1 + GuidanceCount)*, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Panel A) and Poisson (Panel B) frameworks. *MajorGovCustomer* is a dummy variable set to one if the firm has at least one major government customer for at least three out of the four years from 2008 to 2011. *OnlyGovContractor* is a dummy variable set to one for firms holding government contracts but not having major government customers during the same period. Columns 2 and 4 include firm-year controls such as *Size*, *LEV*, *BM*, and *ROA*. Columns 1 and 2 include industry fixed effects and time fixed effects. Columns 3 and 4 include firm fixed effects and time fixed effects. The t-statistics, shown in parentheses, are clustered at the firm level. Significance levels are indicated by ***, **, and * for the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

Panel A: OLS Framework				
	(1)	(2)	(3)	(4)
	<i>Log (1 + GuidanceCount)</i>			
<i>Post*MajorGovCustomer</i>	-0.081** (-2.24)	-0.083** (-2.31)	-0.088*** (-2.72)	-0.084*** (-2.63)
<i>MajorGovCustomer</i>	0.093 (1.01)	0.076 (0.87)		
<i>Post*OnlyGovContractor</i>	-0.003 (-0.11)	-0.023 (-0.82)	-0.015 (-0.57)	-0.020 (-0.80)
<i>OnlyGovContractor</i>	0.496*** (5.81)	0.283*** (3.44)		
Firm-Year Controls	No	Yes	No	Yes
Ind FE	Yes	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,074	12,074	12,074	12,074
R-squared	0.13	0.19	0.90	0.91
Panel B: Poisson Framework				
	Poisson <i>GuidanceCount</i>			
<i>Post*MajorGovCustomer</i>	-0.139** (-2.16)	-0.140** (-2.18)	-0.146** (-2.54)	-0.135** (-2.41)
<i>MajorGovCustomer</i>	0.140 (0.99)	0.132 (0.95)		
<i>Post*OnlyGovContractor</i>	-0.053 (-1.27)	-0.072* (-1.74)	-0.062 (-1.61)	-0.061 (-1.62)
<i>OnlyGovContractor</i>	0.605*** (6.33)	0.298*** (3.25)		
Firm-Year Controls	No	Yes	No	Yes
Ind FE	Yes	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,074	12,074	12,074	12,074

Online Appendix

OA1. Variable Description

Table OA.1. Variable Definition

Variable	Definition	Source
<i>ANALYSTS</i>	Number of analysts providing forecasts	<i>IBES</i>
<i>AVG_ICC</i>	Equally-weighted average of the <i>AGR_ICC</i> , <i>CAT_ICC</i> , <i>GLS_ICC</i> , and <i>PEG_ICC</i> measures of the cost of capital.	<i>Lee, So, and Wang (2021)</i>
<i>BM</i>	Ratio of the book value of equity to the market value of equity.	<i>COMPUSTAT</i>
<i>DISPERSION</i>	Standard deviation of analyst earnings per share forecasts normalized by share price at the end of the previous quarter	<i>IBES</i>
<i>CAT_ICC</i>	Internal rate of return equating a firm's forecasted cash flows to its current market price, following Claus and Thomas (2001) .	<i>Lee, So, and Wang (2021)</i>
<i>ConnectedPolitician</i>	Number of connected politicians.	<i>OpenSecrets</i>
<i>DELAY</i>	A measure of price delay defined as $1 - \frac{R2_RESTRICTED}{R2_FULL}$. It captures the extent to which stock returns adjust slowly to market-wide information. Higher values indicate greater delay.	<i>Hou and Moskowitz (2005)</i>
<i>DISPERSION</i>	Standard deviation of analyst earnings per share forecasts normalized by share price at the end of the previous quarter	<i>IBES</i>
<i>FCSTERROR</i>	Earnings per share forecast error is the absolute value of the difference between the mean analyst earnings per share forecast and the actual earnings per share normalized by price at the end of the previous year, calculated as $\frac{EPS_MEAN_{y-1} - EPS_ACTUAL_y}{Price_{y-1}}$	<i>IBES</i>
<i>FedContribution</i>	Political contribution amount for firms in federal congressional elections.	<i>OpenSecrets</i>
<i>Forecasts</i>	Total number of all types of management forecasts issued annually by the firm.	<i>IBES Guidance</i>
<i>GuidanceCount</i>	Total number of all types of management forecasts issued annually by the firm.	<i>IBES Guidance</i>
<i>GLS_ICC</i>	Internal rate of return equating a firm's forecasted cash flows to its current market price, following Gebhardt, Lee, and Swaminathan (2001) .	<i>Lee, So, and Wang (2021)</i>
<i>GovCustomNum</i>	The total number of major government customers from 2008 to 2011, four years before the enactment of the STOCK Act.	<i>COMPUSTAT Segment</i>
<i>GovSalesRatio</i>	The ratio of government sales to overall sales from 2008 to 2011, four years before the enactment of the STOCK Act.	<i>COMPUSTAT Segment</i>

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Table OA.1. Variable Definition - Continued

<i>GovSalesVol</i>	The annual volatility of government sales from 2008 to 2011, four years before the enactment of the STOCK Act.	<i>COMPUSTAT Segment</i>
<i>HighGovCustomNum</i>	A dummy variable that equals one if the firm's <i>GovCustomNum</i> is at or above the sample median for firms with major government customers; otherwise, it equals zero.	<i>COMPUSTAT Segment</i>
<i>HighGovSalesRatio</i>	A dummy variable that equals one if the firm's <i>GovSalesRatio</i> is at or above the sample median for firms with major government customers; otherwise, it equals zero.	<i>COMPUSTAT Segment</i>
<i>GPIN</i>	Generalized Public Information Number, which estimates the arrival of private information in financial markets by accounting for variations in noise trading and order flow with a continuous model.	<i>Duarte, Hu, and Young (2020)</i>
<i>Gov_Contract_Terms</i>	Count of average occurrences of government contracting-related terms in a firm's conference call transcripts in a given year. This variable is scaled by the total number of words in the conference call and multiplied by 10,000. The dictionary includes: government contract, government procurement, federal contract, public sector contract, state contract, municipal contract, federal funding, government funding, public procurement, grant funding, Department of Defense, DoD, General Services Administration, GSA, GSA Schedule, Federal Acquisition Regulation, Defense Contract Management Agency, DCMA, Small Business Administration, SBA, Department of Homeland Security, DHS, National Institutes of Health, NIH, Department of Energy, DOE, Office of Management and Budget, OMB, procurement decision, government bid, request for proposal, RFP, request for quotation, RFQ, invitation for bid, IFB, federal compliance, procurement integrity, contract oversight, ethics in government contracting, Buy American Act, cooperative agreement, and small business set-aside.	<i>IBES Guidance</i>
<i>HighGovSalesVol</i>	A dummy variable that equals one if the firm's <i>GovSalesVol</i> is at or above the sample median for firms with major government customers; otherwise, it equals zero.	<i>COMPUSTAT Segment</i>
<i>IDIOSYN</i>	Defined as $\ln\left(\frac{1-RSQ}{RSQ}\right)$. Higher values indicate greater firm-specific information in prices (i.e., higher price informativeness).	<i>CRSP</i>
<i>Kurtosis</i>	Kurtosis of monthly returns over the calendar year	<i>CRSP</i>
<i>LEV</i>	Ratio of total debt to total assets.	<i>COMPUSTAT</i>
<i>MajorGovCustomer</i>	An indicator variable that equals one for firms reporting major government customers for at least three out of the four years from 2008 to 2011, prior to the enactment of the STOCK Act; otherwise, it equals zero.	<i>COMPUSTAT Segment</i>

Continued on next page

Table OA.1. Variable Definition - Continued

<i>LowGovCustomNum</i>	A dummy variable that equals one if the firm's <i>GovCustomNum</i> is below the sample median for firms with major government customers; otherwise, it equals zero.	<i>COMPUSTAT Segment</i>
<i>LowGovSalesRatio</i>	A dummy variable that equals one if the firm's <i>GovSalesRatio</i> is below the sample median for firms with major government customers; otherwise, it equals zero.	<i>COMPUSTAT Segment</i>
<i>LowGovSalesVol</i>	A dummy variable that equals one if the firm's <i>GovSalesVol</i> is below the sample median for firms with major government customers; otherwise, it equals zero.	<i>COMPUSTAT Segment</i>
<i>OJM_ICC</i>	Internal rate of return equating a firm's forecasted cash flows to its current market price, following Ohlson and Juettner-Nauroth (2005) .	Lee, So, and Wang (2021)
<i>PEG_ICC</i>	Internal rate of return equating a firm's forecasted cash flows to its current market price, following Easton (2004) .	Lee, So, and Wang (2021)
<i>Post</i>	A dummy variable that equals one for observations from 2012 onward, representing the period after the enactment of the STOCK Act; otherwise, it equals zero.	<i>The STOCK Act</i>
<i>Regulatory_Policy_Risk</i>	A measure of the share of the conversation devoted to risks associated with regulatory and policy topics. This variable is scaled by the total number of words in the conference call and multiplied by 10,000. The dictionary includes: regulatory enforcement, compliance requirement, government oversight, regulatory compliance, legal mandate, federal oversight, agency enforcement, compliance audit, enforcement action, administrative ruling, executive order, legislative proposal, legislation impact, congressional ruling, policy reform, statutory requirement, federal mandate, rulemaking process, legal framework, administrative law, federal budget cuts, public spending cap, fiscal policy shift, government shutdown, deficit reduction plan, appropriations bill, funding allocation, spending authorization, taxation policy, economic stimulus, policy uncertainty, regulatory shift, political risk, legislative uncertainty, rule change, industry regulation shift, legal uncertainty, compliance burden, lobbying restrictions, advocacy risk, STOCK Act, Regulation Fair Disclosure, Foreign Corrupt Practices Act, FCPA, Sarbanes-Oxley Act, SOX, Dodd-Frank Act, Freedom of Information Act, FOIA, Anti-Lobbying Act, Federal Acquisition Regulation, Trade Agreements Act, TAA, Whistleblower Protection Act	<i>Thomson Reuters Firm conference call transcripts</i>
<i>RangeForecast</i>	Average percentage of forecasts a company issues that are range forecasts in a year reported in decimal form. This variable is only computed for firms that issue forecasts.	<i>Thomson Reuters</i>
<i>Size</i>	Natural logarithm of the equity market value.	<i>COMPUSTAT</i>

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Table OA.1. Variable Definition - Continued

<i>R2_FULL</i>	R^2 from a full regression: $r_{i,t} = \alpha_i + \sum_{k=0}^4 \beta_k r_{m,t-k} + \epsilon_{i,t}$, incorporating contemporaneous and four lagged weekly market returns to better capture delayed information incorporation.	<i>Hou and Moskowitz (2005)</i>
<i>R2_RESTRICTED</i>	R^2 from a restricted regression: $r_{i,t} = \alpha_i + \beta r_{m,t} + \epsilon_{i,t}$, where $r_{i,t}$ is the weekly excess return of stock i and $r_{m,t}$ is the contemporaneous weekly market excess return.	<i>Hou and Moskowitz (2005)</i>
<i>ROA</i>	Ratio of income before extraordinary items to total assets.	<i>COMPUSTAT</i>
<i>RSQ</i>	Calculated as the coefficient of determination from a regression of firm excess returns on market and industry excess returns, where the model is defined as $r_{f,t} = \alpha + \beta_1 r_{m,t-1} + \beta_2 r_{m,t} + \beta_3 r_{m,t+1} + \gamma_1 r_{i,t-1} + \gamma_2 r_{i,t} + \gamma_3 r_{i,t+1} + \epsilon_{f,t}$ where r_q , $r_{m,q}$, and $r_{i,q}$ are monthly excess returns of the stock, the market, and the stock's industry during year t .	<i>CRSP</i>
<i>Sigma</i>	Volatility of monthly returns over the calendar year	<i>CRSP</i>
<i>Skewness</i>	Skewness of monthly returns over the calendar year	<i>CRSP</i>

OA2. Data Construction

Table OA.2. Data Construction Process

This table explains the step-by-step data assembly process, detailing how the final dataset is constructed by sequentially applying filters and merges to the initial sample.

Operation	Observations
Initial COMPUSTAT firm observations (2008-2015)	90,615
Keep firms listed in all eight years	52,464
Merge with IBES Guidance and assign zero for non-forecast firms	52,464
Merge with COMPUSTAT SEGMENT and set foreign government as zero for government business dummy	52,464
Merge other variables in all eight years	15,424
Exclude observations with missing values for control variables	12,074

OA3. Text Analysis Methodology

To evaluate how corporate communication about government contracting and regulatory uncertainty changed following the STOCK Act, we conduct a text-based analysis using earnings call transcripts. Our goal is to quantify shifts in firms' narrative disclosure patterns related to procurement access and political risk. We construct two primary measures: *Gov_Contract_Terms* and *Regulatory_Policy_Risk*, following established techniques in the accounting and finance literatures.

We begin by collecting full transcripts of quarterly earnings calls from the IBES Transcript (IBEST) database, covering the 2008–2015 period. Each transcript is parsed and tokenized into individual words. We remove stopwords, punctuation, and numerical artifacts, and standardize all text to lowercase. To ensure comparability across firms and time, we calculate term frequencies relative to total word count in each transcript and multiply by 10,000.

The first measure, *Gov_Contract_Terms*, captures the extent to which executives discuss government contracting and procurement. We construct a dictionary of 42 terms commonly associated with U.S. federal procurement. The dictionary is derived from Federal Acquisition Regulation (FAR) documents and government contracting guidelines, and we augment the list that we derive with ChatGPTs suggestions. We validate term relevance using a manual review of randomly sampled call transcripts. The second measure, *Regulatory_Policy_Risk*, captures firm-level concerns related to evolving legislative or policy uncertainty. This dictionary includes 35 terms linked to political and regulatory change. We cross-validate the list with keyword sets used in prior literature on political uncertainty and regulatory disclosure ([Hassan et al., 2019](#)).

***Gov_Contract_Terms* Dictionary:** government contract, government procurement, federal contract, public sector contract, state contract, municipal contract, federal funding, government funding, public procurement, grant funding, Department of Defense, DoD, General Services Administration, GSA, GSA Schedule, Federal Acquisition Regulation, Defense Contract Management Agency, DCMA, Small Business Administration, SBA, Department of Homeland Security, DHS, National Institutes of Health, NIH, Department of Energy, DOE, Office of Management and Budget, OMB, procurement decision, government bid, request for proposal, RFP, request for quotation, RFQ, invitation for bid, IFB, federal

compliance, procurement integrity, contract oversight, ethics in government contracting, Buy American Act, cooperative agreement, small business set-aside.

Regulatory_Policy_Risk Dictionary: regulatory enforcement, compliance requirement, government oversight, regulatory compliance, legal mandate, federal oversight, agency enforcement, compliance audit, enforcement action, administrative ruling, executive order, legislative proposal, legislation impact, congressional ruling, policy reform, statutory requirement, federal mandate, rulemaking process, legal framework, administrative law, federal budget cuts, public spending cap, fiscal policy shift, government shutdown, deficit reduction plan, appropriations bill, funding allocation, spending authorization, taxation policy, economic stimulus, policy uncertainty, regulatory shift, political risk, legislative uncertainty, rule change, industry regulation shift, legal uncertainty, compliance burden, lobbying restrictions, advocacy risk, STOCK Act, Regulation Fair Disclosure, Foreign Corrupt Practices Act, FCPA, Sarbanes-Oxley Act, SOX, Dodd-Frank Act, Freedom of Information Act, FOIA, Anti-lobbying Act, Federal Acquisition Regulation, Trade Agreements Act, TAA, Whistleblower Protection Act.

OA4. Propensity Score Matching and Entropy Balancing

Our baseline analysis uses all firms that do not declare the government as a major customer. However, there may be concerns that not all of these firms are comparable to our treated sample of firms with major government customers. In this section we examine the robustness of our results to both matching and entropy balancing to mitigate concerns that our results are driven by other observable differences between treatment and control banks.

We reanalyze the results from [Table 3](#) using propensity score matching (PSM), employing nearest-neighbor matching without replacement. To generate propensity scores, we run a logistic regression where the dependent variable is treatment status, and the independent variables include four firm-level controls: log market value, leverage, book-to-market ratio, and return on assets. Each treated firm-year is then matched to the control firm-year with the closest propensity score, ensuring that no control observation is used more than once. This matching procedure yields a final matched sample consisting of 1,854 treated firm-years and 1,854 control firm-years, with no unmatched observations.

Second, we use entropy balancing ([Hainmueller, 2012](#)) to weight sample observations to achieve covariate balance on the same four variables, log market value, leverage, book-to-market ratio, and return on assets. We present the results of our matched sample and entropy balancing analyses in [Table OA.3](#). The results using both methods are consistent with those in the main manuscript. Specifically, the matched and entropy-balanced samples indicate a decrease in management guidance for major government customers post-Act.

Table OA.3. Propensity Score Matching and Entropy Balancing

This table presents the propensity score matched (Panel A) and entropy balanced (Panel B) regression results examining the effect of private communication between politicians and public firms on the frequency of overall management forecasts using nearest neighbor matching. The dependent variable, $\text{Log}(1 + \text{GuidanceCount})$, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Columns 1-4) and Poisson (Column 5) frameworks. The key independent variable is the interaction term $\text{Post} \times \text{MajorGovCustomer}$. Post is a dummy variable equal to one for the period after the enactment of the STOCK Act. MajorGovCustomer is a dummy variable equal to one if the government was a major company customer for at least three out of four years from 2008 to 2011. Columns 2, 4, and 5 include firm-year controls such as Size , LEV , BM , and ROA . Columns 1 and 2 include industry fixed effects and time fixed effects. Columns 3-5 include firm fixed effects and time fixed effects. The t-statistics, shown in parentheses, are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#)

Panel A: Propensity Score Matching					
	(1)	(2)	(3)	(4)	(5)
	$\text{Log}(1 + \text{GuidanceCount})$				Poisson GuidanceCount
<i>Post*MajorGovCustomer</i>	-0.130** (-2.10)	-0.109* (-1.81)	-0.126*** (-2.96)	-0.113*** (-2.67)	-0.150** (-2.12)
<i>MajorGovCustomer</i>	-0.021 (-0.22)	0.004 (0.05)			
Ind FE	Yes	Yes	No	No	No
Firm FE	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	3,708	3,708	3,708	3,708	3,708
R-squared	0.16	0.22	0.92	0.92	
Panel B: Entropy Balancing					
<i>Post*MajorGovCustomer</i>	-0.076** (-2.18)	-0.071** (-2.07)	-0.087*** (-2.76)	-0.079** (-2.56)	-0.113** (-2.15)
<i>MajorGovCustomer</i>	-0.087 (-1.03)	-0.038 (-0.46)			
Ind FE	Yes	Yes	No	No	No
Firm FE	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	12,074	12,074	12,074	12,074	12,074
R-squared	0.15	0.22	0.90	0.90	

OA5. Placebo Tests

In this section, we conduct two placebo tests to assess the validity of our identification strategy and event date. First, to assess whether unobserved confounders could be driving our results, we conduct a placebo test following the approach of [Brogaard, Gerasimova, and Rohrer \(2024\)](#). Specifically, we randomly assign treatment status (*MajorGovCustomer* = 1) to different firms while keeping all other variables unchanged. We repeat this procedure 10,000 times, re-estimating our main regression (Column 4 of Table 3) in each iteration to generate a distribution of placebo coefficients. This procedure simulates a scenario in which the treatment effect is purely random, allowing us to assess whether our observed coefficient could plausibly arise from chance rather than a true causal relationship.

We present our results in [Figure OA.1](#). The resulting distribution is centered at zero, with 99.7% of the simulated coefficients falling between -0.090 and +0.090. In contrast, our actual estimated coefficient of -0.078 falls in the extreme left tail of the distribution ($p < 0.002$). This indicates that the observed reduction in management forecast frequency is highly unlikely to be driven by random chance or omitted variables. These findings provide strong support for the causal interpretation that the STOCK Act's constraints on politically connected firms led to a reduction in their disclosure frequency.

Second, to assess the validity of our event date, we conduct a placebo test using 2006 as our event date, which was the year the STOCK Act was first introduced to Congress but ultimately failed to pass. As discussed in [Section 2](#), the proposal received little public attention and did not meaningfully alter expectations or behavior. If our main results are driven by differential pre-trends or unobservable characteristics of major government contractors, we would expect to see similar disclosure patterns emerge around this earlier, non-event.

We re-estimate our difference-in-differences specification using data spanning 2002-2010 and use 2006 as the event year and report our results for management forecast frequency in [Table OA.4](#). Our results indicate that there are no statistically significant differences in forecast frequency between major government customers and other firms. The absence of an effect reinforces the interpretation that our main results reflect a response to the passage of the 2012 STOCK Act, opposed to other coincidental or pre-existing disclosure trends.

Figure OA.1. Distribution of Placebo Coefficients

This figure presents the distribution of OLS coefficients from 10,000 placebo regressions, following the approach of [Brogaard, Gerasimova, and Rohrer \(2024\)](#). In each iteration, we randomly reassign the treatment status (*MajorGovCustomer* = 1) to a different set of firms while holding all other variables constant. This procedure simulates a scenario in which the treatment effect is purely random, allowing us to assess whether our observed coefficient could plausibly arise from chance rather than a true causal relationship. The resulting distribution is centered at zero, with 99.7% of coefficients falling between -0.090 and +0.090. The actual estimated coefficient (-0.078), marked by the vertical line, is well within the range of the placebo distribution. All variable definitions are defined in [Table OA.1](#)

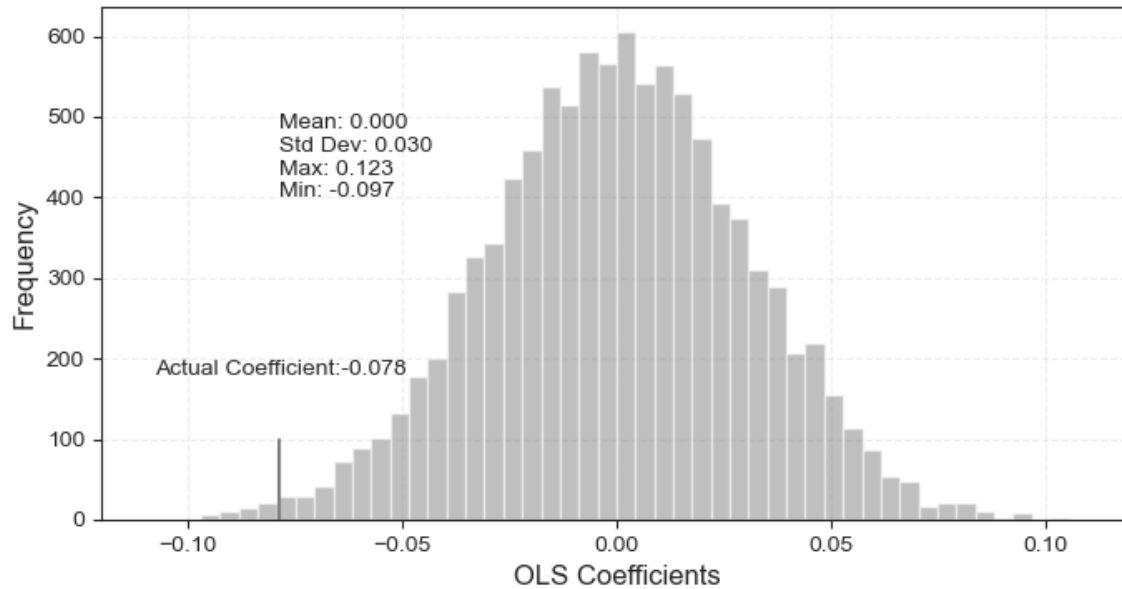


Table OA.4. Placebo Test Using 2006 Pseudo-Treatment Year

This table presents the OLS regression results from a placebo test using 2006 as the pseudo-treatment year. The dependent variable, $\text{Log}(1 + \text{GuidanceCount})$, is the natural logarithm of one plus the total number of management forecasts issued annually by the firm. The key independent variable is $\text{Post2006} \times \text{MajorGovCustomer}$, where Post2006 is a dummy for firm-years after the placebo cutoff and MajorGovCustomer equals one if the government was a major customer in at least three of the four years prior to 2006. Columns 1–4 use OLS, and Column 5 uses Poisson regression. Columns 2 and 4 include controls for Size , LEV , BM , and ROA . Industry and year fixed effects are included as noted. Standard errors are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in [Table OA.1](#).

	(1)	(2)	(3)	(4)	(5) Poisson
	$\text{Log}(1 + \text{GuidanceCount})$				GuidanceCount
$\text{Post2006} \times \text{MajorGovCustomer}$	0.020 (0.44)	0.041 (0.94)	0.012 (0.38)	0.007 (0.21)	0.005 (0.06)
MajorGovCustomer	-0.048 (-1.01)	-0.053 (-1.18)			
Size		0.137*** (18.24)		0.076*** (7.40)	0.278*** (8.60)
LEV		-0.111 (-1.53)		0.078 (1.64)	0.186 (1.46)
BM		-0.001 (-0.10)		0.052*** (4.81)	0.249*** (5.46)
ROA		0.145*** (4.76)		0.030 (1.34)	0.022 (0.25)
Ind FE	Yes	Yes	No	No	No
Firm FE	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	29,142	29,142	29,142	29,142	29,142
R-squared	0.12	0.20	0.83	0.84	

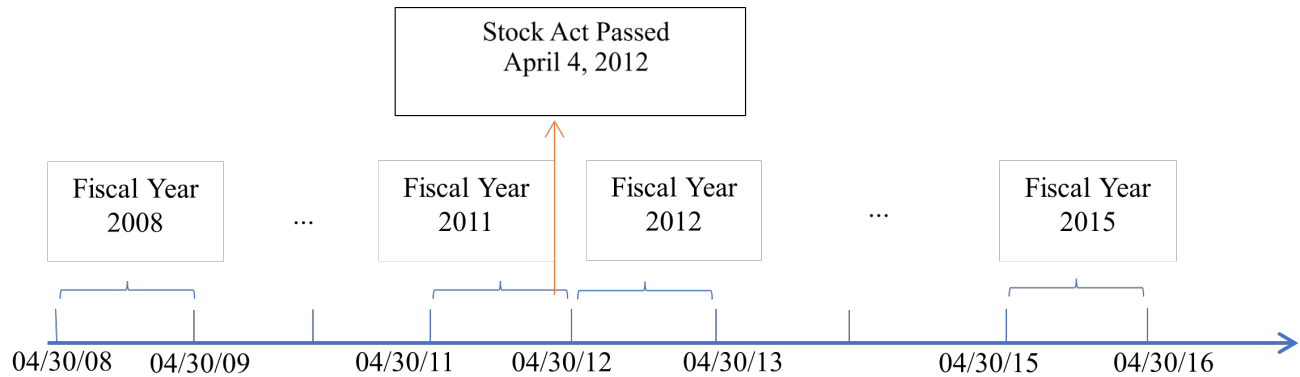
OA6. Alternative Data Sample and Event Dates

Since the STOCK Act's passage date does not align with the end of the fiscal year, we employed two alternative time measures to address this issue. We first applied the commonly adopted fiscal year method, excluding all observations from 2012, the year the STOCK Act was passed, and retaining data for four years before and after. Additionally, we used the same method described in [Section 4.2](#), but replaced April 4, 2012, with April 30, 2012, a date commonly considered the fiscal year-end for many firms. [Figure OA.2](#) Panels A and B illustrate these different time measurements. [Table OA.5](#) Columns 1 and 2 show that both approaches yield consistent results. The outcomes remain negative and significant, confirming the robustness of our initial findings. This consistency indicates that the method of date measurement does not materially affect the observed impact of the STOCK Act on the frequency of management forecasts.

Figure OA.2. Timeline Robustness for Management Forecasts

Panel A illustrates the timeline cut-off robustness for management forecasts, covering eight years from April 4, 2008, to April 4, 2016. The STOCK Act passed during the midpoint of the sample, April 4, 2012. Management forecasts within a defined year are matched to the corresponding fiscal year since the end of April is the deadline for 10-K disclosures for many public firms. Panel B covers the timeline period robustness for management forecasts. The sample period covers eight years in total, using the firm's fiscal year at the annual level to match management forecasts and firm characteristics. All management forecasts predicted in 2012 are excluded to alleviate the potential impact on decision discussion and execution.

Panel A. Timeline Cut-off Robustness for Management Forecasts



Panel B. Timeline Period Robustness for Management Forecasts

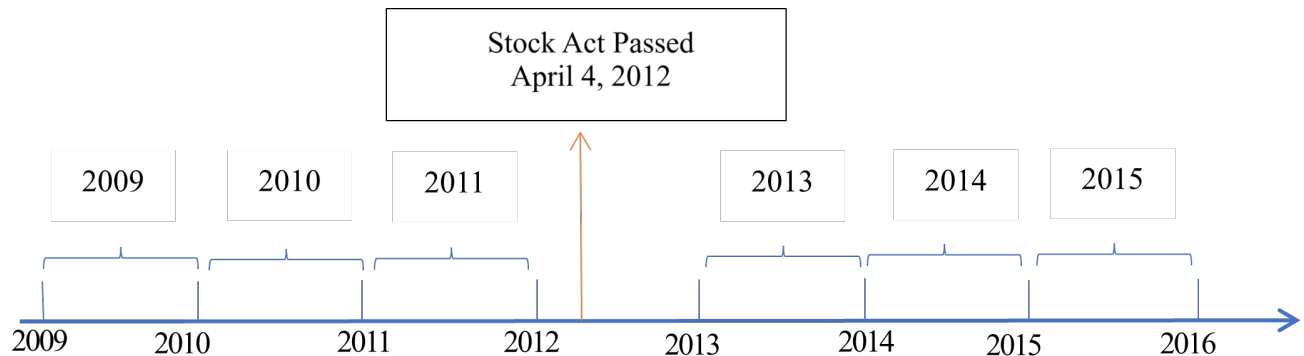


Table OA.5. Time Robustness Tests

This table presents the results of OLS regressions using two different potential time measures for the STOCK Act: excluding the action year (Columns 1 and 3) and using an alternative annual window (Columns 2 and 4). The dependent variable, *Log (1 + GuidanceCount)*, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Columns 1-2) and Poisson (Columns 3-4) frameworks. The independent variable of interest is the interaction term *Post* $\times$ *MajorGovCustomer*. *Post* is a dummy variable that equals one for the period after the enactment of the STOCK Act. *MajorGovCustomer* equals one if the government was a major customer of the company for at least three out of four years from 2008 to 2011. All regressions include fixed effects and firm-year controls, including *Size*, *LEV*, *BM*, and *ROA*. The t-statistics, shown in parentheses, are clustered at the firm level and adjusted for heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Table OA.1.

	(1)	(2)	(3)	(4)
	<i>Log (1 + GuidanceCount)</i>		Poisson <i>GuidanceCount</i>	
	Exclude 2012	Alternative Fiscal Year	Exclude 2012	Alternative Fiscal Year
<i>Post*MajorGovCustomer</i>	-0.098*** (-2.64)	-0.078** (-2.52)	-0.140** (-2.29)	-0.108** (-2.01)
<i>Size</i>	0.064*** (4.53)	0.069*** (5.36)	0.124*** (4.38)	0.140*** (5.30)
<i>LEV</i>	0.074 (1.24)	0.099* (1.78)	0.215* (1.91)	0.204* (1.90)
<i>BM</i>	0.213 (1.33)	0.194 (1.18)	1.381 (1.27)	1.146 (1.48)
<i>ROA</i>	-0.053 (-1.39)	-0.028 (-0.83)	0.025 (0.19)	0.024 (0.20)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	10,932	12,074	10,932	12,074
R-squared	0.89	0.91		

OA7. Robustness of Treatment and Control Group Selection

We conducted additional tests to ensure our findings' robustness and address potential biases in our treatment and control group selection. These tests help verify that specific group characteristics or external factors do not drive the observed effects. In [Table OA.6](#), Column 1, we exclude firms that never issue management forecasts in our primary test sample to avoid potential non-coverage issues. The coefficient on $Post \times MajorGovCustomer$ is -0.157 with a significance level of 0.05. In Column 2, we use control firms without government business for the entire sample period, and our results continue to remain robust.

Table OA.6. Alternative Sample Tests

This table presents the OLS regression results using alternative samples of firms: those making at least one management forecast (Columns 1 and 3), control firms without government business for the entire sample period (Columns 2 and 4). The dependent variable, *Log (1 + GuidanceCount)*, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm, and the analysis is conducted within OLS (Columns 1-2) and Poisson (Columns 3-4) frameworks. The independent variable of interest is the interaction term *Post* $\times$ *MajorGovCustomer*. *Post* is a dummy variable that equals one for the period after the enactment of the STOCK Act. *MajorGovCustomer* equals one if the government was a major customer of the company for at least three out of four years from 2008 to 2011. All regressions include fixed effects and firm-year controls, including *Size*, *LEV*, *BM*, and *ROA*. The t-statistics, shown in parentheses, are clustered at the firm level and adjusted for heteroscedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

	(1)	(2)	(3)	(4)
	<i>Log (1 + GuidanceCount)</i>		Poisson <i>GuidanceCount</i>	
	IBES Covered	Clean Control	IBES Covered	Clean Control
Post*MajorGovCustomer	-0.157** (-2.37)	-0.071** (-2.30)	-0.111** (-2.07)	-0.101* (-1.88)
Size	0.179*** (5.64)	0.083*** (5.74)	0.175*** (5.87)	0.180*** (5.68)
LEV	0.268** (2.40)	0.125** (2.22)	0.253** (2.37)	0.265** (2.45)
BM	0.122*** (3.16)	0.045*** (2.94)	0.161*** (3.85)	0.165*** (3.89)
ROA	-0.049 (-0.58)	-0.044 (-1.25)	0.037 (0.31)	0.008 (0.07)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	6,101	11,682	6,101	11,682
R-squared	0.82	0.91		

OA8. Alternative Disclosures and Non-Financial Reporting

Corporate voluntary disclosure extends beyond financial guidance, encompassing a range of forecast types and non-financial reporting. To better understand the breadth of the STOCK Act's impact, we examine whether its effects on disclosure vary across different forecast categories and whether it extends to non-financial disclosures. This analysis helps determine whether the Act's influence is broad-based or primarily affects financial disclosures that may have been more relevant to private government communication.

Additional Management Forecast Types. We test whether the STOCK Act's impact on voluntary disclosure differs across various types of management forecasts. Specifically, we consider three dimensions: (1) annual versus quarterly forecasts (*AnnualForecasts* and *QuarterlyForecasts*), (2) earnings versus non-earnings forecasts (*EPSForecasts* and *NonEPSForecasts*), and (3) the frequency of forecasting days (*ForecastDays*). To measure forecast frequency, we count the number of days a firm issues at least one forecast in a given year and take the logarithm of that count. For instance, if a company issues sales, CAPEX, and earnings forecasts on the same day, it is recorded as one forecast day rather than three. We present our results in [Table OA.7](#).

The findings reveal that the reduction in forecast frequency is more pronounced for annual forecasts than for quarterly forecasts, with annual forecasts experiencing a significant decline. This suggests that the STOCK Act had a stronger effect on long-term projections, which tend to be more detailed and subject to greater scrutiny.

When comparing earnings versus non-earnings forecasts, we find that non-EPS forecasts exhibit significant reductions, whereas EPS forecasts remain largely unchanged. A possible explanation is that EPS forecasts are more routine and closely watched by investors, making firms hesitant to reduce them due to potential negative market reactions. In contrast, non-EPS forecasts—such as capital expenditure projections—may be more sensitive to changes in government-related information flows, leading firms to scale back these disclosures post-STOCK Act.

The analysis of forecasting days aligns with our baseline results, showing a significant decline in the number of days firms issue forecasts. This suggests that, beyond changes in specific forecast types, firms

are reducing their overall forecasting activity following the STOCK Act.

Taken together, these findings highlight the widespread impact of the STOCK Act on firms' forecasting behavior. The reductions are most evident in forecasts related to long-term performance and non-earnings information, which are more likely to be influenced by government policy and regulatory considerations.

Non-Financial Disclosures. To further investigate whether the STOCK Act's effects extend beyond financial disclosures, we examine its impact on non-financial disclosures. This empirical analysis serves as a form of placebo test, helping us distinguish between financial information that may have been privately exchanged with government officials and broader corporate communications that were unlikely to be relevant to such interactions.

In 2012, investor reliance on ESG information for trading purposes was relatively limited ([Van Duuren, Plantinga, and Scholtens, 2016](#)). Therefore, we expect the STOCK Act to have a minimal effect on non-financial disclosures, as government officials were less likely to rely on such information in their decision-making or trading activities. Ideally, we would directly examine ESG-related forecasts, but given data limitations, we apply the methodologies of [Henry, Jiang, and Rozario \(2024\)](#) and [Zhang \(2021\)](#) to analyze private communications on environmental and social topics using conference call data.

The results, presented in [Table OA.8](#), support this expectation. Columns (1) and (2) show that both environmental and social disclosures are not significantly affected by the STOCK Act within the private communication setting we examine. This finding suggests that, prior to the STOCK Act, firms and government officials primarily exchanged financial rather than non-financial information, potentially for trading purposes.

These additional tests reinforce our central argument: the STOCK Act effectively curtailed the private communication of critical financial information, while non-financial disclosures remained largely unaffected. This distinction further supports the premise that pre-STOCK Act information flow between firms and government officials was driven more by financially relevant insights than by general corporate reporting.

Table OA.7. Different Types of Management Forecasts

This table presents the OLS regression results examining the impact of private communication between politicians and public firms on the frequency of different types of management forecasts. The dependent variables in Columns 1 and 2, *AnnualForecasts* and *QuarterlyForecasts*, represent the natural logarithm of one plus the total number of annual and quarterly management forecasts issued by the firm each year, respectively. Columns 3 and 4 focus on *EPSForecasts* and *NonEPSForecasts*, defined as the natural logarithm of one plus the total number of annual EPS and Non-EPS management forecasts, respectively. Column 5 uses *ForecastDays*, the natural logarithm of one plus the total number of days within a year on which the firm issues at least one management forecast. The key independent variable is the interaction term $Post \times MajorGovCustomer$. *Post* is a dummy variable set to one for the period following the enactment of the STOCK Act. *MajorGovCustomer* is set to one if the government was a major customer of the company for at least three out of the four years from 2008 to 2011. All regressions incorporate fixed effects and firm-year controls, including *Size*, *LEV*, *BM*, and *ROA*. The t-statistics, provided in parentheses, are clustered at the firm level. Significance levels are denoted by ***, **, and * for the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in [Table OA.1](#).

	(1) <i>AnnualForecasts</i>	(2) <i>QuarterlyForecasts</i>	(3) <i>EPSForecasts</i>	(4) <i>NonEPSForecasts</i>	(5) <i>ForecastDays</i>
<i>Post</i> $\times$ <i>MajorGovCustomer</i>	-0.061** (-2.09)	-0.035 (-1.62)	-0.016 (-0.75)	-0.078** (-2.55)	-0.053** (-2.37)
<i>Size</i>	0.086*** (6.40)	0.030*** (3.39)	0.025*** (3.15)	0.075*** (5.65)	0.056*** (5.62)
<i>LEV</i>	0.132** (2.46)	0.075 (1.61)	0.077** (2.02)	0.103* (1.96)	0.072* (1.71)
<i>BM</i>	0.034** (2.57)	0.031*** (2.74)	0.026*** (3.16)	0.040*** (2.83)	0.027** (2.57)
<i>ROA</i>	-0.022 (-0.74)	-0.033 (-1.17)	-0.003 (-0.13)	-0.039 (-1.21)	-0.031 (-1.30)
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	12,074	12,074	12,074	12,074	12,074
R-squared	0.88	0.87	0.90	0.89	0.89

Table OA.8. Non-Financial Voluntary Disclosure

This table presents the OLS regression results analyzing the impact of private communication between politicians and public firms on the frequency of non-financial disclosures. *EnvInfo* refers to voluntary disclosures concerning environmental issues, and *SocInfo* pertains to voluntary disclosures on social issues. The primary independent variable of interest is the interaction term $Post \times MajorGovCustomer$. *Post* is a dummy variable set to one for the period following the enactment of the STOCK Act. *MajorGovCustomer* is a dummy variable that equals one if the government was a major customer of the firm for at least three out of four years from 2008 to 2011. All regressions control for firm fixed effects, year fixed effects, and firm-year characteristics, including *Size*, *LEV*, *BM*, and *ROA*. The t-statistics, provided in parentheses, are clustered at the firm level and adjusted for heteroscedasticity. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variable definitions are defined in [Table OA.1](#).

	(1) <i>EnvInfo</i>	(2) <i>SocInfo</i>
$Post \times MajorGovCustomer$	-0.504 (-1.60)	-0.224 (-0.95)
<i>Size</i>	0.030 (0.33)	0.345*** (3.54)
<i>LEV</i>	0.614 (1.47)	0.731 (1.53)
<i>BM</i>	-0.080 (-0.64)	0.403*** (3.30)
<i>ROA</i>	0.063 (0.25)	-0.515 (-1.53)
Firm FE	YES	YES
Year FE	YES	YES
Observations	12,074	12,074
R-squared	0.85	0.87

OA9. STOCK Act Legislative Timeline and Market Reaction

The Stop Trading on Congressional Knowledge (STOCK) Act was signed into law by President Barack Obama on April 4, 2012. The legislation responded to public outcry following a CBS *60 Minutes* exposé that aired on November 13, 2011, which alleged that some members of Congress were profiting from non-public information gained through their official duties. Prior to the exposé, previous versions of the STOCK Act had repeatedly stalled in Congress, making passage appear unlikely. The media spotlight transformed the bill from a symbolic effort into a legislative priority, prompting swift bipartisan action in early 2012.

The Senate voted to proceed with the bill on January 26, 2012 (96–3), passed the legislation on February 2 (96–3), and the House followed suit with a 417–2 vote on February 9. After reconciliation, the Senate passed the final version on March 22, and the law was signed on April 4. By this point, passage was virtually assured, and markets likely priced in the Act’s consequences before it was signed into law.

The STOCK Act banned members of Congress and other government officials from using non-public information for financial gain and imposed new transparency requirements. We examine whether these restrictions had differential impacts on firms by analyzing cumulative abnormal returns (CARs) around key legislative dates. Specifically, we compare returns for firms with and without significant government customer exposure, using the *MajorGovCustomer* indicator. We present our results in [Table OA.9](#).

Among the five event dates, February 2 stands out as the only day with a statistically significant difference in abnormal returns between firms with and without major government contracts (90 bps). This date corresponds to the Senate’s decisive passage of the STOCK Act, solidifying its momentum toward becoming law. The sharp negative response among government contractors suggests that investors anticipated a disruption to informational advantages or government relationships stemming from the Act’s passage. In contrast, other dates, such as the signing of the Act on April 4, may have already been priced in. The February 2 return differential thus likely captures the first moment when the market internalized the bill’s high probability of passage, making it the most informative event window.

Table OA.9. Three-Day FF-adjusted Cumulative Abnormal Returns Around Key Dates

This table reports three-day cumulative abnormal returns (CARs), estimated using the Fama-French three-factor model [Fama and French \(1993\)](#), around six major legislative milestones associated with the STOCK Act. These dates are: the CBS *60 Minutes* exposé (November 13, 2011), the Senate's motion to proceed (January 26, 2012), Senate passage (February 2, 2012), House passage (February 9, 2012), Senate approval of the reconciled bill (March 22, 2012), and the final signing into law (April 4, 2012). We compare CARs for firms with and without major government customer exposure, as captured by the *MajorGovCustomer* indicator. Statistical significance is denoted at the 10%, 5%, and 1% levels. Variable definitions are provided in [Table OA.1](#).

Event Date	MajorGovCustomer=1	MajorGovCustomer=0	Difference	t-stat
November 13, 2011	-0.0016	-0.0044	0.0028	0.851
January 26, 2012	0.0040	0.0059	-0.0020	-0.643
February 2, 2012	-0.0050	0.0040	-0.0090	-2.613***
February 9, 2012	0.0009	0.0026	-0.0017	-0.560
March 22, 2012	-0.0010	-0.0005	-0.0005	-0.173
April 4, 2012	-0.0058	-0.0041	-0.0017	-0.523